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ARTICLES

OBSERVATION, CONTEXT, AND SEQUENCE IN THE STUDY OF POLITICS

RICHARD F. FENNO, JR.
University of Rochester

Observation is at the heart of political analysis. Observing the behavior of U.S. senators involves watching them in two contexts—at home and in the capital city. It also entails sensitivity to the sequences of events or contexts which impinge upon senatorial behavior. Contexts and sequences of legislative life have not been observed in the rich detail they deserve, because not enough political scientists are presently engaged in observation.

All students of politics are, perforce, students of politicians. Whether we place them near the center or at the periphery of our work, we cannot avoid thinking about those people who, in any society, pursue public careers, make public decisions, and enmesh themselves in public values. There are, of course, many ways to study politicians. I wish to talk about one of them: observation. By observation, I mean following politicians around and talking with them as they go about their work. Because I shall draw upon my experience in observing some United States senators, I shall speak most directly to people with an interest in American legislative politics. But I should like, in the course of my remarks, to engage all those who have an interest in what politicians do and why. Hence the pretentiousness of my title. The main question I wish to pose is, What, if any, value can the close personal observation of politicians

bring to our studies of politics? A secondary question is, Should we, as a discipline, encourage more of it?

The observation of which I shall speak is, for lack of a better term, interactive observation. It is not like looking through a one-way glass at someone on the other side. You watch, you accompany, and you talk with the people you are studying. Much of what you see, therefore, is dictated by what they do and say. If something is important to them, it becomes important to you. Their view of the world is as important as your view of that world. You impose some research questions on them; they impose some research questions on you. That interaction has its costs—most notably in a considerable loss of control over the research process. It also has benefits. It brings you especially close to your data. You watch it being generated and you collect it at the source. It is not received data. Furthermore, these data—the per-

ceptions, the interpretations, and the behavior of working politicians—are data that bring you close to serious political activity. This immediate proximity to data about serious political activity produces sensitivities and perspectives which, I shall argue, do give observation some “value added”—almost certainly for students of American legislative politics, and quite possibly for all political scientists who think about politicians.

We are not talking here about a theory of politics. We are talking about a mode of research. But it is a mode of research which can—potentially at least—inform, enrich, and guide theories of politics. Its potential may be greater for some bodies of theory than for others. The very activity of observing politicians carries with it a bias in favor of individual-level theorizing. It is individuals you are watching and it is generalizations about individuals that become the building blocks of your analysis. My own view begins with the idea that politicians are both goal-seeking and situation-interpreting individuals. It proceeds to the idea that politicians act on the basis of what they want to accomplish in their world, and on the basis of how they interpret what they see in that world. It moves from there to the idea that we can gain valuable knowledge of their actions, perceptions, and interpretations by trying to see their world as they see it. And hence to observation. I shall make a minimum claim for it—that it brings value added to individual-level analysis. But I hope that others might explore its further reaches for possible contributions to more macro-level theorizing.

There are, I shall argue, two large lessons to be drawn from observation. Each flows directly from a basic condition of observation as a research mode. First, in order to observe politicians, you must leave the place where you live and work, where routines and people are familiar, and go to some other place where you

intrude—more or less—upon the lives, the work, and the routines of less familiar people. In order to observe politicians, you must operate in an unfamiliar context. And that basic condition drives you, inevitably, to a sharpened appreciation of context as a variable in your analysis.

The relevance of context becomes increasingly evident as you move from the observation of one politician to the observation of another. You are driven to ask, in each new encounter, Who is this person I’m with? What is he or she trying to accomplish? What is the situation in which he finds himself? How do he and his situation differ from that of other people I have observed? The object of your attention is “this person.” But unless you are engaged in the study of individual psyches—as most of us are not—the unit of analysis is always “this person in this situation.” You face an individual who is pursuing certain goals, holding certain personal attitudes and values, carrying a residue of personal experience. But you also face an individual who is perceiving, interpreting, and acting in a complex set of circumstances. And you cannot know what you want to know about that individual until you have knowledge of these “circumstances,” or this “situation,” or, this “context.” By knowledge, I mean to include what you learn by looking at the context yourself and what you learn by seeing the context through the eyes of the individual politician. Observation involves interviewing; but it involves much more than interviewing.

There are two master contexts in which all legislative politicians work—home and the capital city. They can be observed in both places. I began by observing some United States senators at home. That experience conveys, overwhelmingly, the importance of context.

During my first two days with a senator from a large state, he held consecutive meetings with a conference of black ministers, a conservative citizens’ group,

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representatives of two dozen rational PACs, officials of the statewide Catholic Conference, a group of Jewish community leaders, the political operatives of the State Education Association, and officers of the Teamsters Union. Three weeks later, I spent my first full day with a small-state senator, driving across vast farmlands for a series of leisurely visits with groups of citizens in three small-town cafes. I saw the meetings in the large state as impassioned importunings, with various groups interviewing a prospective advocate for their cause. I saw the meetings in the small state as easygoing exchanges, with each group reassuring itself that the visitor was "one of us."

The contextual contrast I saw fit with each senator's own perceptions and interpretations. The large-state senator sees himself working in a contentious context, characterized by a great diversity of organized and insistent groups. He calls his state "a microcosm of the United States." Cultivating it, he says, presents "the same problem that a lawyer with a general practice has, of handling many clients without a specialty. It's the problem of dealing with hundreds, even thousands of interests." The small-state senator sees himself as working in a context dominated by rural-agricultural interests. He cannot escape "a specialty." "In [my state]," he says, "we either farm or we farm the farmers." And he sees no microcosm. He speaks, instead, about the uniqueness of his state, historically, culturally, and environmentally. As for cultivating it, he says, "When they accept you as family, it's easy." To observe senators at home is to become acutely aware that the observation of politicians does indeed, involve "this person in this context." The constituency context, is of course, only an example.

The second large lesson of observation also derives from a basic condition of the research mode. Because you are an outsider in every encounter, and because of

the accompanying problems of access, observation is always episodic, never continuous. Your research is, by nature, drop-in-drop-out-drop-in again research. Your observations get made at irregular intervals and at numerous points in time. You are driven by that condition to an extra appreciation of time. You see not only "this person in this context," but "this person at this time." Attention to time leads, in turn, to an emphasis on the changes that take place over time. Contexts change. And observations made "at this time" will likely differ from those made "last time" or "next time." Finally, any set of serial observations produces a special sensitivity to the sequential aspects of change. Your observation "this time" is embedded in a sequence of "other times." Much of what is important about time and change can be captured in the study of sequences. Some observed sequences exhibit regularities; they seem to have a causal logic to them. They are the most easily and advantageously studied. But random sequences can be observed and studied, too. In sum, the observation of politicians brings with it a sharpened sensitivity to *sequence* as a variable in political analysis.

Observation in the constituency also drives home the importance of sequence. My first encounter with each senator took place in the middle of an election campaign. To observe a campaign is to observe constant change. Contributions, poll results, organizational arrangements, staff, schedules, opposition activities, expectations, media treatment, the candidate's mind—all these ingredients change. The campaign you see in January is not the same campaign you see in the summer, or after Labor Day, or on November 1. Indeed, the campaign you see on the day you arrive is often not the campaign you see on the day you leave. And that is exactly the way the campaigners themselves perceive it. They talk to you about the campaign in the language of time, and

of change and of sequence. They speak of stages and phases, of rhythms and flows, of plans and turning points, of gains and losses, of momentum and pace, of beginning games and end games. Campaigns, for all their improvisation, exhibit a good deal of sequential regularity. At the internal planning levels, there is a sequence involving anticipation, organization, and implementation. At the external, public reaction level there is a sequence involving credibility, expectations, and momentum. These sequences can be studied; and so can the random shocks that often alter them. Again, the campaign sequence is only a convenient example.

If, then, the activity of observing politicians has any "value added," it begins with the special sensitivity observation brings to context and to sequence. Politics is a contextual activity and a sequential activity. Students of politics must be students of context and of sequence. Students of American legislative politics need no reminders of the importance of context or sequence—much less of the importance of constituencies and campaigns. To some degree, then, the large lessons to be drawn from observation will simply reinforce ongoing research, and my remarks will add to the chorus. That is fine. For I do not wish to add to the number of variables we treat; I only wish to argue for the richest possible understanding of some we already recognize as central. Here I would like to push a little. For it does not follow from the recognition that context and sequence are important that they will be given due weight in our research. And it is precisely in the matter of due weight that the observational perspective can be most helpful.

Consider the constituency as context. Its importance for legislative politics is taken to be fundamental and indisputable. One would be hard-put to find a study in the entire field that does not include some mention of the constituency.

Yet one would be equally hard-put to find a single constituency anywhere in the United States whose complexity in terms of "this politician in this context" and "at this time" has been analyzed by a political scientist. We love our constituencies, but we do not study them—not up close, in detail, and over time. Can we be satisfied that we know enough about the process by which politicians get recruited and then accumulate (or dissipate) name recognition, reputation, and trust, bit by bit, in multiple intraconstituency contexts over time, when we have yet to study that process for any legislator?

The same can be said for campaigns as sequence. We generalize about them often. But one would be hard-put to find any legislator's campaign for which its yearly, monthly, weekly, even daily sequences have been analyzed up close, in detail, and over time. Can we be satisfied that we know enough about the ways in which strategic options open up and close down over the course of the campaign, or about the kinds of choices that lead a campaign down one path rather than another, or about the choice points at which such branching decisions get made, when we have yet to follow a single campaign from start to finish? With so little close, detailed investigation of so common a context as the constituency and so common a sequence as a campaign, it seems unlikely that we have yet given these variables the close, hard study they deserve.

From an observational perspective, then, there remains plenty of room for some finely grained, finely calibrated studies of context and of sequence. The observational mode is well suited to such studies. And, indeed, that is the direction in which the close personal observation of politicians will inevitably take us. It is a direction in which we could usefully go. In generalizing about the actions and interpretations of our politicians, we must specify, with the greatest care, the condi-

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tions under which each generalization holds. Context and sequence are two such conditions. Perhaps we can draw precise specifications of these conditions without undertaking fairly microscopic analyses of them. But I doubt it.

I would extend the argument to the other master context in which all legislative politicians work. That is the capital city, where the central activity is governing. Students of legislative politics have given a great deal of attention to the activity of governing and to its sequential patterns, fixed as they are by the formal rules of the legislative process. Here, in our case studies of "how a bill becomes a law," can be found the most numerous examples of scholarly observation. And they have been of enormous value to us. But I should like to push a little here, too. A careful detailing of the perceptions, interpretations, and actions of legislative politicians, developed from the closest possible vantage point, and undertaken with a special sensitivity to context and sequence, can yet improve our understanding of what politicians do in the legislature and why. I shall try to support this argument for "value added" with an extended example.

Ronald Reagan's first foreign policy dispute with the Senate arose over his proposed sale of AWACS planes to Saudi Arabia in the fall of 1981. As governing sequences go, the AWACS controversy was well bounded. By law, the arms sale proposal—the largest in U.S. history—had to be submitted to Congress, where it became subject to a congressional veto if majorities of both houses voted, within 30 days, to disapprove it. The president formally submitted it on October 1 and it was voted on—up or down—by the Senate on October 28. As governing sequences go, also, the AWACS controversy was well publicized. Media interest grew exponentially as the sequence came to its end in the Senate. The House had already voted overwhelmingly to dis-

approve the sale; the president's prestige and power were thought to be hanging in the balance; the outcome was unpredictable almost until the vote itself. When the vote occurred, every senator was in his or her seat, and every senator voted. It was, thus, a sequence that was easy to follow; and it was a vote about which every senator would have something to say.

Since each of my senators was involved—if only to cast a vote—I talked with them and their staffs about what they saw, what they did, and why. If we think about them in terms of context and sequence, they cluster in three groupings, each group deciding in a different context and at a different point in time. I call them the early deciders, the active players, and the late deciders. I shall discuss two senators from each grouping. And, with deep apologies (to you not them) I shall designate them by letter.

Two early deciders, both Democrats, were opposed to the sale. They saw themselves, quite simply, as supporters of Israel and of its position. Accordingly, both committed themselves to that position months before the vote took place. "Basically," said the foreign policy staffer for Senator A, "we were going along with the Israeli position from the beginning. . . . We signed everything that came along. . . . Whatever the argument was, we signed on." Because he was a member of the committee of jurisdiction, Senator A was attentive to the issue from its inception. As soon as the administration floated the idea in February, he joined seven committee colleagues in signing a letter to the president expressing "serious" and "deep" concern about the idea and asking the administration to consult with the committee before taking action. In June he signed a letter circulated by Senator Robert Packwood asking the president not to submit the sale to Congress. And in September he signed Senator Packwood's official resolution of

disapproval, on which the eventual vote would be taken.

Viewed from a distance and in the abstract, Senator A expressed an early, strong policy preference and he voted it. Nothing about his behavior seems problematical. But up close and in context there are complexities of choice to be observed. Despite his normal activism on foreign policy issues, he made a deliberate decision not to extend his influence beyond the acts of signing and voting in this case. "It was not something we focused on," said his foreign policy aide, "We weren't involved. . . . We never took an active part in it. The most active we ever got was the senator's statement in committee when the vote came up there. . . . I wouldn't rank it as one of the biggies we've handled." Senator A did not focus on AWACS because he was preoccupied with other matters at that time. And we are reminded that a strong policy preference does not translate automatically into an equal measure of attention or of involvement.

The timing of Senator A's signature on the formal resolution of disapproval necessitated yet another decision. "We were undecided about signing," explained his staffer, "till late in the summer. Packwood was looking for sponsors even before the hearings were held in committee. [The committee chairman] wanted the members to hold off until they heard the arguments in the committee. We did—until Packwood had gotten into the 40s somewhere, looking for the magic 50 signatures. We didn't want to get in too late, since we had no doubt we'd get in eventually. I remember we discussed it riding in the car. The Senator said 'I don't want to be a Johnny-come-lately.' So we jumped in." And Senator A became number 47 out of the eventual 50 cosponsors. The content of his decision was never in doubt. But the timing of his decision was calculated so as to make certain he would receive full credit for that deci-

sion. He believed he would have lost credit with other supporters of Israel if a 50-bar pro-Israel train had left the station without him. Whatever the strength or the content of his policy preferences, therefore, he still had to decide how and when those preferences should be publicly expressed. Decisions about how to vote are separable from decisions about attentiveness, decisions about involvement, and decisions about timing. We have devoted far more energy researching the first kind of decision than the other three. Observation could stimulate a useful corrective.

Senator B also committed early and did not change. Like Senator A, he contributed no more to the process or the outcome than to sign and to vote. Senator B became attentive to the AWACS issue not because of his committee, but because of an early spring trip to the Middle East. "Not long after my trip," he said, "someone at home asked me what I thought about AWACS. I said I was against it." Like Senator A, he interpreted the issue as one of support for Israel; and he made his decision in those terms. In his post-trip interview in April with his state's largest paper he said, "This vote could be the most important vote this session, next to the economy. It's a symbolic vote. Where do you stand, with the Arabs or the Jews?"

He was quick to sign both the June Packwood letter and the September Packwood resolution of disapproval. "I was one of the early signers of the Packwood letter," he explained. "I thought that since I had declared a position on the issue, I would become very conspicuous if I did not become a part of the Packwood group." It was the fact of his early commitment more than his policy preference that motivated these actions. And it continued to do so. "I can't say I didn't have second thoughts about it. . . . I never had that anti-Saudi feeling that Packwood had. I'm not sure why I committed so

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early. But once I did, I felt I should stay to the end." Near the end, the president invited Senator B to the White House for a chat. He went. "They must have thought they saw a soft underbelly," he said, "that they would say, 'Senator, you've just got to support the president.' One of the deficits a politician can get is to be known as a flip-flopper. So I said to myself, You'll just have to ride this one out." Which he did. "I told the president that I would shed no tears if he won, but that I could not change."

Once again, at a distance and in the abstract, Senator B's behavior is quite straightforward. He expressed a policy preference early and strongly, and he voted it. But observation up close and in sequence reveals something more complex. The strength of his commitment bore little relationship to the strength of his policy preference. And his vote can be explained more by the timing of his commitment than by the substance of it. He committed early; and that act constrained every subsequent interpretation and action. He deemed constancy of commitment to be crucial to his success as a politician. And he interpreted the two Packwood initiatives and the presidential chat as situations in which that connection was being tested. The cases of Senators A and B suggest that the more carefully we study sequence, the more carefully we shall have to study timing. If we are to explain outcomes, who decides when may be as important to know as who decides what. We have devoted more energy to studying policy positioning in space than to studying policy sequencing in time. To our rich comprehension of the politics of left, right, and center, we can usefully add an equally rich comprehension of the politics of early, later, and late. Here, too, observation can stimulate a useful corrective.

The cases of A and B suggest, also, that we must think of any politician's goals as mixed, and subject to change. As contexts

change and as sequences unfold, the relative importance of goals may change. Senator A's policy goal came to be tempered by that of political success. And Senator B's policy goal gradually gave way to one of political success. Both senators were cultivating professional reputations at home and in Washington. Each acted so as to protect that reputation—A so as not to be known as a "Johnny-come-lately," B so as not to be known as a "flip-flopper." These interpretations and actions suggest the supreme importance politicians attach to their reputations. They suggest, too, that the cultivation and protection of favorable reputations involve favorable balances of credit and blame. They suggest, finally, that timing, within a sequence of activity, can be a key determinant of credit and blame. The pursuit of favorable reputations and favorable credit/blame balances can usually be studied by observation.

Senators C and D, two active players, interpreted and acted very differently from Senators A and B. First, they deliberately decided to exert an influence on the outcome beyond their signatures and votes. Second, while both thought of themselves as staunch supporters of Israel, neither interpreted the issue in simple "for or against Israel" terms. Third, because they wanted to maintain negotiating flexibility, they did not commit themselves until much later than A or B. Fourth, by postponing their commitments, they became active in an altered context. In the spring there was much position taking, as with A and B, but no negotiating. In the late summer and early fall, Washington wisdom held that the Senate would disapprove the sale; and that expectation created a context in which the administration had incentives to negotiate—which they did, first with Senator C, later with Senator D.

Senator C, a Democrat, focused on AWACS because, as a member of the committee of jurisdiction, he had long

been involved in the problem of arms sales. He possessed a widely acknowledged expertise and commanded much collegial respect in that field. He had developed strong policy preferences. During most of his involvement, these policy goals guided his actions. His pivotal concern was that the AWACS technology not fall into unfriendly hands. His solution was to have the planes flown by joint U.S.-Saudi Arabian crews, and he worked to negotiate a change along these lines prior to the administration's formal submittal. His proposals attracted the support of other senators.

He described his influence in the language of sequence:

I was involved in it all the way through, but my maximum involvement came early. I was light years ahead of anyone in the Senate when AWACS came up. . . . From the beginning, I understood what the problem was and I had strong views about it. I believed that wherever that plane went—that very sophisticated piece of technology—our people should go too. I'd say 25 to 28 senators came to me early on to ask for my views. I sat down with each of them for half an hour and talked to them in detail. At this time, I think I had quite a bit of influence on a lot of people's thinking.

His influence came from his ability to redefine the issue and, for a time, to persuade others to focus on his definition of the issue—in this case, the willingness of the Saudis to accept joint Saudi-U.S. crews on the planes. In mid-September Senator C participated, for a few days, in intensive conversations with Senate leaders, with administration officials and Saudi representatives. At one point he thought he had brokered, at a meeting in his office, an agreement on joint crewing. But the Saudi government said it would not accept any such arrangement; the administration decided not to push the Saudis, and two days later the proposed sale was formally submitted to the Senate unchanged. "We had an agreement we thought would save the world," said Senator C, "but it turned to worms over-

night." And the context of negotiation changed accordingly.

Senator C quickly broke off his negotiating relationship with the White House—largely on the grounds that the Saudi-U.S. actions had rendered his policy goals unattainable. The administration asked him to continue negotiating. But with the demise of his most preferred policy, another personal goal entered his calculation. Senator C wanted to be president of the United States. He feared that any future negotiations would be conducted on the administration's terms, and that he might be "set up" by them to get none of the credit and much of the blame if the sale went through—blame from American Jews that no Democratic hopeful could withstand. As in the earlier cases, changing contexts changed the mix of goals; and strategic calculations were heavily weighted with expectations concerning credit and blame.

When Senator C broke off negotiations, other negotiators entered the picture. Senator D was one of them. And he saw the world very differently from Senator C. Senator D was a Republican. He was a first-year senator. He had no recognized expertise. He had never visited the Middle East. He was not a member of the committee of jurisdiction. He did not have strong policy preferences. But he had a high priority goal. "I want to be known as an effective senator," he said. To that end, he wanted to start, as he put it, "getting involved in the issues, taking some initiatives, doing the work and getting things accomplished." And he had a lot of energy. AWACS became his proving ground. "[It] took more time than anything else I did this year," he said later. He focused on AWACS partly because "I've always been interested in foreign policy," but mostly because "I like the president and I want him to succeed. This is the president's first foreign policy vote. We can't want him to lose it." A Reagan loyalist, with no presidential

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ambitions of his own, he was not constrained to negotiate at arm's length like Senator C. And he plunged in.

"When I first read about AWACS," he said, "I was lukewarm toward it. . . . But I sat here thinking. There must be some way to get this through. . . . I even went so far as to call my friend George Will, and I asked him, 'George, how do we get the president out of this mess?' " The question at issue was being reinterpreted for yet a third time—from "Are you for or against Israel?" to "How can we get the Saudis to make concessions?" to "How do we get the president out of this mess?" For my senators, at least, this sequence of interpretations ran parallel to a sequence of action.

Senator D went to majority leader Howard Baker and offered to sound out the uncommitted freshman Republicans on Senator C's joint crewing idea. Senator D himself favored some variant on that idea, and he became Baker's liaison with half a dozen freshmen. When the joint crewing notion "turned to worms," Senator D and his first-year colleagues put forward the idea of an informal letter from the president promising adequate safeguards for the technology—a so-called "letter of certification." Senator D drafted that letter, and others in his group worked it over. Five days after the president sent up his proposal, the freshman group met with James Baker in Senator D's office. "We told Baker," he said, "that this was what we needed if we were to go along, that otherwise we would vote against the sale." The negotiation produced "95% agreement" on the letter. And the letter, said Senator D, "became the mechanism for winning over doubting senators. Each one would read it, change it a little bit, and put his Hancock on it somewhere." The letter, as signed by the president, enabled Senator D to make a firm commitment and go home to explain it in mid-October. He thinks the letter provided a necessary rationale for three

others in his group. So, Senator D got involved and had an impact on the outcome. It was not large, but it was timely and tangible—and it moved him a measurable distance toward his personal reputational goal.

Legislative outcomes, we know, are invariably negotiated outcomes. This brief and episodic observation of a negotiating sequence suggests that negotiations are, like campaigns, a constantly moving stream. The context in which Senator C's early negotiating efforts took place was not the same context in which Senator D's later negotiating efforts took place. With the passage of time comes contextual change. And with contextual change come new negotiators pursuing different goals, redefining the issue at hand, altering the decision context for others, and, altogether, shaping new coalitional possibilities. For purposes of coalition building, the United States Senate of late September was simply not the same United States Senate that existed in mid-October. That sense of constant motion ought to superintend all our studies of governing. It is a sense that can be sharpened and shaped by observation.

Also the goals of Senators C and D remind us that for every politician, all governing sequences, like all campaign sequences, are embedded within a yet more encompassing sequence. That is the individual's career sequence. Whenever you observe a politician, he or she is at some stage in a career that stretches back in time and reaches forward in time. His behavior can be interpreted from the perspective of the career path that brought him to where he is, and of the career path he expects will take him where he wants to go. In the case of C and D, their long-run aspirations help us understand their short-run interpretations and actions on AWACS: Senator C's desire to be president; Senator D's desire to be an effective senator. Career aspirations, career building, and career sequences are highlighted

by observation and can usefully be studied by observation.

Senators E and F, two late deciders, were among the very last members to make a final decision. Senator E decided the night before the vote. Senator F decided the afternoon of the vote. Like the two early deciders, they had no extra involvement in the process; like the early deciders, they signed both Packwood initiatives. But unlike the early deciders, E and F changed their position and voted for the sale. They did so in yet another decision context at yet another stage in the decision-making sequence. In late October, President Reagan and the presidentially oriented media entered the picture, and together they reinterpreted the issue at hand. An issue revolving about the various conditions of the sale gave way to one concerning the president's ability to conduct foreign policy. As Senator B, reflecting on his presidential chat, put it, "The substantive issue was lost sight of. The question became, 'Are you for or against the President?'" In this latest of interpretive contexts, Republican Senators E and F reconsidered their earlier decision.

For Senator E, it was the media—aided by the president—which refocused his attention on AWACS. "The whole issue changed in the last week or 10 days," he said. "The media began to play it up as a question involving the president's ability to conduct foreign policy. . . . The media hyped the deal way out of proportion to what it really deserved. But the media hype made you reevaluate your vote. Had it not been for the media hype, the issue would have been strictly the arms sales. And I would have been very comfortable voting against AWACS." His policy preference never changed; it simply became irrelevant to a vote cast on institutional grounds. "It was," he said, "something I felt changing gradually. No one thing brought me to my conclusion." Not even a last minute chat with the president. "I

had a very pleasant conversation with the president. . . . At the end I said, 'Mr. President . . . I wish I could say I'm with you, but I can't. All I can say is, I'll keep thinking about it.'" The night before the vote he had a two-hour talk with "the political advisor in the family"—his wife. "We made a checklist—zip, zip, zip on one side, zip, zip, zip on the other side. She asked me 'Well, what do you think it adds up to?' And I said, 'It looks like I ought to support the president.'" And he went to the phone to tell Senator Packwood that he could no longer support the disapproval resolution.

Senator E's early commitment was not controlling for him, as it was for Senator B, because, unlike Senator B, he did not believe that his reputation was at stake. With his Senate colleagues, Senator E's reputation was undamaged because his change of position moved him back under the protective cover of his party's majority and of his party's leader. With his constituents, Senator E simply did not believe that his reputation depended on constancy of commitment. "The state media were very good to me," he said afterward. "With them I have established a reputation as someone who is independent, who does as he goddamned pleases, who can't be pushed around. That helps. It gives you room to cast a vote like this without people saying you've been had. It's not easy to climb down from your position and take another one in the full glare of national publicity. But the state media treated me like I knew what I was doing." While all politicians act so as to cultivate and protect their professional reputations, how they go about it depends not only on the reputation they want, but on what they think their reputation already is. That crucial matter of perception and interpretation can be studied by observation.

There are nearly as many reasons why senators pay attention to a problem as there are senators. It was committee

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membership for A, a trip for B, expertise for C, a desire to help the president for D, the media for E. We need to know what these stimuli are if we are to understand the subsequent involvement, interpretation, and action of each individual politician. For Senator F, conversations with Israeli friends triggered his reevaluation. He thought long and hard about the problem, and the immense pressures he felt on the day of the debate were self-generated.

He had a strong attachment to Israel; but he gradually became convinced that Israel might be just as well off with the sale as without it. "The more I looked at it," he said, "the more I began to worry about what the situation would look like if the deal did *not* go through—the Saudis with their hurt pride, the British in there selling the Saudis the same weapons, Israel asking us for arms to control the Saudis, Israel blamed for the defeat of the sale, and a president who had his wings clipped in his first foreign policy effort. As bad as the sale was for Israel, I began to think the defeat of it might be worse." He found that most of his Israeli friends agreed with him. He did not reinterpret the question in institutional, "for or against the president" terms as E had done. Senator F's preferred policy changed because he took in more information relevant to his original question—the effect on Israel. His preference changed because his information had changed. Either way, through the imposition of a new issue or through the addition of information on the old issue, preferences can change as the governing sequence unfolds. Thus we are reminded that the sequence itself has an autonomous effect on the politicians involved in it. And it must be studied as such. Observation at close range is well suited to studying this impact of processes on outcomes.

On the eve of the vote, Senator F was leaning but undecided. "I stayed up all night before writing a pro-AWACS

speech. But I still was not certain how I was going to vote. . . . Up until 10 minutes before I gave my speech I honestly wasn't certain what I was going to do." His course of action was finally fixed by a confluence of context and of sequence. He found himself in a situation in which he could cast the deciding vote for the sale, and he consciously did so. "A little after noon," he said, "I went to Howard Baker and I asked him what his count was. He said, 'Forty-nine.' I asked, 'What about X?' He said, 'He won't talk to anyone.' I asked, 'How about Y?' He said, 'We can't get to him.'" "I was the only one they could count on," explained F. And he added, "If the sale was going through, and if I could be the fiftieth vote, then I could exert more leverage with the administration and be in a better position to help Israel. . . . Conversely, I didn't want some oil state senator to be the fiftieth vote." The sale went through, 52 to 48.

On his credit/blame balance sheet, Senator F got the worst of both worlds. Supporters of Israel assumed his was the decisive vote; and they blamed him for their loss. "I got more flak from that vote by far than any I have ever cast in the Senate," he said later. And supporters of AWACS gave him no credit for their victory. Three years afterward he said, "Do you know, the administration never thanked me for helping them on that vote, never gave me anything, never acknowledged that I helped them." So much for our abstract generalizations about the special benefits accruing to the latest deciding, pivotal voters. In the concrete, it seems, there are interpretive contingencies to be reckoned with. You are pivotal only if the distributors of credit and blame think you are, and politicians understand that condition. Thus, the allocation, as well as the creation, of credit and blame should be studied—and they can both be studied by observation.

It should not be thought that I have told the story of AWACS. Far from it. At best

I have told six little stories to feed into the larger story of AWACS. But the six stories suggest how complex *the* story of any large legislative decision must be, and how many different decision contexts and decision sequences are involved. They further suggest how much research room yet remains for the microscopic, observation-based analyses of the governing activity of legislative politicians. If, that is, such analyses have value. I have tried to make the case, by example, that they do.

The stories of decision making by six senators have been told to suggest that observation can be an aid to discovery, description, and theorizing. They have been told to further suggest that this "value added" flows from a heightened sensitivity to context and to sequence. In these briefest of discussions—about such analytic foci as attentiveness, involvement, and timing in decision making; or about changing interpretive contexts and changing contextual impacts on decision making; or about calculations concerning goals, reputations, and credit/blame balances; or about interpretive sequences, negotiating sequences, voting sequences, and career sequences—there is, I would argue, sufficient warrant for the claims of observation. These claims have been stated in incremental language, as befits the notion of "value added," to wit: "sensitize," "emphasize," "suggest," "remind," "correct," "stimulate," "aid," "improve," "highlight." But in research involving politicians, these modest claims may represent an indispensable increment of knowledge and understanding.

I shall close by raising two questions about observation. Do we need political scientists to do it? And if we do, are enough political scientists doing it?

As for the first question, there is a huge corps of journalists who observe politics every day. They do it very well. And we are already heavily dependent on what they tell us about our politicians. If we do

not do it, they will do it for us. But it is in the nature of their occupation that they have neither the training, nor the patience, nor the interest to conduct a dialogue with political science theorizers. Journalists are not conceptualizers or generalizers. They are more interested in episodes than regularities. Their observations are not driven by the questions of political science. Put it this way, if my remarks this evening are interchangeable with the remarks a journalist would have made to you, then I shall desist. If not, I shall argue that we cannot leave the field to the journalists, that we need political scientists to go take a first-hand look at our politicians and report back to us. And we need political scientists to keep doing it. For only we can persist in attaching observation to theory. Surely we cannot construct theories of politics on the basis of observation. But we might ask ourselves whether it is possible to construct theories of politics without observational perspectives—if, that is, we wish our theories to encompass the serious activity of our politicians.

As for the second question, I believe that not enough political scientists are presently engaged in observation. Unless I misread our journals, our graduate methods courses, and our reward structure in general, young political scientists have little incentive to expend time and energy observing politicians. Observation-based research is a rarity in the *American Political Science Review*. There are many reasons for this; and each of them may be valid in its own terms. But the question is a disciplinary one. How much value do we place on observation as a research mode? How much legitimacy do we wish to bestow on observation-based research? Have we tried our very best to teach observation and failed? Or have we made a standing decision against its claims of added value to us? We shall not answer these questions until we examine them with care. Per-

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haps, sometime and somehow, that careful examination will take place within the political science profession. I hope so.

Note

This essay is the presidential address presented on August 29, 1985 at the 81st annual meeting of the

American Political Science Association in New Orleans. I owe a very large debt to many of my fellow political scientists, upon whose work I have drawn. But this essay, printed here exactly as it was delivered, was prepared as a public talk, so it does not include footnote references to the work of the very large number of political scientists to whom it is indebted.

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AMERICAN VOTER TURNOUT IN COMPARATIVE PERSPECTIVE

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Despite relatively favorable citizen attitudes, voter turnout in American national elections is far below the average of 80% of the eligible electorate that votes in other industrialized democracies. The American institutional setting—particularly the party system and the registration laws—severely inhibits voter turnout, and probably also accounts for the unusual degree to which education and other socioeconomic resources are directly linked to voting participation in the United States.

Using a combination of aggregate and comparative survey data, the present analysis suggests that in comparative perspective, turnout in the United States is advantaged about 5% by political attitudes, but disadvantaged 13% by the party system and institutional factors, and up to 14% by the registration laws. The experience of other democracies suggests that encouraging voter participation would contribute to channeling discontent through the electoral process. Even a significantly expanded American electorate would be more interested and involved in political activity than are present voters in most other democracies.

Seen in comparative perspective, American voter turnout presents an interesting paradox. Americans seem to be more politically aware and involved than citizens in any other democracy, yet the levels of voter turnout in the United States are consistently far below the democratic average. The resolution of the paradox lies, apparently, in the nature of voting as a form of participation.

In their study of different forms of political participation in seven nations, Verba, Nie, and Kim (1978) suggest a distinction between two types of forces that

shape political activity. On one hand, there are the attitudes and characteristics that individuals bring to the participatory arena. Participation is, in general, facilitated by greater socioeconomic resources and by general levels of political awareness and self-confidence. On the other hand, participation is also facilitated or hindered by the institutional context within which individuals act. Legal rules, social and political structures, and configurations of partisanship all present the individual with conditions that shape his or her choices, and are relatively difficult for the individual to change. Analysis of

the different types of participation suggests that voting is particularly likely to be dominated by institutional factors.

The present analysis attempts to explain the paradox of American voter participation in terms of the conjunction of the two types of forces cited above. Americans do possess political attitudes that encourage their voting activity. If citizens in other democracies possessed the American configuration of attitudes, their voter participation would on average increase. However, the American attitudinal advantage is only a marginal enhancer of voting. Its effects are limited, first, because in recent years the American attitudinal advantage has declined, and, more importantly, because voting is so powerfully shaped by institutional context. In comparative perspective, the American registration rules, electoral system, and party system inhibit voter participation, outweighing by far the attitudinal advantage.

The subsequent discussion is divided into three sections. The first of these presents aggregate evidence on American advantages in social and attitudinal resources compared with other modern democracies, and contrasts these with the putatively disadvantageous institutional conditions. The second section uses econometric analyses of survey and aggregate data to estimate the relative importance of specific individual and system characteristics that affect voter participation. The third section analyzes the contributions made by each type of factor to relative American voter turnout by combining the estimates of the impact of various individual and institutional attributes with the evidence on the relative American advantages and disadvantages.

The Cultural and Institutional Environment for Voting

Table 1 presents some evidence that allows us to characterize the American

political cultural environment in comparative perspective. The table is divided into several sections. The first compares the democratic publics on some attitudes often demonstrated to be associated with political participation. The second section compares the citizens' reports of three forms of political activity that might be expected to be related to their likelihood of voting. The third section compares the electorates on some demographic characteristics expected to be related to voter turnout.

Although attitudes facilitating participation declined in the United States in the late 1960s (Abramson and Aldrich, 1982; Nie, Verba, and Petrocik, 1976), comparison with political attitudes in the European nations still shows the American public to good advantage. Partisanship has declined, but is still equal to or above average. Eighty-three percent of Americans named a party they usually felt close to, placing the United States behind only the Netherlands and Finland in a twelve-nation comparison. On the other side of the partisanship scale, 14% said they felt very close to their party, a figure slightly behind only two other countries (Austria and Italy), although a drop from the America of the early 1960s.

Despite the drop in political confidence, the United States still led all countries in the number of citizens believing that they had some say in government. In general political interest, the 90% of Americans who reported at least some interest were comparable to their counterparts a decade earlier, and well ahead of the cross-national average. Only in the decline of political trust did the Americans drop from a leading position to one well back. The United States ranked ahead of only Italy in trust of the national government in 1974. (While most of the other attitude levels stabilized in the mid-1970s, and did not decline further, trust actually dropped an additional 8% between 1974 and 1980.) But political trust, although related to voter participation in America, was the

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**Table 1. The Cultural Environment for Voter Participation:
America Compared to Other Industrial Democracies in the 1970s**

Variables		United States %	Averages from Other Industrial Democracies		Rank of the United States
			11 Nations %	7 Nations %	
<i>Attitudes Facilitating Voter Turnout</i>					
Partisanship:	mention a party they usually feel close to	83	73	77	3/12
Efficacy:	reject: "People like me have no say in what the government does."	59	—	33	1/8
Trust:	trust the national government to do what is right: most or all of the time.	34	—	47	7/8
Interest:	possess at least some interest in politics.	91	—	75	1/8
<i>Other Forms of Political Activity</i>					
Discussion:	discussed politics with others	89	68	74	1/12
Persuasion:	tried to convince others during elections.	40	—	27	2/8
Party Work:	worked for a party or candidate during an election.	30	—	14	1/8
<i>Demographic Characteristics Facilitating Participation</i>					
Education:	beyond lower (ninth grade) education.	79	—	39	1/8
Occupation:	white collar jobs in work force	64	49	48	1/12
Age:	over age 34 among population of voting age.	64	66	66	11/12

Sources: See acknowledgement in Notes section for surveys. Occupation from Taylor and Jodice, 1983. Age from United Nations, 1979-1983. Other Democracies (Austria, Britain, Finland, Germany, Italy, Netherlands, Switzerland, Belgium, Denmark, France, and Ireland): Political Action Study (Barnes et al., 1979) and Euro-Barometer (Rabier and Inglehart, 1981).

least important of the four attitude variables in the American attitude studies.¹ We must conclude, therefore, that despite the decline in the period from 1960-1975, American political attitudes should still facilitate more political participation than political attitudes in other democracies.

This conclusion is strongly supported by the second section of the table, which compares three other measures of political activity: (1) discussing politics with other people, (2) trying to convince others to

vote for a party or candidate during an election campaign, and (3) working for a party or candidate during elections. Nearly 90% of the American public reports discussing politics at least some of the time, compared to an average of only 68% across the 11 European nations. (Comparisons at the most active end of the discussion measure yield similar comparative results.) Thirty percent of the American citizens report having worked during a campaign at some time, more

than double the average for seven European nations in which this question was asked. In reporting having tried to convince others during election campaigns, the Americans trail only the West Germans, with 40% affirming such activity, in comparison to an average of 27% in seven other countries. These American results parallel other studies of the American electorate quite closely. The strong relative position of the American public in attitudes that facilitate participation and in various measures of political activity other than voting also appears in other comparative studies, particularly in the five-nation Civic Culture study (Almond and Verba, 1963), and in the seven-nation Participation and Equality study (Verba et al., 1978).

The last section of Table 1 compares the American and European mass publics on three important demographic characteristics that have been shown to be related to political participation propensities. The American participation studies, as well as the comparative studies, have demonstrated that possession of greater social and economic resources, particularly higher levels of education, is associated with attitudes and behavior that facilitate participation. While these comparisons must not be taken completely literally, given differences in occupational structure and in educational quality and content, they help explain the relatively high levels of political awareness and involvement in the United States. The average educational level is much higher in the United States than in most of Western Europe (reflecting the much older American concern for mass education, as opposed to the European elite emphasis). The American citizen is also more likely to hold a white-collar or professional job than his European counterpart, although here the differences are not quite so marked, as all these nations are relatively economically developed.

Only in its age structure do the demographics of the American public tell

against political participation. Many studies have shown that the youngest segment of the electorate, in general those under age 35, tends to participate less in most forms of political activity.² At the time of the 1970 census, the proportion of young voters in the American electorate was above average, although the differences were not very great. The gap increased notably by the 1980 censuses, due in part to reluctance in a few European nations to lower the voting age to 18, but primarily to the "bulge" of young people entering the American electorate in the late 1960s and the 1970s. This demographic change, as we shall discuss later, did increase the gap in turnout between the United States and the other democracies. As late as 1970, however, the age gap was slight.

The picture of American political attitudes and demographic characteristics that emerges from Table 1 leads us to expect high levels of American voter participation. However, the institutional factors facing the American voters are for the most part highly inhibiting, compared with those in the other industrial nations. The following discussion summarizes these differences from the American point of view. The full distribution for the 20 contemporary democracies is presented in Appendix 1.

As has been emphasized in previous comparative studies of voter participation (Crewe, 1981; Glass, Squire, and Wolfinger, 1984; Powell, 1980), the legal situation is of great importance. In some nations, legal sanctions are used to encourage voters to go to the polls. While the nature of the penalties and the level of enforcement varies within and across countries, there is no doubt that such sanctions tend to increase voter turnout. Such mandated voter participation has not been used in the United States, but is present in Australia, Belgium, Greece, and Italy, as well as in parts of Austria and Switzerland.

Moreover, the registration laws make

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voting more difficult in the United States than in almost any other democracy. In 16 of the democracies examined, initiative for registering eligible voters is taken by the government in some fashion. In Australia and New Zealand the citizens must take the initiative; however, they are legally required to do so, and subject to fines or other penalties for failing to register. Of the 20 democracies outside the United States, only France leaves voter registration to voluntary initiative of citizens (Herman, 1976). In France citizens are required to register in their community and to obtain identification cards, which facilitates voter registration. In the United States registration is entirely the responsibility of the citizens and no set of requirements brings them to the registration site. Moreover, although the residency requirement in the United States has now been limited to 30 days before a federal election, only a handful of states with small populations have day-of-voting registration. Other states vary greatly in their registration hours and places, and in the degree to which these facilitate registration (see Rosenstone and Wolfinger, 1978), but all require the voter to re-register if he or she has changed residence since the last election. As the 1980 census showed that 47% of the population had moved in the past five years, it would seem that about half the eligible citizens must, in effect, make a double effort to vote in a presidential election: first the effort to register, then to vote.

A feature of the institutional context that has been the subject of much debate and analysis among American political scientists, but whose comparative implications are harder to measure and assess, is the competitive context. Intuitively, it would seem that in elections in which the outcome was expected to be close, citizens would feel more reason to participate and, perhaps more importantly, party organizations and activists would feel

more incentive to get their voters to the polls.

In Appendix 1, and in the statistical analysis in the next section, two possible aspects of such competition are examined. First, we consider the frequency with which control of the national chief executive, by a party or coalition of parties, changed after an election in the twenty-year period from 1961 through 1980. By this measure, there is no doubt that the environment of the American presidential election was among the most competitive in any democracy. In the United States, party control of the presidency changed hands following four of the six elections between 1960 and 1980. We must note, however, that there were few changes in party control of the national legislature in the United States in this period, with the Democrats maintaining control of the House and losing control of the Senate only in 1980.

The second aspect of competitiveness concerns the possible influence of the electoral constituencies on competition in different parts of a country. The idea is simply that the electoral constituencies help determine whether parties and voters have equal incentive to get voters to the polls in all parts of the country, or whether there may be reason to neglect less evenly balanced regions in turning out the vote. Where the chief executive is chosen by simple majority or plurality vote, all regions should be equally important (e.g., France). In countries where the chief executive is chosen by the legislature, as in the various parliamentary systems, the question becomes the nature of the constituencies electing the legislators. With proportional representation from the nation as a whole or from large districts, parties have an incentive to mobilize everywhere. With single-member districts, some areas may be written off as hopeless.

Various American studies have found such effects in races for state legislative

seats and governor (Caldeira and Patterson, 1982; Patterson and Caldeira, 1983; and references cited therein). Studies in Britain and Canada (Denver and Hands, 1975; Irvine, 1976) also suggest effects that dampen participation in some districts. In this intranational consideration of competitiveness, we would expect the American situation to be most like those in the single-member district countries, as the state acts as the electoral unit in the electoral college, and its electoral votes are delivered as a block, rather than proportionally. We must note, however, that American states shift support in quite volatile fashion. In 1972 about two-thirds of the states went for a candidate by a margin of over 20%, but in 1976 only 4% did so, while in 1980 20% did so.

Another important aspect of the partisan context is the linkage between political parties and social groups. For a variety of reasons, we expect voter turnout to be higher in countries having sharper partisan-group differentials in support. Partisan choice should seem simpler to the less involved; cues from the personal environment of the individual (friends, family, and co-workers) should be more consistent; party organizers can more easily identify their potential supporters in making appeals and in helping voters to the polls on election day. The last column in Appendix 1 shows a measure of the differential ties between voters' membership in social groups and their partisanship. In some countries, as in Sweden or the Netherlands, to know a voter's occupation or religion enables us to predict his or her voting preference to a very great degree. In Sweden in 1964, for example, about 84% of those with manual labor occupations supported the Social Democrats or the Communists, while only about 32% of the voters with white-collar or farm occupations did so, yielding a "class voting" index of 52. In the United States in the same year the manual labor support for the Democrats exceeded

white-collar support by only about 17%. Indices of partisan support based on occupation, religion, and church attendance were calculated from surveys in 20 countries. Appendix 1 shows the highest of these indices.³

As shown in Appendix 1, the party-group support differentials in the United States were only about half as great as those in the average democracy. In fact, the United States had one of the lowest levels of party-group support of any modern democracy. As the bases of the old Roosevelt coalition continued to crumble during the last 20 years, the vote scores on party-group differences fell even faster than these numbers, based on party identification, would indicate (see Abramson, Aldrich, and Rohde, 1983).⁴

While the party-group linkage measure seems to tap an important feature of the party system for voting mobilization, it would be desirable to have explicit measures of the strength of comparative party organizations. We would expect dense, penetrative, nationally-oriented party organizations to be most effective in getting voters to the polls in national elections. Unfortunately, there seem to be no reliable quantitative studies of party organization strength across nations. The only comparative study that even attempts to describe party organization in these terms is Janda's work (1980), which relies on expert coding estimates for 12 of the nations being considered here. The time period was the early 1960s.

The Janda study results are suggestive, and can be summarized here briefly. They portray the American party system as slightly above average in the sheer magnitude and extensiveness of party organization, but highly decentralized and with very weak ties to other social organizations. The results regarding extensiveness are interesting, but most of the major parties scored close to the maximum on the measures reported. There was not much variation across nations on the variable;

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the slight American advantage resulted from weak scores by some of the smaller parties in multiparty systems. I have not been able to discover comparative survey results on voters' reports of contacts by parties or other measures of comparative party effort.

The measures of centralization and organizational ties between parties and social organizations, called "penetration" in the Janda study, differentiate the democracies more sharply. The American parties are highly decentralized, especially in the selection of legislative candidates. On a combined measure of organizational structure, funding, and candidate selection, the United States ranks lowest of the 12 modern democracies in the degree of centralization. On the "penetration" measure, examining the ties between parties and such organizations as trade unions, religious bodies, and ethnic groups, the American parties ranked ahead of only the Irish parties. This measure seems to be the organizational counterpart of the party-group linkage measure based on partisan behavior, and empirically is closely related to it. We would expect that the lesser capacity of American parties to make use of other social organizations to spread their messages and to get voters to the polls would hinder their mobilization efforts.

We can summarize the description of the institutional environment for voting in the United States in comparison with other democracies by saying that it is the inverse of our description of the cultural environment. With few exceptions (age structure and political trust), the evidence on American political culture suggests that it should facilitate all kinds of individual political activity. With one exception (experience of party changes in national control of the executive), the institutional factors would seem to make the act of voting more difficult, and to impede the ability of parties and activists to mobilize supporters through appeals or

through election day efforts to get them to the voting booth.

Estimating the Impact of Individual and Institutional Variables on Voter Turnout

While the evidence in Section I suggests reasons for America's exceptional pattern of an involved, but nonvoting citizenry, we need much more precise estimates of the relative importance of factors at the individual and system levels believed to affect voting. As it is likely that variables at both the individual and system levels are significant, the ideal analysis would consider both types of factors simultaneously. Unfortunately, we do not have comparable attitude surveys for half of the industrialized democracies. For the moment, therefore, we shall develop separate models of voter turnout. One of these models will be based on aggregate analysis of system-level variables, using the full set of 20 industrialized democracies. The second will be based on individual surveys from nine nations. In conclusion we shall consider on a preliminary basis the interaction of levels.

Comparisons of American Voter Turnout

The first step is to measure American voter turnout in comparative perspective. The problem is fraught with technical difficulties, but the overall situation is quite clear. In comparison with other democracies, the United States has relatively low participation of its citizens in major national elections. Average turnout in presidential elections in the United States as a percentage of the voting-age population was 54% in the period from 1972 to 1980. In the other 20 industrialized democracies the average turnout was 80%. American national voter participation exceeds only that of Switzerland. Among the nations that did not have

compulsory voting, average voter turnout was 77% of the population of voting age, nearly 50% higher than turnout in the United States. This comparison is probably the most valid and reliable of those available. Detailed data are shown in Appendix 1. Similar comparisons may be found in Crewe (1981), Glass et al. (1984), and Powell (1980).

These comparisons rely on official reports of votes in the election that determines most directly the control of the chief executive. In the United States and France, these are presidential elections; in the other industrialized democracies, the most comparable elections are for the national legislature. The major problem in comparison is the denominator of the voting ratio: the eligible population. As turnout tends to be quite stable from election to election, the use of averages does not conceal many changes, but does help even out small errors in the census survey-based estimates of the voting age population at the time of the election. Appendix 3 discusses problems for comparison created by resident aliens in the population.

An important point to recognize about American voter turnout in comparative perspective is its close relation to voter registration. The United States is unique in the low registration rate of its population of voting age. Comparisons of turnout as percentages of either voting-age or registered populations lead to similar numbers in most countries, but radically different ones in the United States. In the United States perhaps two-thirds of eligible citizens are registered; of the other democracies, only in Switzerland are less than 90% of the citizens of voting age registered.

A final comparative point considers the time perspective. In most countries voter turnout changed little from the 1960s to the 1970s, remaining rather stable from election to election. In the United States, however, turnout dropped notably, for

reasons various scholars have discussed elsewhere. These changes only served to widen the gap in electoral participation between the United States and the other democracies. They did not create the gap, nor, in fact, did the decline in American turnout even change its rank against any other country.

Aggregate-Level Explanations of Voter Turnout

I have collected data on most of the contextual and institutional factors described in the previous sections for the modern democracies. We can use these to attempt to explain differences in turnout across the democracies. As the institutional conditions do not change very often, it seems more appropriate to compare country averages than to enter individual elections as separate cases. Table 2, then, presents the model for voter turnout as a percentage of the population of voting age. Theoretically, we would expect that in the presence of compulsory voting, other factors encouraging participation have less effect. The countries where voting is compulsory are therefore deleted in the models in Table 2. (If we include them and a dummy variable for compulsory voting, the latter is powerful and significant, while the other aggregate coefficients are slightly depressed.)

The figures for the 1960s and 1970s are shown separately, as we hope to have more confidence in the results that are consistent at the two time periods. With a few minor exceptions, the independent variables are also measured separately at the two time points. Spain is not included in the first decade, as it did not become a democracy until the late 1970s. (Absence of party-group linkage data for Switzerland before 1972 and Israel in the 1970s forces us to use the same figure for both decades.) Switzerland is included as a dummy variable because of the unique

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**Table 2. Aggregate-Level Explanations of Voter Turnout:
Predicting Average Turnout as Percent of the Population of Voting Age
in Democracies Without Compulsory Voting**

Predictor Variable	Regression Coefficient	Standard Error	Standardized Beta
<i>Voter Turnout in the 1960s: 1960-1970</i>			
Age: Percent over age 34	.01	.32	.00
Automatic Registration	3.70	2.51	.23 ^a
Frequent Changes in Executive	.43	1.58	.03
Nationally Competitive Election Districts	1.89	1.45	.22 ^a
Party-Group Linkage Index	.38	.15	.41 ^a
United States Dummy	- 1.71	7.74	-.04
Switzerland Dummy	-30.11	5.61	-.75 ^a
N=15 R ² = .90			
<i>Voter Turnout in the 1970s: 1971-1980</i>			
Age: Percent over age 34	.38	.28	.13 ^a
Automatic Registration	.33	1.97	.02
Frequent Changes in Executive	1.26	1.23	.11
Nationally Competitive Election Districts	1.78	1.09	.18 ^a
Party-Group Linkage Index	.45	.18	.33 ^a
United States Dummy	-12.30	6.46	-.25 ^a
Switzerland Dummy	-38.50	4.89	-.79 ^a
N=17 R ² = .94			

Sources: Sources and data coding shown in Appendix 1.

^aSignificant at the .05 level.

nature of its collective executive and other special features of Swiss democracy.⁵ A dummy variable is also included for the U.S., with its special registration conditions.

If we consider the general aggregate models emerging from Table 2, we see that they are reasonably stable and consistent across the time periods. (With few exceptions, they are also consistent with the analysis of voter turnout in 29 democracies reported in Powell, 1980, 1982.) The coefficients for intranationally competitive electoral laws and party-group linkages are rather similar in both time periods; party-group linkages are particularly strong and significant. The impact of changes in the chief executive is both weaker and less consistent. The unstandardized age coefficients are larger in the second decade, presumably reflecting the lowering of the voting age in most of the

democracies in the 1970s, increasing the weight of an under-34 component. The dummy for Switzerland reflects the significantly lower turnout in the Swiss system in both decades, although the gap has increased over time. The presence of automatic registration facilitates turnout; its effects are stronger if we delete the U.S. dummy. (The increasingly negative U.S. dummy is discussed below.)

The implications of these data are that voter turnout in the United States is severely inhibited by its institutional context. The only feature of the institutional context where the United States seemed to enjoy a clear-cut advantage was in the frequent changes in chief executive—a variable that was insignificant in each decade in the aggregate analysis. (Considering the Swiss case as unique, rather than occupying the end of a continuum, in this regard.) The U.S. was disadvan-

tagged by voluntary registration, unevenly competitive electoral districts, and very weak linkages (perceptual and organizational) between parties and social groups. The distance of 23 points between American party-group linkages and those in the average democracy would alone have been predicted to depress turnout by about 10%, based on the average coefficient in Table 2.

These models also work very similarly if we use turnout as a percent of the registered electorate as the dependent variable. The major difference is a reversal in sign of the automatic registration variable. That is, the presence of automatic registration facilitates voting participation of the age-eligible population, but leads to lower turnout among the registered. Such effects probably reflect the differing degrees of interest and partisanship required to enter the pool of the registered in the two kinds of registration situations. It is consistent with the well-known fact that turnout of registered voters is actually very high in the United States.

One variable not shown in the table is worthy of comment. While comparable education statistics were not available, I did attempt to use a measure of the percent of the labor force in white-collar occupations to get at the greater socioeconomic resources and skills available in some populations. The white-collar variable was positively related to aggregate levels of political discussion, in the 14 nations for which I had such a measure, at a significant and positive level ($r = .53$), just as we would predict from participation theory and from the individual models examined in the next section. However, the percent white-collar was negatively related to voter turnout ($r = -.18$), and when entered into the multiple regression with the institutional variables tended to be reduced toward zero. Accordingly, the white-collar variable is deleted in Table 2; its effects in

mobilizing awareness are not sufficient to make an impact on turnout in the aggregate data analysis.

The analysis in Table 2 makes it possible to estimate the effects of the institutional variables in shaping voter turnout in modern democracies. I would emphasize, however, that we must be careful in the substantive interpretation of the institutional variables. The variables for intra-national constituencies that enhance competition, and those for party-group linkage are the major source of concern. They are getting at some institutional property of the systems that affect turnout, to be sure. But these two variables are themselves related, and, in the subset of 12 countries for which we have the rather doubtful data, both are related to party organizational structure—especially party penetration of social groups. The party organization variables are not entered in the models, because of the limited number of cases and dubious nature of the data. If we do analyze those 12 countries, we find that centralization and penetration of social groups are strongly associated with turnout and with party-group linkage. Extensiveness, which has little variance, is not related to turnout. In multiple regression analysis, however, party-group linkage tends to reduce the impact of centralization and penetration to insignificance. Given the measurement problems, it is unwise to be confident about which of these aspects of the party and electoral system are the ones shaping turnout.

Individual-Level Explanations of Voter Turnout

Our analysis of voter turnout at the individual level relies on comparative survey studies. We would like to know two things. First, are the processes of voter involvement in the United States similar to those in other countries despite the differences in context? Second, if not,

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are there reasonably similar processes operating in other democracies, so that we could estimate the relative importance of various individual-level variables if the United States did have electoral and party contexts more comparable to those in other countries?

One difficulty should be noted first. We know that American survey studies consistently report greater levels of participation than do aggregate statistics. The difference reflects in part the population reached by the completed survey, and in part a tendency of citizens to overreport their participation. Judging by analysis of the voter validation studies conducted by the University of Michigan Survey Research Center (SRC) in 1964, 1976, and 1980, the overstatement of participation of some 18% in the 1970s reflects about an 11% net overreporting by citizens, and a problem of about 7% in the survey reaching parts of the population of voting age (Clausen, 1968-69; Katosh and Traugott, 1981). Citizen surveys in most other modern democracies do not show as large an overstatement as do the American studies. However, the percentage of non-voters who report voting in other countries seems not dissimilar to that in the United States—about one-fourth to one-third the nonvoters in the sample. (This statement is an informed guess, based on comparison of official and reported turnout across 14 surveys; the proportions due to sampling and response errors cannot be estimated accurately.) It is reassuring for our purposes that the relative rank of the United States among the nations which we shall be comparing here is the same using either survey or official statistics, with the United States ahead of Switzerland, but behind the other nations.

Table 3 uses standard demographic variables plus party identification (ID) to predict political discussion and voting in the most recent national election from surveys in nine nations, including the United States. The left side of the table

shows the model for voter turnout; the right side shows a model for political discussion. On the far left side of the table we see categories of increasing education and age, as well as entries for sex and party identification. The cell entries show the increased probability of voting as we move, for example, from one education group to the next, in comparison with the base-line group. In the United States there is on average no increased probability of voting as we move from sixth grade to ninth grade education, but an increase of 10% as we move from ninth grade to the eleventh grade, and another 17% for actually completing high school. Turnout among the college educated is 35% greater than among those with a primary education only. We can compare these sharp education effects with the average effects in the other eight nations: a consistent, but small, increase of 2-3% as we move up the comparable categories, with turnout among the college educated only 10% higher than among those with a primary education. The American voter participation process is obviously far more affected by education levels than the process in other nations.

Table 3, and Table 4 below, show the unstandardized regression coefficients (multiplied by 100) from the ordinary least squares regression equations that predict voting using demographic and attitudinal variables. The dummy variables for each group category allow us to make direct comparisons with the base-line category and observe possible curvilinear effects, while controlling for other variables in the equations. Because voting is dichotomous, however, there is a potential problem of misestimating the magnitude of the coefficients by using multiple regression. The use of LOGIT or PROBIT provides reliable estimates with such dichotomous dependent variables. All the equations in the individual level analysis have been reestimated using LOGIT and PROBIT. In fact, as usually

Table 3. Individual-Level Explanations of Voter Turnout:
Predicting the Increase in Probability of Voting
and Talking Politics in Nine Nations

Independent Variables	Predicted Increment in Activity Relative to Base-line Group in the Category ^a			
	Percent Who Voted in Last National Election		Percent Who Discussed Politics	
	United States	Eight-Nation Average	United States	Eight-Nation Average
<i>Education Level^b</i>				
Basic	—	—	—	—
Lower	(0)	2	10	11
Extended Lower	10	4	16	17
Middle	27	7	24	25
Post-Secondary	35	10	29	28
<i>Sex</i>				
Male	—	—	—	—
Female	- 6	- 1	- 5	-12
<i>Age</i>				
20-25	—	—	—	—
26-29	9	21	(5)	(1)
30-39	21	25	(5)	(0)
40-49	25	30	(3)	(- 1)
50-59	32	(30)	(5)	(- 2)
60+	40	(30)	(- 2)	- 8
<i>Party Identification</i>				
No	—	—	—	—
Yes	18	17	13	13

^aThese numbers are $100 \times$ the unstandardized regression coefficients in regression equations with dummy variables for each group except the base-line group in each category. Coefficients in parentheses are not significant at the .05 level. As demonstrated in Appendix 2, LOGIT and PROBIT models provide nearly identical estimates. The eight nations are Britain, West Germany, Netherlands, Austria, Italy, Switzerland, Finland, and Canada. (Coefficients are averaged, not taken from a single pooled data equation.) For Canada the analysis uses federal party identification in 1974 and the same education categories that were used in the United States.

^bThe education variable is based on codes constructed for each country that attempt to identify comparable levels across countries. See Barnes et al. (1979), pp. 584-588. In the U.S. the levels correspond to the following categories: Under 7 grades completed, 7-9 grades, 10-11 grades, Completed High School, and Post High School.

seems to be the case, the solutions are virtually identical for those with ordinary multiple regression. Appendix 2 demonstrates the extremely similar predictions from the three models. As multiple regression provides more readily interpreted coefficients, it is used in Tables 3 and 4. The results of this analysis emphasize the robustness of regression results when

dealing with dichotomous dependent variables, and suggest that the concern frequently expressed by readers on this score is seldom justified.

For readers accustomed to standardized regression coefficients, I can report that comparing, for example, average regression coefficients for a Socioeconomic Resource Level (SERL) variable of educa-

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tion and income, as used by Verba et al. (1978), in the equations yields a standardized beta coefficient of .33 between SERL and vote in the U.S., and only .05 between SERL and vote in 10 other nations.

The greater power of education effects in the United States might lead us to suspect that the education variable was being measured inadequately or differently in different countries. While we cannot disprove this possibility, some persuasive evidence that it is not so is offered in the two right-hand columns of Table 3. Here, the model is applied to a dichotomous measure of political discussion. The discussion results are striking for the great similarity between the impact of education in the United States and the other democracies, and are a tribute to the comparability of the Barnes and Kaase (1979) coding of education level. Not only does the probability of discussing politics rise with each increment of education, but the effects are extremely similar across countries.⁶ This comparison seems powerful evidence that the voting participation process, but not the general process of personal involvement, is quite different in the United States.

Not only education, but age, shows a different relationship to voter turnout in the United States and the other countries. In the United States, each age increment shows increased probability of voting. In the other nations, the effects are very great going from the first to the second age group, but quite weak thereafter. On the other hand, the effect of party identification on voter turnout, and on discussion, is about the same in the United States as in the European average. Women voted somewhat less often than men in the United States, while the average difference was very small in other countries. Political discussion showed a sex gap in all countries, but the American difference was much less than that in the other democracies.⁷

The comparison of averages in Table 3 does, of course, blur differences among the non-American nations. Moreover, we need clearer evidence on the attitudinal processes involved in getting voters to the polls. In Table 4 we see individual participation models for the eight nations in the 1973-1976 Political Action study plus Canada in 1974, and including measures of interest and efficacy variables used in many American studies. For the sake of simplicity, political trust is not included here; its effects are quite weak, although in the predicted direction in most countries. The top of the table shows, again, the increased amount of voting participation expected from each increment of education, controlling for interest and efficacy as well for sex, age, and party identification. Toward the bottom of the table we see the predictions for interest and efficacy. Each of the nine nations is presented separately, as we are not controlling for system-level effects. (The efficacy variable is a two-question variable, including the "no say" item shown in Table 1, exactly as used in Abramson and Aldrich, 1982.)

The data in Table 4 are complex, but rich in information. The first point to note is that the individual voter participation processes in Austria and, especially, Italy (shown at the far right of the table) are rather different from those in the other countries. The attitudinal variables, particularly interest, but also efficacy, education, and even party identification, have much less effect in these two countries. And age is notable for the very great increase between the 21-24 group and the next older one, with very little subsequent change. It seems likely that voter participation in Austria and Italy is dominated primarily by institutional effects. Both countries have substantial compulsory voting, and Austria has an extremely well-organized and penetrative party system, so these patterns are not too surprising. Moreover, the extremely high

Table 4. Individual-Level Explanations of Voter Turnout:
Predicting the Increase in Probability of Voting from
Demographic and Attitudinal Variables in Nine Nations

Independent Variables	Predicted Increment in Voter Turnout Relative to Base-line Group (%) ^b								
	USA	BRIT	WGER	NETH	SWITZ	FIN	CAN	AUST	ITALY
<i>Education Level</i> ^a									
Basic	—	—	—	—	—	—	—	—	—
Lower	- 5	- 3	0	- 1	10*	- 3	1	1	0
Extended Lower	2	- 2	2	2	7*	0	5	1	3
Middle	15*	1	4	7	9*	5	9*	- 0	0
Post-Secondary	21*	6	3	7	15*	4	5	2	- 4
<i>Sex</i>									
Male	—	—	—	—	—	—	—	—	—
Female	- 3	1	- 0	8*	- 4	0	- 0	1	0
<i>Age</i>									
20-25	—	—	—	—	—	—	—	—	—
26-29	10*	12*	14*	13*	9*	25*	3	40*	48*
30-39	19*	26*	16*	9*	16*	32*	6	39*	51*
40-49	23*	36*	15*	15*	23*	31*	15*	40*	51*
50-59	29*	34*	16*	19*	25*	35*	10*	40*	52*
60+	37*	33*	15*	21*	20*	35*	15*	40*	52*
<i>Party Identification</i>									
No	—	—	—	—	—	—	—	—	—
Yes	13*	24*	8*	33*	21*	9*	4*	4*	3*
<i>Political Interest</i>									
Not at All	—	—	—	—	—	—	—	—	—
Not Much	17*	9*	5*	8*	19*	9*	24*	2	2
Somewhat	30*	15*	7*	12*	34*	15*	34*	1	4*
Very	32*	18*	8*	8*	33*	14*	36*	2	- 0
<i>Political Efficacy</i>									
Low	—	—	—	—	—	—	—	—	—
Mixed	6*	3	2	2	8*	1	- 1	1	- 3*
High	9*	6*	1	7*	6	- 0	- 0	1	- 4*
Reported Turnout (%)	74	79	94	85	59	90	82	96	95

^aSee Note to Table 3 for education levels.

^bSee Note to Table 3 for source of estimate.

* indicates that turnout of group was significantly above base-line group (.05). N of cases from 1030 to 2149.

reported turnout levels of 95% leave limited room for attitudinal effects to have play.

The second point to note about Table 4 is that the six "middle" countries, with automatic registration but without compulsory voting, manifest voter process models that seem rather similar. Naturally, we do find substantial variation—as

we would expect from the measurement and language differences, the rather small subgroups in some categories, and the very high reported turnout levels (over 90% in Germany and Finland). But in each country we see sharp, slightly curvilinear effects of political interest and, less consistently, efficacy. Party identification, although varying in magnitude, is a

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significant direct predictor of turnout in each country.⁸ Age has a generally positive and curvilinear effect, although the timing of drop-off varies. Sex is an insignificant predictor of turnout in all countries except the Netherlands, after interest and party identification are taken into account. Finally, we see that after political interest and efficacy have been included in the model, education has very weak and often insignificant effects on turnout. In most countries a somewhat weak initial effect of education (summarized in Table 3) is further reduced by taking account of political interest. Scattered and collectively significant small effects do remain, particularly at the middle and higher education levels.

Considering the American model in comparison with the other six nations without compulsory voting suggests both commonality and difference. On the one hand, attitudinal effects are rather similar. Party identification has an effect which falls about at the average of the other democracies. Interest is similar in strength, if on the higher side, and similar in its curvilinearity (not much impact by the increase from "somewhat" to "very" interested). Efficacy has an effect in the United States that is somewhat above the democratic average. Sex is insignificant in its effect on turnout after attitudinal variables are taken into account.

On the other hand, the direct effects of age and, especially, education are much greater in the United States than in any of the other countries. The age variable is notable for its continuing impact as we move to increasingly older groups. The impact is about average as citizens age from 20 to 39, but continues in the U.S., while becoming weaker in the higher age categories elsewhere. It seems likely that the lesser mobility of the older age groups, in conjunction with the unique American registration system, plays a major role in accounting for this continuing impact of age in the United States

(Squire, Glass and Wolfinger, 1984; Wolfinger and Rosenstone, 1980, ch. 3). Unfortunately, a variable for length of residence is not included in the eight-nation study.

Most distinctive of all is the direct impact of education on American voter turnout. While the effect of education shown in Table 3 is reduced somewhat with interest and efficacy in the model, the direct effects are still quite powerful, with high school graduation worth 13% and college work another 6%. Not only does the United States have the most educated citizenry, but education has much more direct impact on voter turnout.⁹ It seems very likely, although we cannot demonstrate it directly, that the difficulty of registration in America is also responsible for this remarkable distinctiveness of American voting processes. The great weakness of the party system in its organization and linkage to social groups may also enhance the value of personal characteristics and resources.

Of course, the overwhelming point to make about the models shown in Tables 3 and 4, when compared to the evidence on the cultural environment from Table 1, is that they all lead us to expect high voter turnout in the United States. The American electorate is highly educated, has above average levels of party identification, and is more interested and more efficacious than citizens in other democracies. The relatively low levels of trust are not as important as these other American advantages.

These individual-level modeling efforts redirect our attention to contextual factors operating largely, it would seem, at the national level. It is possible, of course, that incorporation of attitudinal and demographic variables here unmeasured would succeed in explaining the differences across nations in level of voter turnout. It would be especially reassuring to see a variable for citizen duty available across countries.¹⁰ But the comparisons

with political discussion suggest that the present variables, at least, are being measured fairly well, and that they do not account for the relatively low levels of American voter participation.

Effects of Misspecification in the Aggregate- and Individual-Level Models

Thus far we have estimated the importance of aggregate and individual characteristics in predicting voter turnout in separate models. To use the estimates in evaluating the factors depressing or encouraging American voter turnout, we need some assurance that each model is being reasonably well specified, despite absence of the variables in the other model. We cannot fully solve this problem without comparable attitude surveys in all the democracies, but we can gain some confidence by conducting the individual and aggregate analysis jointly in the subset of countries for which we have both kinds of data, and within which the individual models were fairly similar. We can pool the 7000 cases in the six countries that have both automatic registration and voluntary voting, replace the names of countries with their values on aggregate contextual variables, and estimate the coefficients with both individual and contextual variables operating together.

The problem in this approach is that we still have only six countries, despite 7000 cases, and some of the contextual variables interact badly, creating unstable estimates. Nor do we have the degrees of freedom to enter many contextual variables simultaneously. We can, however, get some sense of the specification problem by using only a few of the aggregate variables. We can first run the model with these variables only, then add the attitudinal variables and see if the estimates change greatly. The process is reversed to ascertain the stability of the attitudinal estimates. The results of these procedures are relatively reassuring.

Consider first the aggregate coefficients. I ran the model with dummy variables for Switzerland, the age categories, and district competitiveness (coded single-member district, multi-district proportional representation [PR], national PR). Then I added the full set of individual-level variables for education, party identification, interest, and efficacy. The estimates for district competitiveness increased slightly after adding the individual-level variables. (The resulting model is that shown in Appendix 2.) Repeating this analysis using party-group linkages rather than district competitiveness also led to an increase in the party-group linkages coefficient by about 20% after including the attitudinal variables. Although the size of the coefficients is, naturally, rather different than the results in Table 2, because of the small number of cases and the inclusion of only one variable at a time, the analysis suggests that we can use the estimates from Table 2 without being too worried that they are badly biased by the absence of the attitudinal variables. Naturally, we would prefer to use the estimates based on the full set of democracies, rather than the six in the survey.

The results for the individual variables are also fairly robust, but show some need for caution. If we pool the six countries and run the individual-level variables alone, we get estimates for party identification, interest, and efficacy that are fairly similar to results including the aggregate variables (either as in Appendix 2, or with dummy variables for five of the countries). But the education effects, always somewhat weak and inconsistent in these countries, are estimated to be weakly negative after interest and efficacy are taken into account. Not until we include a variable for district competitiveness, or the country dummies, do we get the weakly positive education estimates we might expect from averaging the coefficients in Table 4.

The results of this exploration of the

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specification problem suggest that we can use the estimates directly from Table 2 for the contextual variables. But estimates for the individual-level variables must either explicitly include contextual variables, as in Appendix 2, or use country dummies or an averaging procedure that implicitly takes account of such effects.

Estimating the Effects of Cultural and Institutional Setting on Relative American Voter Turnout Levels

The coefficients estimated in the previous section can be used to analyze the effect of individual and institutional variables on levels of voter turnout in the United States as compared with other democracies. We must, however, keep in mind the unique nature of the American model, as shown in Tables 3 and 4. Because of the unique difficulty of registration in the United States, if for no other reason, it is difficult to be sure how the contextual variables affect American participation under present conditions. We shall approach the problem by using the coefficients estimated in the previous section to predict how turnout in the average modern democracy would change if its attitudinal and competitive conditions were similar to those in the United States.

Beginning with the individual-level variables, we have estimated the increment to turnout created by increased levels of party identification, education, interest, and efficacy. By comparing the percentage of the American citizenry with those characteristics with the percentage in the average democracy, we can multiply the difference by the estimated turnout increment to see the predicted effect of the average democracy developing a political culture similar to the American. For example, 84% of the American respondents stated a party identification, compared to 78% of those in the other democracies. If we multiply the 6%

American advantage in party identification by the predicted increment of 14% (from Appendix 2) for individuals in the average democracy having a party identification, we estimate that increasing party identification in other countries to the American level would increase their average voter turnout by .85%—a bit under a 1% gain in turnout. In the case of education, interest, and efficacy, we do the analysis for each category above the base-line category, and sum the results to see the impact of the full distribution on the expected levels of voter turnout. The analysis is shown in the top half of Table 5.

While the analysis is somewhat complex, the results are simply summarized. If citizens in the average democracy were as interested in politics as Americans, voter turnout would increase by 2.2%; American levels of efficacy would increase turnout by .5%. The education coefficients estimated in Appendix 2 would increase turnout by 1.6% if the average democracy reached American education levels. These education estimates, as noted, are somewhat unstable; using average increments from Table 4 leads us to predict a 2.7% increase; using a model with country dummies leads to a figure between these. We should note that while the education coefficients are small, the huge American advantage in education levels has a notable effect. Over all, the United States is advantaged by its political culture. If the average democracy had a political culture as facilitating to voter turnout as American education and attitudes, we would expect turnout to increase by about 5%.

We can do the same thing with the institutional factors, using the models in Table 2. The presentation and analysis are slightly simpler here, because we use the linear estimates from Table 2, and can make predictions on average differences between the U.S. and the average democracy. If the other democracies had

Table 5. Predicting Changes in Voter Turnout if the Average Democracy Had Automatic Registration, but Individual and Institutional Characteristics Similar to the United States

Independent Variables	Distributions on Independent Variables ^a (%)		United States Advantage in 1970s (%)	Estimated Regression Coefficient ^b	Predicted Change in Turnout in Average Democracy (%)
	Other Nations	United States			
<i>Education</i>					
Basic	36	5	—	—	—
Lower	22	16	- 6	- .011	.07
Extended Lower	21	13	- 8	- .003	.02
Middle	11	31	20	.026	.52
Higher	10	35	25	.039	.98
<i>Party Identification</i>					
No	22	16	—	—	—
Yes	78	84	6	.142	.85
<i>Political Interest</i>					
None	17	9	—	—	—
Not Much	27	21	- 6	.118	-.71
Some	38	44	6	.189	1.13
Very Much	17	26	9	.197	1.77
<i>Political Efficacy</i>					
No	49	34	—	—	—
Some	29	29	0	.024	0
Yes	22	37	15	.033	.50
<i>Age</i>					
Over 34 Years	63	58	- 5	.380	- 1.90
Nationally Competitive Election Districts Scale	2.76	1	- 1.76	1.800	- 3.20
Party-Group Linkage Index	36	13	-23	.450	-10.40
Net Predicted Changes due to:					
Individual Level Variables of Education, Party, Interest, Efficacy					5.1
Aggregate Age Distribution					- 1.9
Institutional Variables					-13.6

Sources and Variables: See Tables 1, 2, 3, and Appendices 1 and 2.

^aInstitutional variables from Appendix 1, excluding compulsory voting nations. Attitudinal variables for the countries in Appendix 2.

^bCoefficients from Table 2 and Appendix 2.

the American levels on competition-encouraging constituencies and party-group linkages, their turnout would be predicted to decrease by about 13%. The weak American linkages between parties

and groups (and the associated weak party organizations) would reduce turnout by 10%. The low competitiveness of some American electoral constituencies would reduce turnout by about 3%. (The

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variable for changes in the chief executive, insignificant in both decades as shown in Table 2, is not included.) The age level increased turnout in the 1960s by a small amount, but decreases it about 2% in the 1970s, with the American lowering of voting age and the age bulge among the young. The net effect of all the variables in Table 5 is to lower turnout by about 10%, the American attitudinal advantage being outweighed by the institutional disadvantage on a 13 to 5 basis, with age adding another 2% disadvantage.

If we make the more heroic assumption that adopting automatic registration would create an American voting process like those in the other democracies, we would predict, then, that such registration would lead to American turnout levels some 10% below those of the cross-national averages. Recalling that turnout in the countries without compulsory voting (other than Switzerland) averaged 80%, we see that such changes would mean American turnout of the age-eligible would be increased from 54% to 70%. As discussed in Appendix 3, the presence of resident aliens, ineligible to vote, would limit this by at least 2%, to about 68%. Given the American institutional disadvantages (apart from registration), the U.S. would still have one of the lowest turnout levels of any democracy, tied with Canada and ahead of Switzerland, but the gap would be far less.

In comparison to present American voter turnout levels, the analysis implies that if the United States adopted automatic registration, or something similar, turnout might be increased by 14%.¹¹ This estimate is not inconsistent with that based on cross-state comparisons by Rosenstone and Wolfinger (1978), predicting that voter turnout in the early 1970s would have increased by about 9% if all states had had registration laws as facilitating as those in the most permissive states. It also fits reasonably well with the

point made by Glass et al. (1984) that average turnout in states with election-day registration in the 1980 election was about 66%, some 13% above the national average. None of these states, of course, had automatic or compulsory registration. (The 1980 comparison, however, does not consider other attitudinal and institutional characteristics of the states with election-day registration.)

As Glass et al. (1984) and others have argued, getting most American citizens registered would lead to a major increase in American voter turnout. However, the present analysis suggests that it would not lead the U.S. to overtake most other democracies in voter turnout. Assuming that the U.S. does not wish to introduce compulsory voting, the other institutional factors are probably hard to implement. To make the presidential elections competitive across the country by doing away with the electoral college would probably help somewhat, but not as immediately as the 3% constituency factor in the model indicates, as it is surely capturing some party system effects. And the single-member district effects at legislative and lower levels would remain.

A final point here concerns changes over the last two decades. As has been noted by various scholars, the attitudinal characteristics that enhance participation in the U.S. have declined sharply since the early 1960s. If the 1960-64 levels of American education, partisanship, interest, efficacy, and trust are compared to the European averages of the 1970s (shown in Table 1) the U.S. ranks first in each measure by far. As we saw in Table 4, these individual characteristics are particularly important for voting in the American context. The more negative U.S. dummy coefficient in the 1970s in Table 2 seems to reflect the degree to which the attitudinal advantages compensated for the difficult U.S. registration conditions in the 1960s, but not in the 1970s.

Concluding Comments

In closing it is perhaps useful to separate the firm conclusions of this analysis from the more speculative ones. Comparison of voter turnout in the United States with voter turnout in other industrialized democracies leads to four observations in which we can be quite confident:

- (1) Measured as a percentage of the population of voting age, voter turnout in the United States is very low in comparison with the other democracies. It was well below average in the 1960s and has declined even further, while average turnout in the other democracies has been stable at about 80%.
- (2) The American attitudinal environment is, nonetheless, rather favorable to citizen participation of all kinds, including voting, although less so than in the early 1960s.
- (3) The American legal and institutional environment is inhibiting to voter participation.
- (4) As a form of participation, voting is particularly influenced by institutional factors, although attitudes are relevant.

Some other forms of participation, such as political discussion, are much less influenced by the institutional setting, and Americans are comparatively highly active in these forms of political involvement.

Although we cannot be quite as confident, it seems very likely that the unique American registration laws, which require frequent citizen initiatives from a mobile population, play a substantial role in depressing American voter turnout. It is also likely that these laws are responsible for the unusual degree to which education and other socioeconomic resources are directly related to voter turnout in the United States, even beyond their role (found virtually everywhere) in creating

the attitudes of interest and efficacy which encourage participation.

The specific weights that have been attributed to the various factors are much more speculative, for a variety of technical reasons. This analysis suggests that in comparative perspective, the United States would be advantaged about 3% by its configuration of attitudes (especially political interest) and another 2% by its education level, but disadvantaged 2% by the age levels, 13% by the other institutional and party system factors, and up to 14% by the registration laws. There has been a marked decline in the attitudinal advantage over the last two decades. Although the amount of its effect is somewhat in doubt, the registration laws are probably the most easily altered factor, as well as perhaps the most important. Changing these laws would still leave the United States with below-average voter turnout, but the gap between the United States and the cross-national average would be greatly reduced. Changing the structure of party competition to mobilize lower class voters, for example, is probably much more difficult, although blacks represent an obvious target of opportunity for the Democratic party.

A full-scale analysis of the consequences of voter turnout is beyond the scope of this paper. We can, however, briefly conclude with a comment on two aspects of the problem: system legitimacy and voter quality. A substantial debate exists in the democratic theory literature concerning the implications of high levels of voter turnout for the legitimacy and stability of democratic systems. On one hand, theorists favoring citizen participation have argued that higher levels of turnout reflect and encourage political legitimacy and citizen support. On the other hand, theorists concerned about democratic stability have pointed to the often undemocratic values of the less educated, and the high levels of turnout in such unstable systems as Weimar Ger-

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many and postwar Italy, to argue the dangers of high voter turnout levels.

Recent empirical analysis suggests that the theorists favoring participation have the best of the argument. Analysis of voter turnout in 29 democracies clearly shows a strong association between higher turnout and less citizen turmoil and violence. After controlling for various economic, constitutional, and party system variables, higher levels of turnout still seemed associated with less frequent citizen riots and protests, although not related to deaths by political violence or the overthrow of democracy (Powell, 1982, ch. 10). On the other hand, the countries with higher levels of turnout, at least among the more industrialized countries, did tend to have less durable tenure of the chief executive. From the American perspective, where a presidential system usually creates substantial executive durability in any case, the balance of the evidence would seem to favor higher levels of citizen voter involvement. There should be some channeling of citizen discontent out of the streets and through legitimate political channels without loss of executive stability.

The argument about the quality of the voter can be approached from two directions. On one hand, because of the effects of social and economic resources under the present system, there is little doubt that adopting automatic registration or other measures to encourage turnout of the less well-off would bring to the polls a total electorate somewhat less interested, efficacious, and informed than the present voters. We do not want to overstate this fact, because we have already estimated that such changes would only increase turnout to less than 70%, and some small effects of interest and education in encouraging voters remain. But some decrease in the present levels of voter sophistication (not exactly overwhelming in ideal terms) would probably occur. On the other hand, we must recall that by comparative standards, the American electorate is extremely interested and involved. Even the total mobilization of all American citizens would mean a voting group that is more interested, efficacious, and more likely to engage in political discussion and other activity than are present voters in most other democracies.

Appendix 1. Voter Turnout and Institutional Characteristics of
Twenty Democracies in the 1970s

Country	Average Turnout as Percent Eligible	Average Registered as Percent Eligible	Compulsory Voting	Eligible Required to Register	Frequency of Change in Chief Executive ^a	Type of Electoral District ^b	Average Strength of Party-Group Linkages
Australia	86	91	Yes	Yes	3	1	33
Austria	88	96	No	Automatic	3	3	42
Belgium	88	95	Yes	Automatic	2	3	51
Canada	68	93	No	Automatic	4	1	29
Denmark	85	98	No	Automatic	4	4	43
Finland ^c	82	100	No	Automatic	2	3	48
France (presidential election)	78	91	No	No	3	4	33
West Germany ^d	85	94	No	Automatic	2	4	34
Ireland	77	100	No	Automatic	4	2	25
Israel	80	100	No	Automatic	2	4	—
Italy	94	100	Yes	Automatic	2	3	40
Japan	72	100	No	Automatic	1	2	30
Netherlands	82	98	No	Automatic	2	4	45
New Zealand	83	95	No	Yes	3	1	40
Norway	82	100	No	Automatic	3	3	37
Sweden	88	97	No	Automatic	3	4	42
Switzerland	44	85	No	Automatic	1	3	43
United Kingdom	75	100	No	Automatic	4	1	33
United States (presidential election)	54	61 ^e	No	No	4	1	13
Spain	78	100	No	Automatic	—	3	42

Sources: For registered and voted, Mackie and Rose (1982) and *European Journal of Political Research*. Age-eligible population calculated from United Nations Demographic Yearbooks, 1979, 1981, 1983 (based on census data, interpolated to election year).

^a4 = Chief executive changed party hands 50% of elections 1960–80; 3 = clear changes, but under 50%; 2 = partial changes only; 1 = no changes in parties controlling chief executive.

^b4 = national election PR or national pool for some legislative seats, or simple national presidency vote; 3 = large district PR; 2 = PR with three to five members per district; 1 = single member or winner-take-all districts.

^cExcludes Finns living outside of Finland from voted, registered and eligible.

^dExcludes West Berlin (not eligible to vote in West German elections).

^eEstimated from University of Michigan Survey Research Center study; see Katosh and Traugott, 1981.

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**Appendix 2. A Comparison of Three Models of Multivariate Analysis of
Voter Turnout in Six Nations:^a Regression, LOGIT, PROBIT**

Independent Variables	Slope Coefficients			Predicted Turnout (%)			Simple Turnout (%)
	Regression	LOGIT	PROBIT	Regression	LOGIT	PROBIT	
Intercept	.347	4.15	4.08	84	84	84	84
<i>Education^b</i>							
Basic	—	—	—	83	83	83	85
Lower	-.011	-.009	-.016	82	83	83	82
Extended Lower	-.003	.021	.014	83	83	83	82
Middle	.026	.137	.139	86	86	86	83
Post Secondary	.039	.206	.208	87	87	87	88
<i>Age</i>							
20-25	—	—	—	66	67	66	65
26-29	.113	.342	.402	77	77	77	77
30-39	.173	.581	.662	83	83	83	84
40-49	.221	.787	.887	88	88	88	88
50-59	.218	.783	.890	88	87	88	87
60+	.217	.785	.895	87	88	88	87
<i>Party ID</i>							
No	—	—	—	73	74	74	68
Yes	.142	.516	.583	87	87	87	88
<i>Interest</i>							
Not at All	—	—	—	70	72	72	66
Not Much	.118	.353	.404	82	82	82	79
Somewhat	.189	.671	.747	89	89	89	90
Very	.197	.805	.863	90	91	90	94
<i>Efficacy</i>							
Low	—	—	—	82	82	82	81
Mixed	.024	.107	.123	85	85	85	86
High	.033	.148	.174	86	86	86	88
<i>Electoral</i>							
SMD	—	—	—	75	74	74	81
Multi-District PR	.112	.522	.575	87	86	86	75
National PR	.125	.628	.685	88	88	88	92
<i>Switzerland</i>							
No	—	—	—	87	87	87	87
Yes	-.250	-.882	-.997	62	62	62	62

^aPooled data from Britain, West Germany, Netherlands, Switzerland, Finland and Canada. N = 7191. Excludes cases missing on any variable. LOGIT and PROBIT from MVS/OS version of SPSSX; the program adds 5 to the PROBIT and divides by 2 and adds 5 to LOGIT; account is taken of this in the predictions.

^bSee the note to Table 3.

Appendix 3

The problem of fair comparison of voting turnout figures raises many theoretical and technical issues. One of these is worth special comment here: alien residents in the population. In most democracies voting eligibility is limited to citizens. Population figures, on the other hand, usually include noncitizens resident in the country for a year or more. Countries vary substantially in the percentage of such aliens, largely depending on the presence of jobs and ease of access to draw alien workers and their families, but also depending on their naturalization policies in denying or facilitating attainment of citizenship by resident aliens. If we accept the legitimacy of the denial of voting eligibility to aliens, then the countries with substantial alien populations are penalized in column 2 of Appendix 1. Unfortunately, we do not have good data on percentage of residents counted by census who are aliens of voting age, and cannot systematically adjust our turnout data to remove them. For the United States we do have census estimates based on self-reported citizenship. This percentage probably understates noncitizenship; we do not know by how much. The census study figures (U.S. Bureau of Census, 1978, 1982) indicate 3-4% of the population of voting age in the United States were noncitizens in the 1970s. Adjusting the U.S. turnout figures to exclude these noncitizens would increase turnout of age-eligible Americans by 2%—closing only slightly the gap between American turnout and the democratic

average. As we can see in Appendix 1, this adjustment would not alter the position of the U.S. relative to most other democracies.

Moreover, while it is clear that some countries in Appendix 1 have fewer aliens than the United States, figures for West Germany, France, Belgium, and Switzerland indicate that they had more aliens in the resident population than did the U.S. (Britain also had more, but special provisions affecting immigrants from the Commonwealth enfranchised many of them rapidly.) The best figures and most aliens were for Switzerland, where the foreign worker population increased from 10% in 1960 to 16% in 1970. Moreover, a 1975 study indicated that 80% of these Swiss immigrants had lived in Switzerland for over seven years, although very few were able to become citizens under the difficult Swiss rules, which include a twelve-year residency requirement (Miller, 1981; Castles and Kosack, 1973; Krane, 1979). Excluding aliens from both Switzerland and the U.S. brings Swiss turnout nearly to the American level, and accounts for virtually all of the Swiss unregistered population of voting age. Thus, the resident alien problem does not seem to be a serious problem in American comparisons with other democracies, although Switzerland is rather sharply affected. (The slight decline in turnout as percent of resident age population in West Germany in the 1970s may well also be caused by foreign workers.)

Notes

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1. For average gammas between attitude variables and turnout in the United States between 1960 and 1976, see Miller, Miller, and Schneider (1980). The correlation between trust and turnout is .12, compared to .37 for efficacy and .50 for interest.

2. See Niemi, Stanley and Evans (1984), and the references therein. For sake of simplicity, I am here ignoring the slight turnout decline among the oldest age groups.

3. See Crewe (1981) and Powell (1980) for alternative measures. See Powell (1980) for most sources used to estimate party-group linkages. These data were extended with data from the political action surveys and Euro-barometer (Rabier and Inglehart, 1981) study cited above, and with calculations from tables appearing in Flanagan (1984), Holmberg (1981), Levine and Robinson (1976), Linz (1980), and Valen (1979).

4. The one demographic group in the United States with highly differential party support is black Americans. I have not included black support in the American party-group linkage calculations for two reasons. First, because groups as small as 10% of the population are not included in the analysis for other countries, and would increase the linkage numbers in most countries if they were. Secondly, although Verba et al. (1978, ch. 10) and others have shown that blacks do overparticipate as a social segment, relative to expectations from their socioeconomic resources, they seem relatively unmobilized considering their potential value to the Democratic Party. While this may be changing in 1984, it suggests that party-group linkages depend on party mobilization efforts to take advantage of differential group linkages, and are not purely individual-level phenomena.

5. For a more extensive discussion of the Swiss case see Powell (1980) and Appendix 3.

6. See the very comparable relationships between socioeconomic resources and "political involvement" reported by Verba et al. (1978, p. 75) in their seven rather different nations, and the marked contrast they, too, found with voting participation.

7. See also Almond and Verba (1963) and Verba et al. (1978, ch. 12) for similar findings on differences between males and females.

8. Measurement of partisanship is a matter of current controversy too complex to review here. In fact, it seems surprising that the results for partisanship and turnout are as consistent as they are, given the different meanings and stability associated with partisanship across countries. But the coefficients in Table 4 do show some notable differences in the

impact of party identification on turnout. Measurement problems may well be responsible. The present analysis has been replicated using a measure of partisan intensity rather than a simple dichotomous partisan identification variable, with very similar results. In the case of Canada, it should be noted that the general political interest question was not available in the Canadian study. A question about interest in the campaign was used instead, which had a particularly weakening effect on the original Canadian party identification coefficient. However, that coefficient was well below average even without the other attitudinal variables. Efforts to use a provincial or combined party identification measure for Canada, rather than the federal party identification used here, did not change the results.

9. For similar results in a different study, using different surveys, see Verba, Nie, and Kim (1971, pp. 75-79).

10. American voting research suggests that citizen duty is the best predictor of voter turnout, and has not changed greatly in magnitude or power over the past 20 years (Miller et al., 1980). I have not been able to find comparative studies using citizen-duty variables in other countries. The Almond-Verba study (1963) does examine responses to a question on obligations that citizens owe their countries. Americans and Germans report rather similar frequencies of mentioning voting as an obligation—levels higher than the British, Italian, and Mexican respondents. It seems unlikely that duty is a more powerful mobilizer in Europe than in the United States.

11. It must be emphasized repeatedly, of course, that other, unmeasured, attitudinal and institutional variables may account for part of the difference. We have, for example, no measure of citizen mobility. Although its effects should be less under conditions of automatic registration, we would still expect less turnout from the highly mobile. This and other unknown factors are included in what here is ascribed to registration laws. For that reason the estimate may well be on the high side.

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PRESIDENTIAL COATTAILS AND MIDTERM LOSSES IN STATE LEGISLATIVE ELECTIONS

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The president's party consistently loses partisan control of state legislatures in midterm elections; a pattern similar to the loss of seats in the U.S. House of Representatives in midterms. This study examines presidential coattails as a possible explanation of these losses. Aggregate state legislative election outcomes between 1944 and 1984 in 41 states are examined. The analysis indicates that the president's party gains seats in presidential elections in proportion to the presidential vote in a state, and subsequently loses seats in midterm elections also in proportion to the prior presidential vote in the state. The presidential coattail and the midterm repercussion effects are evident even when gubernatorial coattail effects are introduced, but are fairly modest in states lacking competitive parties.

Despite their number and importance, state legislative elections have not received a great deal of scholarly attention. One recent exception to this neglect is John Bibby's study of state legislative midterms (1983a, 1983b). Bibby found a definite pattern in midterm changes in partisan control of state legislatures. The president's party regularly loses control of state legislatures at the midterm, much as it loses seats in the U.S. House of Representatives. In each of the 9 midterms from 1950 to 1982, the presidential party suffered a net loss in the control of state legislative chambers. These losses ranged from 8 chambers in 1970 to 24 chambers in 1974.

This analysis proposes to extend and

explain Bibby's findings. First, this study will extend Bibby's analysis of changes in control of the state legislature by examining changes in the percentage of seats held by the president's party. Bibby's findings regarding changes in partisan control are quite probably just one manifestation of a more general loss of seats for the president's party. Second, the study's main purpose is to determine if presidential coattails explain midterm losses. Other theories of presidential midterm losses in Congress have been suggested, notably Tufte's referendum on economic conditions and presidential popularity thesis (1975, 1978). However, the coattails theory (Bean, 1948; 1950; 1972, pp. 50-60) and, in its more sophisticated

form, Campbell's "surge and decline" theory (1966) have been shown to go a long way toward explaining the pattern of midterm loss in Congress (Campbell, 1985). The coattails thesis is that in a presidential election year a successful presidential candidate assists in the election of his party's slate of candidates, whether they be candidates for the U.S. House of Representatives or for state legislative seats. At the midterm many of these candidates, boosted into office in the prior election with help from the top of the ticket, lose when they run for reelection without the benefit of presidential coattails.

One might argue that presidential coattail effects in state legislative contests don't make sense. State legislators and presidents deal at different levels of government and for the most part are concerned with different issues. Nevertheless, parties are generally known by the presidential candidates they nominate, and candidates for state legislative seats are a good deal less well known to voters than the congressional candidates who ride presidential coattails. Therefore, even though one can make a case that there may not be good policy reasons for coattails to affect state legislative races, many voters may use their presidential vote as a guide in casting a vote for the state legislature because they lack other information (Hinckley, Hofstetter, and Kessel, 1974).

The analysis is divided into four major sections. The first section examines state legislative seat changes in presidential election years. The central question in this portion of the analysis is, to what extent do presidential coattails extend to state legislative elections? The second section examines state legislative seat changes in midterm elections. Again, the central question concerns the possible repercussions of presidential politics for state legislative elections: To what extent do seat changes at the midterm reflect the

removal of presidential coattails and the return to normal partisan politics?

Within each of these two sections the analysis will proceed in two steps. First, the extent of seat gains or losses for the president's party will be examined for all states in both presidential and midterm years. The distribution of gains and losses should provide an initial indication of whether coattails exist at the state legislative level, and whether Bibby's findings are a manifestation of a pattern of state legislative seat loss for the president's party at the midterm. Second, models will be constructed to account for seat changes in both presidential and midterm elections. At the state level the models will be examined with aggregate state election results over a period of elections. The state-by-state analysis is designed to control for the numerous idiosyncracies of the states that might otherwise mask the more general effects.

The third section of the analysis involves an examination of variations in coattail and midterm effects from state to state. Is there systematic variation, and what are its sources? In particular, do differing levels of party competition account for differences in presidential coattail effects and their midterm repercussions. The fourth section compares presidential coattails to gubernatorial coattails in state legislative elections.

The Data

Data are drawn at the state level from the 11 presidential elections and the 10 midterm elections from 1944 to 1984. Two states, Nebraska and Minnesota, are excluded because their state legislatures were nonpartisan for at least half of the time period under study. An additional 7 states are set aside because of peculiarities in election scheduling. Four of these states—Alabama, Louisiana, Maryland, and Mississippi—have four-year terms rather than the usual two-year terms for state

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legislators (Council of State Governments, 1968, p. 49). Mississippi and 3 other states—Kentucky, New Jersey, and Virginia—hold their state legislative elections in odd-numbered years rather than the standard even-numbered years (Jewell and Olson, 1982, p. 16). Two states, Alaska and Hawaii, have been included although data on only 6 midterm and 7 presidential elections are available. The active data set covers a total of 443 presidential elections and 402 midterm elections in 41 states.

Since the analysis is concerned with on-year as well as off-year elections, there are two principal dependent variables. The first is coattail seat gain.¹ This is calculated as the percentage point gain in the seats held by a party before and after the presidential election. For example, if the Democratic party held 30% of a state legislature's seats before a presidential election, and won 40% of those seats in the election, the seat gain for that party would be 10 percentage points. The second dependent variable is midterm seat loss. The midterm seat loss variable measures the change in seats held by the president's party in the midterm election. Like the coattail variable, the midterm seat loss variable is calculated in terms of percentage point differences. The partisan compositions of the state legislatures were extracted from various volumes of the Council of State Government's *The Book of the States* (Council of State Governments, 1942–1982) and from *Congressional Quarterly Weekly Reports* (Donnelley, 1984).

A number of independent variables have also of course been collected. The principal independent variable is the percentage of the two-party statewide vote won by each of the two major presidential candidates in the presidential election. In addition, election results for gubernatorial races have also been collected in both presidential and midterm years in states holding these elections. Election

returns were obtained from *Presidential Elections Since 1789* (Congressional Quarterly, 1975) and various issues of the *Congressional Quarterly Weekly Report* (Congressional Quarterly, 1981, 1984; Cook, 1976).

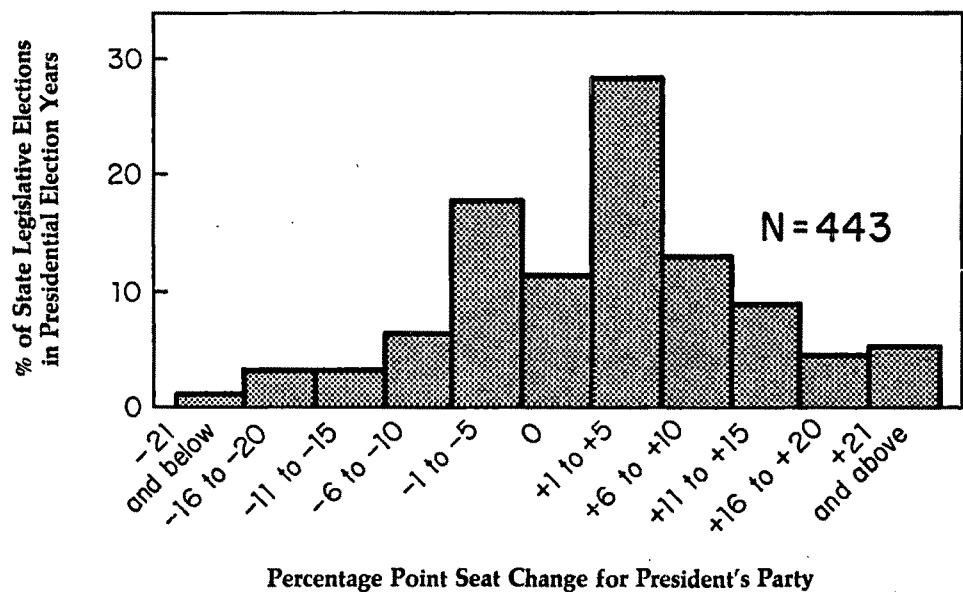
Seat Change in Presidential Elections

As expected, the party winning the presidency also tends to do well in state legislative contests held during presidential election years. The distribution of seat changes in state legislatures after presidential elections is skewed in favor of the party of the winning presidential candidate, much as one would expect if coattails were at work. Figure 1 is a histogram of the frequency of gains and losses for the president's party in presidential elections.

The party winning the presidency is nearly twice as likely to win seats in state legislatures as it is to lose seats. The winning presidential party gained at least 1% of the state legislative seats in 58% of the state elections examined (257 of 443). It lost 1% or more of the seats in 31% of the elections. There was no change in the remaining 11%. The mean seat change in all elections examined was a 3.2% gain for the winning presidential party. While this is a fairly definite pattern consistent with presidential coattails, an even stronger pattern may emerge in a multivariate model addressing less aggregated data. Presidential party losses may well be concentrated in states the candidate winning nationally failed to carry.

A single-equation model was constructed to examine state legislative seat changes in presidential elections more thoroughly. The dependent variable is simply the percentage point change in the seats held by the Democratic party after the presidential election. There are two major independent variables. The first is the percentage of the two-party presiden-

Figure 1. Net Change in the Percentage of State Legislative Seats for the President's Party Following Presidential Elections



tial vote won by the Democratic presidential candidates. This is the coattail variable. The presumption is that the stronger the presidential candidate runs in a state, the more help he is to the bottom of his party's slate. Although other measures of the coattail variable have been suggested (Calvert and Ferejohn, 1983; Miller, 1955), the simple presidential vote variable has served quite adequately in previous studies exploring its effect in congressional elections (Campbell, 1985, 1986). The second major independent variable is the Democratic party's strength in the state legislature going into the election. If a party already holds a large percentage of the seats, it should be more difficult to add to that large base. It is a simple arithmetic fact of life that you cannot gain what you already have. Parties having a large initial base are also confronted with the likelihood of diminishing returns. It is likely to be more difficult to win an additional seat

if you already hold 99% of the seats than it is if you only hold 30%. The dependent variable and both of the major independent variables are oriented in terms of the Democratic and Republican parties, rather than in terms of the winning and losing parties, in order to capture the full variance of the variables. Orienting the analysis according to the winning or losing parties would artificially restrict variance and attenuate measures of association.

A third independent variable is also introduced into the analysis as a trend variable. Since the data set spans four decades, a great many changes in state and local politics may have occurred that could affect seat changes. For instance, a state's partisan balance may shift over time. It would be very difficult to obtain individual measures of these changes and, given the small number of cases for each state, would be even more difficult to incorporate such variables in the model.

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As such, the analysis uses a simple counter variable to take trends into account. The variable is simply the last two digits of the presidential year. Since this variable is meant to control as much as possible for trends that might exist in any given state, and since it is not of any theoretical interest in and of itself, it is only included when it has a statistically significant effect.

Table 1 presents the regression results for each of the 41 states examined. On the whole the model appears to be fairly strong. The model accounts for 63% of the variance in the median state and for more than half the variance in 29 of the 41 states. Moreover, the two main independent variables have their expected effects.

The regressions present pretty convincing evidence that presidential coattails reach down to carry state legislative candidates into office. The coefficient of the presidential vote is positive in 39 of the 41 regressions. This pattern is, of course, extremely unlikely to occur by chance alone.² Moreover, even though the analysis could not take into account very many idiosyncracies associated with particular elections in particular states—idiosyncracies likely to blur the true impact of presidential coattails—the presidential vote coefficient is statistically significant ($p < .05$) in more than half of the regressions (22 of 41).

The actual strength of presidential coattails varied a good bit. While small negative coefficients were found in the examination of 2 states, positive coefficients exceeding 1.0 were found in 7 states. The median state had a coefficient of .49. That is, about every 2 additional percentage points of the two-party presidential vote won by a party meant a net gain of 1% of the state legislative seats for that party. Moreover, the presidential vote in states commonly varies by much more than 2 percentage points. The standard deviation of the Democratic presidential vote of the median state is 9.2

percentage points. Thus, in a typical state, a change of one standard deviation in the presidential vote can be expected to precipitate about a 4.5% change in the partisan composition of the state legislature.

Some perspective on the magnitude of these effects can be gained by comparing them to coattail effects in congressional elections. A previous study (Campbell, 1986) found, in elections from 1944 to 1980, that a party could expect a net gain of about 3.2 seats in the House of Representatives for every percentage point gained by the party's presidential candidate. In percentage terms, this translates into a coefficient of .74, only slightly more than the median coattail effect in state legislative contests. Thus, by present estimates, presidential coattails at the state legislative level are only slightly shorter than at the congressional level.

The second and less interesting independent variable, the initial base of the party in the state legislature, also had the expected effect. Negative coefficients were associated with the base variable in 39 of the 41 regressions. The negative base effects were statistically significant ($p < .05$) in 23 of the 41 tests, despite the small number of cases. The median state had a base coefficient of $-.58$. In less precise terms, holding a large proportion of the seats in a state legislature made gaining additional seats substantially more difficult.

Seat Change in Midterm Elections

According to Bibby's analysis and the coattail model, the pattern of seat changes in midterm elections should be the mirror image of seat changes in presidential years. The president's party should lose state legislative seats at the midterm, as the president's coattails are no longer available to state legislative candidates of the president's party.

**Table 1. The Coattail Model of Change in State Legislative Seats
in Presidential Elections, 1944-1984**

State	Intercept	Presidential Vote	Initial Base	Trend	R ²
Alaska	-.63	.98 (11.29)	-.90 (48.73)	.0105 (5.26)	.98
Arizona	-.13	.61 (2.66)	-.29 (3.65)	—	.31
Arkansas	.08	-.07 (2.63)	.15 (0)	-.0009 (3.36)	.40
California	.04	.01 (0)	-.09 (.29)	—	.03
Colorado	-.14	1.05 (7.82)	-.64 (8.48)	—	.70
Connecticut	-.21	.55 (3.06)	-.29 (3.87)	—	.53
Delaware	-.24	1.11 (6.09)	-.57 (10.60)	—	.64
Florida	-.05	.23 (3.19)	-.08 (.56)	—	.31
Georgia	.15	.16 (6.83)	-.27 (2.16)	—	.46
Hawaii	.03	.48 (16.93)	-1.32 (40.72)	.0098 (19.76)	.94
Idaho	-.10	.66 (8.42)	-.46 (3.36)	—	.52
Illinois	-.12	.96 (20.40)	-.98 (8.93)	.0026 (3.62)	.81
Indiana	-1.22	2.75 (47.11)	-1.23 (77.80)	.0083 (13.90)	.95
Iowa	-1.46	2.23 (53.75)	-1.20 (22.21)	.0142 (15.33)	.91
Kansas	-.11	.36 (1.73)	-.09 (.28)	—	.21
Maine	-.67	.35 (3.17)	-1.19 (9.47)	.0151 (7.79)	.70
Massachusetts	-.25	.24 (5.57)	-.13 (3.58)	—	.53
Michigan	-.39	.83 (16.31)	-.92 (34.79)	.0072 (12.25)	.89
Missouri	-.30	.89 (16.67)	-.91 (25.18)	.0067 (12.22)	.89
Montana	-.30	.40 (.90)	-.97 (9.41)	—	.63
Nevada	.20	.49 (1.92)	-.62 (8.20)	—	.55
New Hampshire	-.19	.46 (15.94)	-.03 (0)	—	.69

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Table 1 (continued)

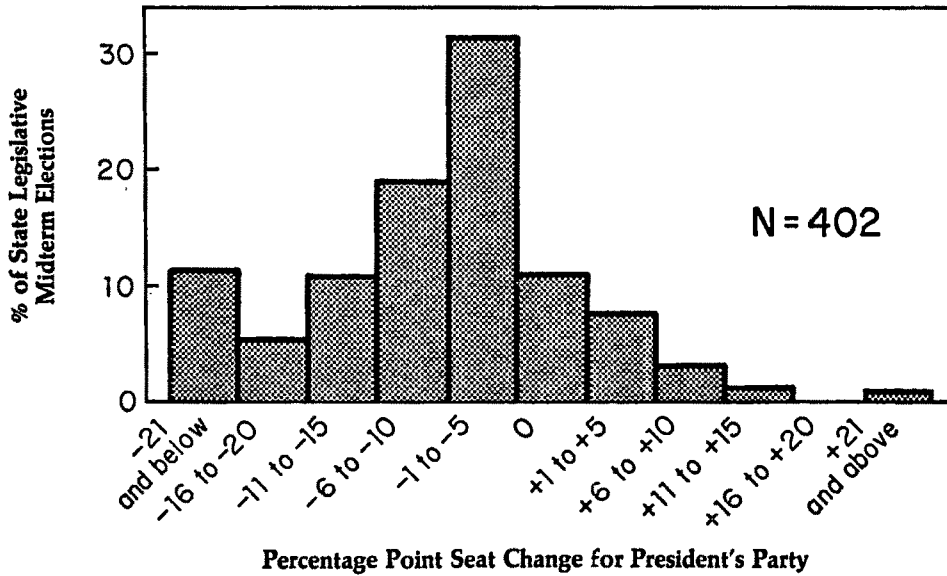
State	Intercept	Presidential Vote	Initial Base	Trend	R ²
New Mexico	.30	.26 (.36)	.68 (6.71)	—	.46
New York	— .22	.54 (15.28)	—1.12 (24.25)	.0077 (16.30)	.85
North Carolina	.30	.49 (8.03)	— .66 (3.35)	—	.50
North Dakota	— .95	1.34 (14.29)	— .77 (9.53)	.0095 (6.46)	.70
Ohio	— .05	.29 (.47)	— .20 (1.34)	—	.24
Oklahoma	.06	.19 (6.70)	— .19 (1.29)	—	.47
Oregon	.25	— .29 (.66)	— .19 (2.53)	—	.30
Pennsylvania	.08	.54 (11.12)	—1.00 (62.80)	.0024 (5.16)	.93
Rhode Island	.08	.29 (3.76)	— .33 (3.81)	—	.53
South Carolina	.68	.03 (.12)	— .89 (.13)	—	.02
South Dakota	— .23	.78 (2.78)	— .39 (2.79)	—	.37
Tennessee	.05	.37 (5.50)	— .35 (8.74)	—	.54
Texas	— .43	.14 (3.66)	.36 (9.55)	—	.76
Utah	— .11	1.03 (29.11)	— .67 (19.18)	—	.81
Vermont	— .26	.15 (2.65)	— .43 (7.76)	.0052 (7.67)	.63
Washington	.33	.63 (2.91)	—1.13 (21.93)	—	.77
West Virginia	— .14	1.12 (18.66)	— .58 (9.55)	—	.77
Wisconsin	— .08	.37 (2.58)	— .21 (6.76)	—	.54
Wyoming	.01	.79 (6.58)	— .87 (7.43)	—	.66

Note: The coefficients are unstandardized regression coefficients. The numbers in parentheses are *F*-values.

The evidence supports our hypothesis. There is a strong tendency for the president's party to lose seats at the midterm. A histogram of gains and losses for the president's party in midterm elections is presented as Figure 2. In more than 3 out

of 4 (308 of 402) state midterm elections examined, the president's party suffered a net loss of at least 1% of the seats. The president's party made gains in only 13% of the midterm elections, and no change was observed in the remaining 11%. Put

Figure 2. Net Change in the Percentage of State Legislative Seats for the President's Party Following Midterm Elections



somewhat differently, the president's party is more than six times as likely to lose seats as it is to win seats. The mean seat change in all midterms examined was a 7.3% loss for the president's party. Of course, as in the case of seat losses in presidential elections, this level of analysis is a pretty rough cut of the data. The repercussions of presidential coattails for midterm elections are better examined in a less aggregated and multivariate analysis.

The single-equation model of midterm seat loss is quite similar to the coattail model for presidential elections. The dependent variable is a simple change variable computed by subtracting the percentage of state legislative seats held by Democrats after the presidential election from the percentage held after the midterm election. The independent variables in the midterm equation are identical to those used in the presidential election equation: two major independent varia-

bles, and a third independent variable introduced to control for trends favoring either of the parties. As in the presidential election equation, the first independent variable is the percentage of the presidential vote won by the Democrats. There is, however, one very important difference from the presidential election analysis: Whereas the effect of the presidential vote on seat changes for a party is hypothesized and shown to be positive in presidential elections, it is hypothesized to be negative in midterm elections. State legislative candidates receiving more help from the top of the ticket in presidential years have more help to lose in the following midterm election.

The second independent variable is the same Democratic base variable used in the presidential election equation, the percentage of seats held by the Democratic party prior to the last presidential election. The selection of this variable posed a problem. The logical choice of a base

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variable for the midterm equation is the percentage of seats held prior to the midterm, rather than the percentage held prior to the previous presidential election. However, because seat gains at the presidential election are dependent on the presidential vote, the base variable following the presidential election is quite often highly correlated with the presidential vote. The collinearity problem, particularly when we are dealing with so few cases, can be substantial, and the coefficient estimates are often unstable or insignificant as a result. For this reason, the pre-presidential election base is substituted for the post-presidential election base in the regressions.³

The third independent variable is a trend variable identical to that used in the presidential election equation. It is simply a counter variable entered to take trends over this period into account. Regressions including the trend variable are only reported when it is statistically significant. Otherwise the regressions do not include the trend control.

Table 2 presents regression results for the midterm model. The model accounts for a good bit of the variance in midterm seat change, though it generally performed less well than the presidential election equation. The equation accounts for 49% of the variance in the median state. One reason for the somewhat lower predictive power of this equation is the use of the pre-presidential base instead of the post-presidential election measure. Using the post-presidential base, the equation accounts for 58% of the variance in the median state, and for more than half of the variance in 27 of the 41 states. This performance is on the same order as that of the presidential election equation.

The regressions indicate that midterm seat changes are in fact a negative function of the previous presidential vote in the state. Parties lose seats at the midterm in an inverse proportion to how well their presidential candidate did in the state in

the previous election. The withdrawal of presidential coattails significantly affects midterm results. The pattern is quite consistent. The coefficient of the presidential vote is negative in 39 of the 41 cases. It is also statistically significant ($p < .05$) in half of the cases (21 of the 41), despite the fact that a variety of considerations having the possibility of distorting the relationships could not be entered into the equation because of the limited number of cases.

As with the initial coattail effects, the strength of midterm coattail repercussions varies considerably. The latter effects range from the slightly positive coefficients found in North Carolina and Vermont to coefficients of -1.0 or less in 18 different states. The coefficients are less than -2 in 5 states. The median coefficient for the presidential vote is $-.67$. When the post-presidential election base is used, the presidential vote coefficients are slightly smaller. The median presidential vote coefficient in these regressions is $-.52$.

As in the case of presidential-related gains in presidential elections, presidential-related losses at the midterm can be set in some context by comparison to congressional election patterns. In midterm elections the president's party loses about 3.2 seats in the House of Representatives for every additional percentage point won by the president in the prior presidential election (Campbell, 1985). In percentage terms, a loss of 3.2 seats out of 435 seats translates into a coefficient of $-.74$, just slightly more negative than the median coefficient for coattail removal losses in state legislative midterms.

A second point of comparison for presidential-related midterm losses is, of course, the presidential coattail effect of the prior election. The negative presidential vote coefficients in midterm elections are of about the same magnitude as the positive presidential vote coefficients in presidential election years. The symmetry

Table 2. The Coattail Repercussion Model of Change in State Legislative Seats in Midterm Elections, 1946-1982

State	Intercept	Presidential Vote	Initial Base	Trend	R ²
Alaska	.52	-1.04 (1.64)	-.13 (.04)	—	.36
Arizona	.25	-1.10 (14.05)	.33 (7.00)	—	.68
Arkansas	.09	-.02 (.22)	-.78 (.06)	—	.05
California	.47	-.67 (4.67)	-.28 (1.93)	—	.51
Colorado	.68	-1.48 (10.49)	-.19 (.51)	—	.60
Connecticut	-.53	-.39 (.36)	-1.31 (3.37)	.0231 (3.70)	.41
Delaware	-1.33	-2.86 (9.89)	.16 (.02)	—	.59
Florida	.12	-.30 (.88)	-.86 (0)	—	.19
Georgia	.07	-.05 (7.77)	-.04 (.42)	—	.66
Hawaii	.22	-.16 (.21)	-.16 (.06)	—	.13
Idaho	.30	-.55 (2.89)	-.16 (.19)	—	.37
Illinois	.65	-1.53 (74.45)	.12 (.23)	—	.91
Indiana	1.50	-3.54 (14.51)	.18 (.38)	—	.71
Iowa	1.23	-2.76 (151.05)	-.03 (.07)	—	.96
Kansas	.35	-.62 (6.33)	-.30 (4.11)	—	.59
Maine	.25	-.54 (7.64)	0 (0)	—	.52
Massachusetts	.07	-.13 (2.60)	.02 (.14)	—	.28
Michigan	.36	-.95 (3.45)	.14 (.42)	—	.37
Missouri	.26	-.63 (2.18)	.12 (.26)	—	.36
Montana	.92	-1.65 (49.72)	.08 (.29)	-.0034 (5.98)	.91
Nevada	.24	-.28 (.58)	-.26 (1.60)	—	.24
New Hampshire	.37	-.30 (14.28)	-1.22 (14.72)	.0035 (11.52)	.86

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TABLE 2 (continued)

State	Intercept	Presidential Vote	Initial Base	Trend	R ²
New Mexico	1.53	-1.62 (5.48)	.13 (.13)	-.0128 (7.69)	.60
New York	.11	-.51 (13.30)	-.38 (2.53)	.0051 (6.90)	.79
North Carolina	.16	-.52 (3.22)	.13 (.05)	—	.41
North Dakota	.77	-1.77 (9.77)	-.14 (.24)	—	.63
Ohio	.56	-1.05 (2.96)	-.11 (.19)	—	.30
Oklahoma	.28	-.05 (.12)	-.32 (.96)	—	.14
Oregon	.51	-1.06 (7.55)	-.02 (.03)	—	.53
Pennsylvania	.25	-.85 (2.68)	.30 (.96)	—	.34
Rhode Island	.23	-.50 (10.91)	.09 (.22)	—	.62
South Carolina	-.07	.02 (.04)	.04 (.03)	—	.01
South Dakota	.49	-1.05 (4.10)	-.14 (.31)	—	.41
Tennessee	.23	-.43 (2.29)	-.03 (.02)	—	.33
Texas	.65	-.11 (3.90)	-.45 (4.73)	-.0026 (5.60)	.49
Utah	2.22	-2.43 (17.72)	-.49 (2.68)	-.0169 (7.67)	.80
Vermont	-.03	.17 (1.27)	-.06 (.29)	—	.17
Washington	.90	-1.51 (11.00)	-.39 (1.80)	—	.63
West Virginia	1.00	-2.10 (57.94)	-.17 (.63)	—	.89
Wisconsin	.53	-1.03 (6.38)	-.02 (.02)	—	.48
Wyoming	.45	-1.08 (5.89)	0 (0)	—	.46

Note: The coefficients are unstandardized regression coefficients. The numbers in parentheses are *F*-values.

can be seen in a comparison of median coefficients, but is even clearer in comparisons of individual state coefficients. Where we find evidence of strong coattail effects in presidential election years, we tend to find evidence of equally strong

coattail repercussions at the midterm. Conversely, where we find evidence of weaker coattails, we also tend to find weaker coattail removal or repercussion effects. A more systematic comparison indicates how closely coattail gains and

subsequent coattail losses are related. The presidential vote coefficients from Tables 1 and 2 are themselves strongly correlated with one another ($r = -.80$). One may interpret this negative association to mean that presidential coattails offer little or no residual benefits for a party. The benefits in the presidential election last only until politics returns to normal at the midterm.

As in the presidential election regressions, the base, the second of the principal independent variables, had a negative effect on seat change. As expected, however, the effect was somewhat stronger and more consistently negative when the post-presidential election base was used. The pre-presidential election base had a negative effect in 37 of the 41 regressions and was statistically significant ($p < .05$) in eight cases. Recall that the pre-presidential base has been used in Table 2 to reduce collinearity with the presidential vote variable.

Variations Among the States

The effects and midterm repercussions of presidential coattails on state legislative outcomes, though evident in most states, varies a good bit from state to state. Given the strong correlation ($r = -.80$) between the coefficients for the presidential year and the midterm, there is good reason to believe the state-to-state variation is systematic. The source of this systematic variation is somewhat less clear. There are a number of possibilities. Particularly heterogeneous states may be less uniform in their support for a presidential candidate, and consequently appear less responsive to the average vote in the state for that candidate. The very weak effects found in California, for instance, may be traceable to the very heterogeneous nature of the state. The varying sizes of state legislative districts, the use of multimember districts, and the drawing of district boundaries may dampen or accentuate coattail effects, particularly as they vary within a state

over four decades. The popularity of the president at the midterm can certainly accentuate or depress repercussion effects. Secular trends in a state, untapped by the crude trend indicator in the equations, may also complicate matters. Large scale immigration into western states over this period may be one such trend. Of the many potential sources of interstate variation, two in particular will be examined: interparty competition and ballot form.

Party Competition

States with competitive party systems should respond more strongly to fluctuations in presidential politics than states dominated by a single party. No presidential candidate, no matter how popular, can help his party capture state legislative seats unless his party actively contests those seats. Two measures of party competition are used to test the proposition of the relationship of party competition to coattail effects.⁴ The first measure is based on the mean percentage of state legislative seats held by a party over the period from 1944 to 1982. This competitiveness index ranges from a value of 100 in states where the parties evenly split the state legislature to a value of 50 in states completely dominated by a single party. The second party competition measure taps the volatility of state legislative elections. This second measure is the standard deviation of the distribution of state legislative seats in all elections in this period.

According to both measures of party competition, there is a relationship between coattail effects, both presidential (Table 1) and midterm (Table 2), and state party competition. Both measures are positively correlated with coattail coefficients in presidential elections ($r = .31$ and $r = .34$, respectively) and negatively correlated with coattail repercussion coefficients in midterm elections ($r = -.46$ and $r = -.45$, respectively). An inspection of the scatterplots of both coefficients against the party competition measures

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reveals a bit more about the relationship. The plots suggest substantial heteroscedasticity. Coefficients vary a good bit among competitive states but are uniformly small in noncompetitive states. The 7 least competitive states of the 41 examined are Arkansas, Florida, Georgia, North Carolina, Oklahoma, South Carolina, and Texas. Each had a score of .7 or less on the mean percentage indicator of party competition. Only one of these 7 noncompetitive states (North Carolina, .49) had a coattail coefficient greater than .25, and only 1 (again North Carolina, -.52) had a coattail repercussion effect more negative than -.30. The range of coefficients among the remaining more competitive states is substantial. Among the 15 most competitive states—those having a score of .9 or more on the mean percentage indicator of party competition—coattail effects ranged from a high of 2.75 in Indiana to a low of -.29 in Oregon. Coattail repercussion coefficients were similarly dispersed, ranging from a strong effect of -3.54 in Indiana to a weak effect of -.39 in Connecticut. In other words, party competition is a necessary but not a sufficient condition for strong coattail effects. In competitive states a number of factors previously mentioned (e.g., heterogeneity) may mask the coattail effects in individual state legislative district contests.

Ballot Form

Coattail effects ought to be stronger in states with ballot provisions for a straight party vote (Weber and Parent, 1985, p. 26). Generally, such a provision is included in the party-column ballot, but not all forms of the party-column ballot have the single mark option for the straight party vote. On the basis of nine reports in the Council of State Governments' *Book of the States* between 1943 and 1982, the 41 states in this study were grouped by their provision for a straight party vote. Fourteen of the 41 states

changed their straight party vote provisions during this period, and were consequently dropped from this portion of the analysis. An additional 3 states were dropped because they were noncompetitive as measured by the first party competition measure (a score of 70 or less). The remaining 24 states divided nearly evenly, 11 having no straight party voting provision, and 13 having the option.

As expected, coattail effects appear somewhat stronger in the states with the straight party vote option. The median coattail effect in states lacking a straight party vote ballot is .49. The median coattail effect coefficient in states with a straight party vote ballot is .83. An analysis of variance, however, indicates that the difference is not significant at the .05 level ($p = .17$).

Gubernatorial Coattails

As portrayed to this point, the dynamics of state legislative elections would appear to be entirely driven by presidential electoral politics. Of course this is not the full story. Local politics undoubtedly have some effect. Caldeira and Patterson (1982b) have shown that local campaigns have a significant influence on state legislative election results. Moreover, candidates other than those running for president, most notably gubernatorial candidates, may have coattails (Weber, 1980). To test further the reliability of the presidential coattail findings and to set those findings in context, gubernatorial coattails are estimated by inserting the Democratic gubernatorial two-party vote percentage in the presidential and midterm equations of state legislative seat change. The equations are estimated for the 8 states consistently holding gubernatorial elections throughout this period in presidential election years, and for the 30 states consistently electing their governors in midterm elections. Three states (Florida, Illinois, and North Dakota) are

eliminated because of their changes in the scheduling of gubernatorial elections. Also, because of the small number of cases per state, the trend variable is omitted. The regression estimates are presented in Table 3.

These regression estimates make three points relevant to our analysis. First, the regression results generally indicate that candidates for governor have coattails that extend to their parties' state legislative candidates. Positive gubernatorial effects were found in 3 out of 4 states (29 of 38). The median gubernatorial effect is .37; that is, a party should gain about 2% of the state legislative seats for every 5 additional percentage points won by its candidate for governor. While this effect is statistically significant in a minority of states (8 of 38)—perhaps because of the small number of cases—it would appear that a strong run by a party's candidate for governor helps the party win seats in the state legislature.⁵

Second, the previous estimates of presidential coattail effects and their repercussions withstand the introduction of gubernatorial coattails. The coefficients still fit the expected pattern of positive effects in presidential elections and negative effects in midterm years. The expected signs are found in more than 9 out of 10 states (35 of 38). It is true that there are significant differences between the coefficients for a few states shown in Table 3 and the comparable coefficients that were estimated without controlling for gubernatorial coattails. However, in 3 out of 4 states, the difference between the estimates is less than .3, and the two sets of estimates are quite highly correlated ($r = .90$ in presidential year cases, and $r = .83$ in midterm year cases). Moreover, the estimated coefficients in Table 3 are nearly as likely to be stronger than the previous estimate (16 v. 22) as they are to be weaker.

The final point to draw from these regressions concerns the relative strength

of presidential coattails and gubernatorial coattails. How do the presidential effects measure up against the gubernatorial effects on state legislative contests? The regressions suggest that the two effects are roughly equal in strength. The basis of this conclusion is a comparison of the absolute values of the standardized regression coefficients for the two variables in each of the 38 regressions. In the 8 states holding concurrent presidential and gubernatorial elections, presidential coattails appeared a bit stronger than gubernatorial coattails. Presidential effects were clearly stronger in 5 of the states, and very small differences (less than .05) were found in the remaining 3 states. In the states electing governors in midterm elections, half exhibited stronger gubernatorial coattails than presidential coattail repercussion effects, and half exhibited stronger presidential coattail repercussion effects than gubernatorial coattails.

Conclusion

There is a pulse to state legislative elections, a regular pattern of gains and losses very much like that observed in congressional elections. Moreover, the pulse is regulated by presidential electoral politics, again much like that of congressional elections. Presidential coattails extend to state legislative candidates. All things being equal, the change in a party's share of state legislative seats in presidential election years is proportional to the share of the vote won by its presidential candidate in the state. In presidential election years, a presidential candidate running a strong race in a state helps his party to gain additional state legislative seats. The opposite pattern is found in midterm elections. Whatever help the presidential candidate extended to his party's state legislative candidates in presidential election years is absent in the midterm election. Parties initially helped to significant

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**Table 3. Gubernatorial Coattail Effects on Changes in State Legislative Seats,
in Presidential and Midterm Elections, 1944-1982**

State	Intercept	Presidential Vote	Initial Base	Gubernatorial Vote	R ²
Presidential Elections					
Delaware	- .44	1.08 .50	- .56* - .71	.43 .22	.66
Indiana	- .92	1.64* .36	- .97 - .68	1.24 .20	.89
Missouri	- .16	.67 .43	- .45 - .53	.21 .10	.74
Montana	- .35	.70 .36	- .99* - .70	1.02 .37	.74
North Carolina	.32	.37 .89	- .65 - .76	.07 .12	.56
Utah	- .18	1.05* .85	- .66* - .65	.12 .09	.82
Washington	- .05	.54 .25	-1.28* - .91	1.05 .27	.83
West Virginia	- .37	.69* .47	- .74* - .65	1.04* .51	.89
Midterm Elections					
Alaska	.34	-1.12 - .67	- .06 - .05	.38 .20	.39
Arizona	.06	-1.11* -1.49	.34* 1.08	.36 .29	.76
Arkansas	.05	- .04 - .36	- .07 - .09	.06 .54	.30
California	.26	- .28 - .24	- .46* - .62	.29 .61	.72
Colorado	.88	-1.64* - .87	- .20 - .19	- .25 - .12	.61
Connecticut	-1.08	- .04 - .02	- .33 - .36	2.59* .93	.76
Georgia	.07	- .04 - .66	- .03 - .15	- .01 - .09	.66
Hawaii	1.72	.01 .12	-1.17 -1.04	-1.65 - .97	.44
Idaho	.45	- .68 - .67	- .15 - .13	- .20 - .18	.39
Iowa	1.40	-2.73* - .96	- .03 - .03	- .40 - .11	.97
Kansas	.02	- .65* - .64	- .42* - .70	.76 .46	.75
Maine	.35	- .55* - .74	.02 .05	- .22 - .20	.56

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Table 3 (continued)

State	Intercept	Presidential Vote	Initial Base	Gubernatorial Vote	R ²
Massachusetts	.15	-.19 -.77	.03 .18	-.12 -.32	.31
Michigan	-.81	-.12 -.07	.17 .25	1.60* .74	.68
Nevada	-.20	-.18 -.16	-.33* -.52	.83* .76	.80
New Hampshire	.15	-.40* -.83	-.37 -.28	.34 .42	.76
New Mexico	-.88	-.28 -.13	.13 .10	1.94 .32	.16
New York	-.10	-.30 -.41	.02 .03	.53 .48	.65
Ohio	-.48	-.18 -.10	.02 .02	1.19 .59	.44
Oklahoma	-.03	.07 .17	-.31 -.33	.45* .84	.76
Oregon	.27	-.98* -.66	-.11 -.21	.55 .38	.64
Pennsylvania	-.86	-.24 -.15	.18 .18	1.80* .71	.70
Rhode Island	.22	-.49 -.75	.08 .10	.02 .04	.62
South Carolina	.24	-.04 -.17	-.40 -.59	.18 .93	.34
South Dakota	.20	-1.30* -.74	-.22 -.26	.96* .62	.76
Tennessee	.22	-.44 -.57	.11 .18	-.14 -.28	.36
Texas	.14	-.07 -.52	-.20* -.67	.19* 1.21	.73
Vermont	-.10	.14 .33	-.09 -.29	.10 .52	.42
Wisconsin	-.25	-.52 -.35	-.20 -.39	1.24 .64	.66
Wyoming	.85	-1.30* -.81	-.12 -.08	-.54 -.26	.50

Note: The top coefficients are unstandardized regression coefficients, and the bottom coefficients are standardized regression coefficients.

* $p < .05$.

gains subsequently suffer commensurate losses when running without the benefit of coattails. In midterms, all things being equal, the change in a party's share of state legislative seats is inversely proportional to the share of the vote won by its

presidential candidate in the prior presidential election in the state.

The evidence of a coattail pulse in state legislative elections is quite strong. First, the pattern is consistently found in different states. In 39 of 41 state-level

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presidential election year multivariate regressions, the presidential vote has the expected positive coattail coefficient. Again in 39 of 41 state-level midterm election year multivariate regressions, the presidential vote has the expected negative coattail repercussion coefficient. Second, the pattern is statistically significant in a majority of states, despite the small number of cases (elections) considered. Third, the median magnitude of presidential coattail and repercussion effects in state legislative races is only slightly less than those effects in congressional races. Fourth, though the effects varied from state to state, this variation appears to be systematic. Presidential coattail repercussion effects in a state are generally comparable to the initial presidential coattail effects. State circumstances that blunt aggregate coattail effects, such as ballot forms and the lack of two-party competition, also blunt aggregate coattail repercussion effects. The correspondence of the two types of effects should add to our confidence about the findings. Where evidence is found of one of the effects, there should be evidence of the other, and this is precisely what we find. Finally, presidential coattail and repercussion effects continue to be found after controls for gubernatorial coattail effects are introduced.

Having carefully documented the substantial role presidential candidates play, by their presence in presidential election years and their absence in midterm election years, in determining state legislative election outcomes, the natural question is, what does this mean for state legislative politics? What are the more general political and policy implications? Are presidential coattails at the state legislative level beneficial or harmful to our politics?

The conventional wisdom regarding presidential coattails in congressional elections has been that the system benefits from the linkage of presidential and con-

gressional electoral politics. Coattails promote cooperation between the executive and legislative institutions. Congressmen beholden to the president for their election or reelection, or perhaps even for their future reelection, have sympathetic ears for the president's proposals. If the institutions are structured to disperse power, possibly creating a chaotic or deadlocked system (Burns, 1967), then coattails are a compensating force promoting leadership and cooperation.

In state legislative politics, the role of presidential coattails is not quite so clear. On the face of it, one might conclude that presidential coattails in state legislative elections are detrimental to the system, or at best introduce extraneous noise into state politics. The problems facing the national government are not those facing state governments, which also possess different powers. According to this assessment, there is no reasonable linkage of national and state politics by presidential coattails.

However, the case can be made that presidential coattails are desirable in state legislative elections for much the same reasons they are desirable in congressional elections. To be sure, national and state issues often differ, but we can easily underestimate the number of issues common to both national and state politics, and the cooperation and coordination required to address these overlapping concerns. In a sense, presidential coattails in state legislative elections may help bridge the federalism gap between national and state governments, in much the same way that presidential coattails in congressional elections may help bridge the separation of powers gap between the president and Congress. Moreover, even when the national and state governments are not dealing with common issues, the public may benefit from its different governments pursuing different policy areas with some common underlying perspective or ideology. Presidential coat-

tails, at whatever level, may promote this common perspective. Presidential coattails seem to foster greater coherence between policies of nation and state, even though such bonding between them presumably is smaller than could be effected by "responsible" political parties.

Notes

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1. For an excellent discussion of the different constructions of election outcome variables in state and local election research, see Weber and Parent (1985, pp. 3-9). The advantage in using a seat-change variable as the dependent variable to construct the necessarily parsimonious model required by the small number of cases per state is that a number of variables that might affect the absolute success of a party (e.g., partisanship in the state) are likely to be near constant over a two-year period, and would not affect change in a party's success.

2. If there were no coattail effects, the probability of estimating a positive coefficient in any given state would be approximately one-half. Given this probability, a binominal distribution indicates that there is less than one chance in one thousand of estimating 39 or more positive coefficients out of 41 states. Thus, the proposition that coattail effects do not exist at this level can be quite safely rejected.

3. Results of regressions using the post-presidential election base rather than the pre-presidential election base are available from the author.

4. There are a variety of choices in selecting indicators of party competition. The two indicators used here are selected for several reasons. First, they cover the entire period under study. Although levels of party competition in a state may change over time (Broh and Levine, 1978; Patterson and Caldeira, 1984; Tucker, 1982), the trend variable in the original regressions should correct for this movement. Second, the dispersion indicator of party competition is somewhat sensitive to the aggregation problem raised by Ray and Hawik (1981). A state legislature might be evenly divided between the parties but lack competitive state legislative districts. By the mean percentage of seats indicator such a state would look more competitive than it really is. While the dispersion indicator does not directly measure this district-level competition, it should be somewhat sensitive to it. Third, the mean percentage of seats indicator is consistent with previous research in

party competition, and focuses on competition for the particular office in question. Among others, Ranney (1976) and Patterson and Caldeira (1984) use a party's percentage of seats in the state legislature as one component of their more general indices of state party competition.

5. The gubernatorial coattail effects found in both presidential and midterm elections most probably have their own repercussion effects in the following election.

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THE POWER OF THE STATES IN U.S. PRESIDENTIAL ELECTIONS

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The Electoral College is a uniquely American political institution, yet its impact on both the power of the American states and the relative power of citizens living in different states is not well understood. Game theorists have broached the state power problem exclusively in terms of the size of each of the states. Empirical investigators have been less systematic, basing their analyses solely on which states have been close in a single election. In this paper we present a model of state power which combines the idea of the pivotal player from game theory with an empirical model of state voting. In doing so we provide a theoretically derived and empirically meaningful assessment of state power in presidential elections. We then trace the implications of the power of the states for the relative power of individual voters, finding large disparities between voters from different states.

How powerful is each of the American states in determining the winner in a presidential election? This question would be of no interest if citizens elected the president directly, rather than through the Electoral College. As the institutional mechanism used to choose presidents, the Electoral College has been and continues to be the object of debate. However, this debate has not been informed by knowledge of the actual impact of the Electoral College system on the relative power of the states, or on voters who live in different states. In this paper we shall provide a theoretically meaningful assessment of state power in presidential elections. We shall

then trace the implications of our model for the one-man one-vote principle of importance to democratic political theory.

Previous attempts to assess the power of the states and of individual voters have essentially followed game theory (Banzhaf, 1968; Mann and Shapley, 1962; Margolis, 1977, 1983; Owen, 1975; Riker and Shapley, 1968). These analyses have relied only on the size of states without any consideration of their political competitiveness. Attempts by empirically oriented researchers to utilize competitiveness in assessments of state and group influence have been largely *ad hoc*, simply examining which states were close to the 50-50 split in a single election (for

Table 1. Illustration of the Calculation of the Shapley Value

Permutations ^a			
* ABCD	* BACD	* CABD	* DABC
* ABDC	* BADC	* CADB	* DACB
* ACBD	* BCAD	* CBAD	* DBAC
* ACDB	* BCDA	* CBDA	* DBCA
* ADBC	* BDAC	* CDAB	* DCAB
* ADCB	* BDCA	* CDBA	* DCBA
Column Totals			
0A 1B 1C 4D	1A 0B 1C 4D	1A 1B 0C 4D	2A 2B 2C 0D
Totals			
4A			
4B			
4C			
12D			

^aAssume A has 3, B and C 2, and D 4 votes; * denotes pivot position.

example, Spilerman and Dickens, 1974).¹ We shall construct a model of state power which will take advantage of the analytic rigor of the game theory approach, and at the same time will recognize that states have different levels of electoral competitiveness. The most widely used measure of power in weighted voting situations like the Electoral College is the Shapley value. The Shapley value defines the power of actor A as the number of permutations (orderings) in which A occupies the pivotal position (that is, orderings in which A can cast the deciding vote) divided by the total number of possible permutations. The idea can be illustrated with a simple example. Let us consider four states and label the states A, B, C,

and D, with A having three votes, B and C having two votes each, and D having four votes. To calculate the traditional Shapley value we would consider the $4! = 24$ possible orderings of the four states, and note each time a state plays the pivotal role of turning a coalition from a losing one into a winning one. For example, if the order from most Democratic to most Republican were CABD, B would be the pivotal state, since six votes are needed to win and C and A together account for $3 + 2 = 5$ votes, and by including B's two votes they have a winning coalition. Table 1 shows the 24 possible orderings of the states, and places an asterisk (*) above the pivotal state in each case. In this particular example the most powerful

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actor is D, playing the pivotal role one-half of the time, with A, B, and C having equal power, each playing the pivotal role one-sixth of the time. Notice that A's extra vote does not add to A's power, since A requires both B and C as partners, or D as a partner, which is similar to B and C's situations. Thus, having three votes gives A no more power than the two votes that both B and C have. In contrast, D's extra vote is quite potent. By calculating the Shapley value we have a far more meaningful assessment of state power than we did simply by knowing the basic 3-2-2-4 vote distribution: D is distinctly more powerful than B or C, while A is not.

The problem with the standard Shapley value is the untenable assumption that each ordering is equally likely. Suppose, going back to the four state example, that states A and B are heavily Democratic and ideologically liberal, state D is Republican and ideologically conservative, while state C is balanced between the parties and is ideologically moderate. Consistent with this, suppose that in a series of presidential elections the only orders from most Democratic to most Republican of the four states were BACD and ABCD. In this instance, with the same 3-2-2-4 vote distribution, state C would be the true power source, since C would always determine the winner. In the series of elections there might be some landslides where all four states voted for the same candidate, but there could not be a single election where C is on the losing side. Winning in state C, under these assumptions, is tantamount to winning the election. A power model which assigns half of the power to D and only one-sixth of the power to C, as the standard Shapley value does, is quite misleading and lacks verisimilitude.

The pivotal player concept underlying the Shapley value can be applied to situations where all orderings of states are not equally likely to occur. Based on a sugges-

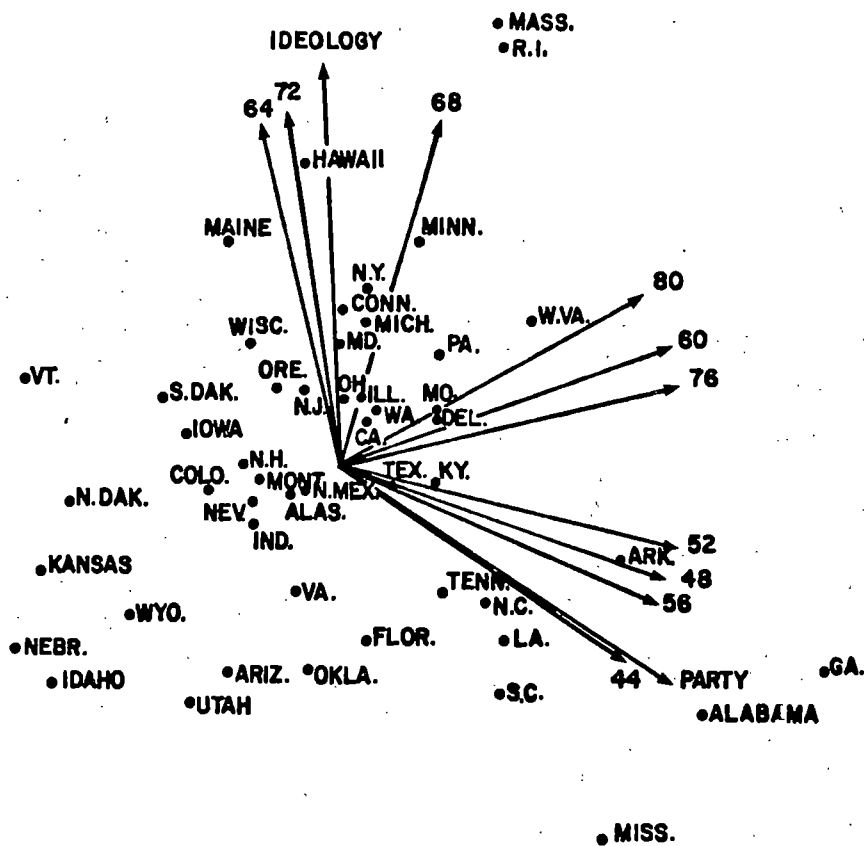
tion of Owens (1971), Shapley (1977) himself has proposed such a modification. The idea is to observe the percentage of times each player occupies the pivotal location, now recognizing that the orderings are constrained by the political preferences (spatial locations) of the players. Using this modification in our example, C would be assigned a power value of 1.0, because it is always in the pivotal position, while each of the other players would be assigned a zero. This simple example serves to illustrate that any meaningful assessment of voting power must recognize that some orderings of states are more likely to occur than others.

The Structure of State Voting in Presidential Elections

The question of which of the many possible orderings of states are likely to occur is intrinsically a structural question.² We constructed the four state example above to be essentially unidimensional. The actual structure of states in a political space is likely to be more complicated than that. Unlike the example in which each state's partisanship matches its ideology, some states tend to be quite Democratic and at the same time quite conservative (e.g., Georgia), while others tend to be Republican and relatively liberal (e.g., Maine). In fact, we find when we analyze state voting in presidential elections that the structure of states in a political space tends to be two-dimensional, with most contemporary (post-1940) elections conforming well to that two-dimensional structure.³

The structural (spatial) analysis of states that we perform uses principal components to analyze the Democratic percentage of the presidential vote in the 50 states from 1944 to 1980, where each election year is a variable and the 50 states are the units of analysis. In Appendix 1 we

Figure 1. Fifty States and Elections with External Axes



show that based on the individual-level spatial choice model, this is an appropriate method to estimate the location of state means in a political space, as well as to estimate the location of election vectors (axes) in that space.⁴ The spatial positions of the states along with the orientations of the election vectors are shown in Figure 1. We have also included on the figure two reference axes, one of which is designed to measure the relative partisanship of the states, and the other the relative ideology of the states. They were placed in the space subsequent to the factor analysis, and have exerted no influence whatsoever on the structure. The reference axes are

included to facilitate interpretation of the structure.⁵ We selected the 1944-1980 set of elections because the time frame is politically coherent, postdating both the great depression and the start of U.S. involvement in the Second World War, and is long enough to provide some historical scope.

The state locations appear consistent with an interpretation of the space according to party and ideology. The upper left quadrant of the space is the liberal Republican section, with no state occupying an extreme position in this quadrant. Maine, and to a lesser extent Wisconsin, are the most obvious members

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of this group. The liberal Democratic section of the space is in the upper right, with Massachusetts and Rhode Island the salient members of that group. The southern states lie primarily in the lower right, conservative Democratic quadrant of the space. Several of the mountain and traditionally Republican midwestern farm states occupy the lower left, with Nebraska and Idaho in the most extreme conservative Republican position. Partisanship was of great importance in the elections from 1944 through 1956. The 1964, 1968, and 1972 elections are more ideological, while 1960, 1976, and 1980 present a mixture of party and ideology. In short, the structure seems intuitively reasonable, and the general orientation of the elections fits nicely with analyses of the specific elections based on mass national surveys.⁶

The spatial model which underlies the factor analysis is a *long-term relative support model*. The model conceives of the ordering of support for the two major party candidates in each state as a function of the position of the state on the two critical underlying dimensions (party and ideology). The model is not concerned with predicting outcomes in the sense of winners and losers; hence it does not consider the variety of short-term forces (candidate appeal and national economic conditions, for example) which are likely to have a consistent impact on the vote in each of the states. The concern is with relative support rather than actual voting percentages, because it is the position of a state in the order from most Democratic to most Republican which determines whether or not that state occupies the pivotal position. Electoral tides may make an election overwhelmingly Democratic or Republican, but the state in the pivotal position is always the fulcrum that divides the states into those which are relatively Democratic and those which are relatively Republican. In a sufficiently close election it is the pivotal state which determines the outcome.

The structural model is not a perfect predictor of electoral orderings. Such factors as local economic conditions, home state effects, and transient issues of particular concern to a state or region are likely to influence the order of state support in any specific election. These state-specific effects are most naturally modelled as random perturbations, and our model of state power will incorporate a random component to represent these forces. Of more immediate concern is the question of how well the structural model accounts for the behavior of the states. The structural model predicts voting patterns over a four decade period on the basis of just two underlying explanatory dimensions. Quite conceivably the model could account for very little. That is not the case.

The structure recovered is quite a good predictor of the specific elections. The results of the principal component analysis are summarized in Tables 2 and 3. Two components account for 79.2% of the variance in state voting across the elections. The first component accounts for 48.1% and the second 31.1% of the total variance in election results. If a third component were added, relatively little additional variance (6.5%) would be explained. With the exception of 1948, every election has at least 70% of the variance accounted for by the component analysis. In 1948 a substantial 57.1% of the variance is still explained.

The structure displayed in this component analysis is critical to the development of a model of state power. If there were no structure undergirding the voting behavior of states, we would be back to the original Shapley assumption that each potential ordering of states is equally likely. The high degree of structure shown in the spatial analysis suggests that some orderings are far more likely to occur than others. Furthermore, the structure forms the basis for assessing how likely various orderings are to occur. The order in which the states are predicted to vote in an elec-

Table 2. Summary of Principal Component Analysis

Component	Eigenvalue	Percent Variance Explained	Cumulative Percent Variance Explained
1	4.81	48.1	48.1
2	3.11	31.1	79.2
3	0.65	6.5	85.7
4	0.52	5.2	90.9
5	0.34	3.4	94.3
6	0.21	2.1	96.3
7	0.17	1.7	98.0
8	0.08	0.8	98.8
9	0.07	0.7	99.6
10	0.04	0.4	100.0

tion is the order in which they project on the election axis. This follows directly from the linear nature of the model underlying factor analysis and the proof we show in Appendix 1.

We can readily illustrate how the orderings are obtained. Consider the spatial position of just the five states (to keep the figure uncluttered) that appear in Figure 2. Panel A of the figure shows the five states and the various election axes; it is the same as Figure 1, with five rather than fifty states. Panel B shows the projections of the five states on the 1972 election axis. The states project in the order Maine, Minnesota, West Virginia, Washington, and Arizona. In an election with the par-

ticular orientation of the 1972 election, the prediction is that the most Democratic of the five states would be Maine and the most Republican, Arizona. The orientation of the 1972 election axis is very much in the ideological direction, suggesting ideology was predominant in determining the order of the states in that year.

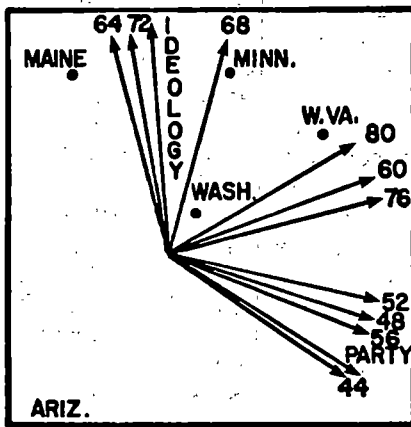
The orientation of the 1980 election axis is about midway between the reference party and ideology axes, suggesting each was about equally important in that election. The projection of the five states on the 1980 axis is shown in Panel C. The order is West Virginia, Minnesota, Washington, Maine, and Arizona—quite different from that of the states in the more

Table 3. Varimax Rotated Coefficients and Variable Communalities

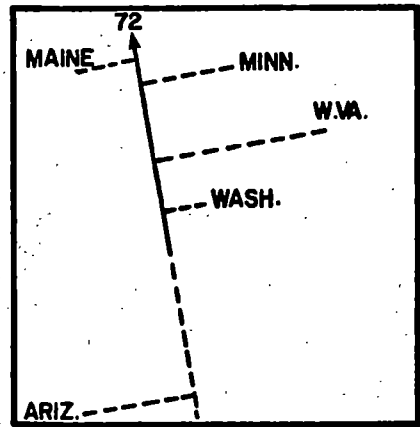
Year	Loadings on:		Percent Variance Explained (100X Community)
	Dimension 1	Dimension 2	
1944	0.76	-0.51	82.8
1948	0.72	-0.24	57.1
1952	0.94	-0.22	93.0
1956	0.83	-0.34	79.6
1960	0.83	0.29	76.8
1964	-0.20	0.88	81.0
1968	0.25	0.89	86.2
1972	-0.13	0.91	83.9
1976	0.84	0.20	74.8
1980	0.76	0.44	76.4

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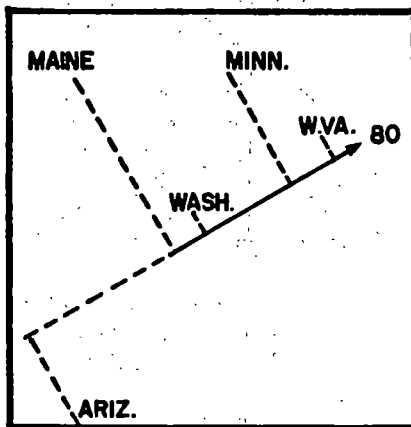
Figure 2. Projections of Five State Points on Several Axes



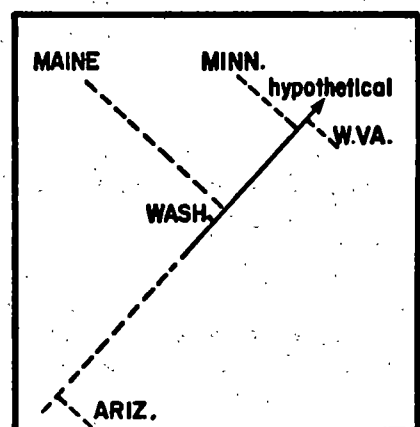
Panel A
Configuration of Points and Axes



Panel B
Projections on 1972 Axis



Panel C
Projections on 1980 Axis



Panel D
Projections on Hypothetical Axis

ideologically oriented election shown in Panel B. Panel D shows the projections of the states on an election axis we drew to represent a hypothetical election which is somewhat more ideologically oriented than the 1980 election. We drew this panel to make clear that any axis drawn in the space represents a hypothetical election and will yield a specific ordering of states.

The ordering in this case is West Virginia, Minnesota, Maine, Washington, and Arizona.

The predicted and actual orders for the 1952, 1972, and 1980 elections appear in Appendix 2, and give some sense of the relationship between the actual election results and those recovered by the analysis. Each of the elections represents

one of the three basic clusters that we will label Party, Ideology, and Mixed, respectively. The states that occupy both the predicted and actual pivot positions are italicized in the table. The state which occupies the actual pivot is also denoted by a "*" in the predicted column.

The predicted orders are quite accurate. The Spearman rho correlations between the predicted and observed orders are .979, .909, and .831 in the three cases, respectively. As should be expected, these closely parallel the communalities reported in Table 3 for the three elections. Of the three states which actually occupy the pivot position, Michigan is predicted to be two states away from the pivot in 1952, Missouri two states away from the pivot in 1972, and Illinois one state from the pivot in 1980. California is the predicted pivot in each case and actually falls on the Republican side of the pivot in 1952 and 1980, and on the Democratic side in 1972.

Upon further examination of those states that are close to the pivotal position, we do find some variation in the three types of elections. Massachusetts, for example, is close to the pivot in the party-oriented 1952 election, but quite far from the pivot in the 1972 and 1980 orientations. In contrast, the two Carolinas and Louisiana are closer to the pivot in the mixed 1980 orientation than in either the 1952 or 1972 orientation, while New Hampshire is closer to the pivot in the ideologically oriented 1972 election. These different orderings suggest that strategic importance is itself a function of election type. That the strategic importance of states will vary by election type makes considerable sense: different states are likely to be important in different kinds of elections. The fact that state power depends on election type, however, does complicate the analysis.

In Figure 3 we show the configuration recovered in the component analysis divided into ideological mixed, and par-

tisan sectors. The ideological and mixed sectors are also combined to form a modern sector. We shall construct distinct power measures for each of the states in each of these sectors. The boundaries of the sectors are based on the cluster structure. The ideological sector begins one degree counterclockwise of the 1964 election axis, and goes to the midpoint between the 1968 and 1980 elections. The end of the mixed sector is one degree clockwise of the 1976 election. The modern sector is thus the sector which incorporates all the election orientations observed since 1960. The party sector ends one degree clockwise from the 1944 election. In general, the particular boundaries selected are not critical, since mild changes of boundaries have very little impact on power measurements.⁷

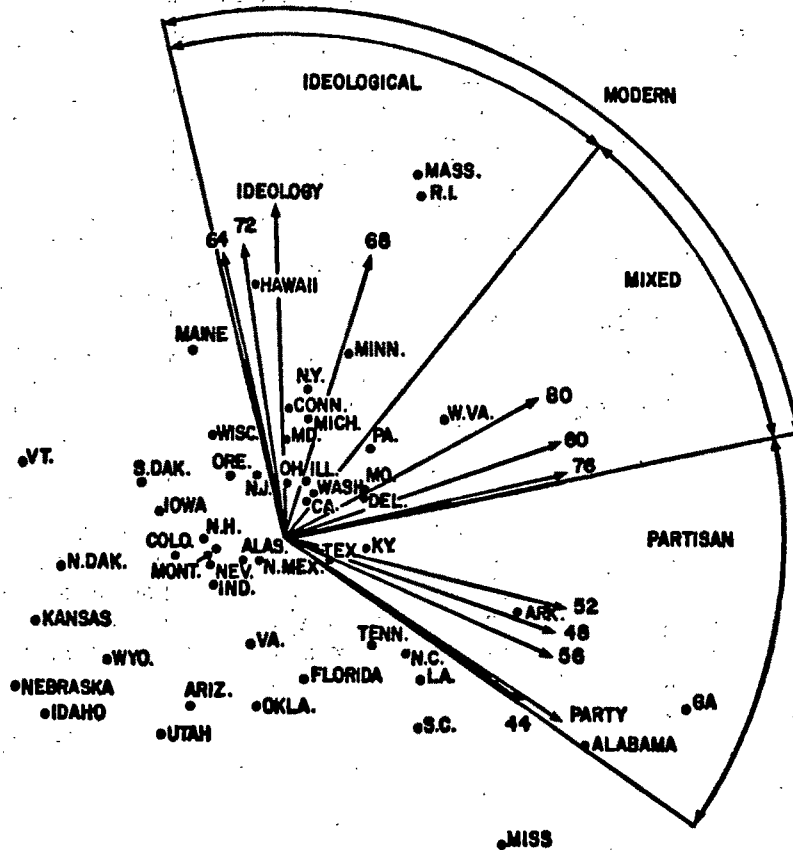
Judgments Underlying the Power Model

To assess power in a sector requires two judgments: (1) Where in the sector are future elections likely to be located? (2) How likely are the orderings of states in future elections to conform to those predicted from the spatial analysis? Without additional information it seems most appropriate to assume elections can occur anywhere in the sector with equal likelihood, i.e., a uniform distribution of elections in each sector.

The second judgment is more complex. There is always the possibility that future elections might not follow the structural constraints laid down by those in the past. This is less a danger than it might first appear. A model based on past presidential voting would have been off the mark in only one election in the 1944-1980 sequence—that of 1964. At the time of the 1964 election, and for several years preceding it, the ideological preferences of the states (as measured by the average ADA and ACA ratings of state congressional delegations) were quite different

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Figure 3. Political Space Divided into Sectors



from the way the state electorates were voting in presidential elections. The apparent deviance of 1964 was simply the first presidential election in which the ideological dimension became influential in presidential voting. No such anomaly between the voting behavior of state congressional delegations and the structure of state voting in presidential elections currently exists. Hence, presidential elections in the near future are likely to be structurally similar to those within the recent past.

Once we recognize that past structure is a useful guide to the structure of future elections, we must consider how accurate

the predictions are likely to be. The principal component analysis recovered 79.2% of the variance in outcomes in the 1944–1980 series of presidential elections, and only in the 1948 election did the variance explained drop below the 70% level. Principal component analysis as a technique, however, is *post hoc*, and does not necessarily constitute a reasonable means for assessing the structural predictability of future cases. Therefore, we performed a second principal component analysis based on just the 1944–1968 series of elections, and then regressed the 1972–1980 elections on the two recovered components. The variance explained in the

three elections was 71.6%, 52.3%, and 52.4%, respectively, for an average explained variance of 58.8%. Since at least half the variance was explained by previous structure in these three cases, and the average was between 50–60%, we felt a reasonable yet conservative estimate of state power would allocate half the variance to previous structure, and half to election-specific factors not predictable prior to the campaign itself.

A Monte Carlo Simulation Approach

Once we have made the judgments concerning the distribution of orientations within the sectors of interest and the relative impact of past structure versus idiosyncratic effects, we can assess state power by simulating large numbers of elections within each sector. A simulation approach is dictated by the complexity of the problem, and parallels the original approach of Mann and Shapley (1960) to the simpler state power problem in which only state size was considered.

The Specific Method

Each orientation of a potential election axis provides an estimate of how the states will be ordered based on how they project on that axis. We assume that any election with that orientation will differ from those projections based on random factors. If we denote the set of projections as the vector P_o and the set of normal random perturbations as the vector N_i , then the simulated election, E_{oi} , is the sum of P_o and N_i , or

$$E_{oi} = P_o + N_i.$$

For each simulated election the states can be ordered from most Republican to most Democratic, and the pivotal state observed.

To simulate a set of elections in a sector we first locate an election vector (a P_o) in the most counterclockwise orientation in

the sector, and then systematically alter that orientation in two-degree units in a clockwise direction through the sector. For each orientation, 10,000 different random vectors (10,000 N_i) are constructed; each random vector provides the basis for a unique simulated election. This method assures that the distribution within sectors will be uniform and will include a full range of orientations. The exact number of simulations per sector using this procedure is 260,000 in the ideology sector, 210,000 in the mixed sector, 470,000 in the modern (ideology plus mixed) sector, and 240,000 in the party sector.

In sum, we are simulating many hypothetical elections in each of the sectors. Each simulated election entails two steps. First, we place in the space an axis on which the states are projected. Second, the position of each state is altered by a random perturbation which represents state-specific effects and reflects uncertainty about the exact state position in the ordering. The power of a state in each sector is determined by the percentage of times it occupies the critical pivot position.

Washington, D.C.

One final point needs to be considered before we present the power values: the case of the District of Columbia. We had inadequate data to include Washington, D.C., in the component analysis. We did, however, do so in preliminary analyses in which we estimated missing votes for the District. In all instances the District of Columbia was located far from the main cluster of states in the Democratic and liberal direction. Thus, in analyzing power we followed two strategies. The first was simply to ignore the District and assume that there were only 535 electoral votes; the second was to include the District automatically as part of the Democratic coalition. We feel the latter is the more realistic way to proceed. The power values were virtually indistinguishable in

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the two cases; the values we report are those obtained including the District automatically as part of the Democratic coalition.⁸

The Power of the States

The Modern Sector

The three most powerful states in the modern sector are California, Texas and New York. These are the states with the largest populations and the largest number of electoral votes. The simple order, however, does not reveal the extent to which competitiveness influences the power distribution. Table 4 shows state power in the modern sector. Column 1 is the power value, showing the percentage of times the state occupies the pivotal position. That percentage is converted to electoral vote units in Column 3 by multiplying the power value by the 538 electoral votes. This value represents the number of electoral votes the various states would have if the number of electoral votes were proportional to power. Column 2 shows the actual electoral votes of the state, and column 4 shows power in electoral vote units using the traditional Shapley value without any consideration of competitiveness. In column 5 is the difference between the power measures, which indicates the extent to which state competitiveness influences state power.

The potent position of California is striking. California's electoral vote of 47, which a traditional Shapley analysis would increase to 49.9, is enhanced by our measure to 64.6, equivalent to a gain of almost 15 electoral units over the Shapley value. Texas, the second on the list and significantly less powerful, is also quite advantaged by its spatial position as it picks up eight electoral units. In contrast, New York actually loses some strength—slightly more than one electoral unit. Of the 10 most powerful states in the modern sector, only New York and Florida are net losers, with Florida losing

the equivalent of about three electoral votes.

The traditional view of a potent midwest is supported by the analysis. The two largest midwestern states, Illinois and Ohio, are the fourth and fifth most powerful states. The power of both states is enhanced by spatial position by more than six electoral vote units. The next 5 states among the 10 most powerful are Pennsylvania, Michigan, New Jersey, Florida, and North Carolina. Of these, the only one substantially advantaged by strategic (spatial) position is New Jersey, picking up more than three electoral units.

At the bottom of the list are the least powerful states. As one would expect, these states tend to be small and politically predictable. The one exception is Massachusetts which is not small, but is predictable. Massachusetts is far and away the biggest loser in voting strength, dropping the equivalent of 11 electoral vote units, which in proportional terms leaves Massachusetts with less than one-seventh of its original voting strength.

The Ideology, Mixed, and Party Sectors

The discussion to this point has focused exclusively on power in the modern sector. We initiated our discussion with the modern sector because we feel future elections are likely to fall somewhere within that sector. We shall now consider the question of state power within the more specific ideology, mixed, and party sectors. Appendix 3 shows power in electoral vote units in the three sectors, along with the difference between the estimated power in the sector and that based on the size-only Shapley value.

Some states are consistently powerful and substantially advantaged by their spatial position—most notably California, Texas, Illinois and Ohio. Several other states, while not as large as these four, are also consistently advantaged by

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Table 4. Power in the Modern Sector

Rank	State	Power % ^a	Electoral Votes	Power Electoral Votes ^b	Shapley Electoral Votes	Power- Shapley ^c
1	California	12.02	47	64.64	49.91	+14.74
2	Texas	6.99	29	37.60	29.60	+ 8.00
3	New York	6.70	36	36.02	37.29	- 1.27
4	Illinois	5.71	24	30.74	24.25	+ 6.50
5	Ohio	5.47	23	29.42	23.19	+ 6.23
6	Pennsylvania	4.96	25	26.68	25.31	+ 1.37
7	Michigan	3.97	20	21.37	20.05	+ 1.33
8	New Jersey	3.66	16	19.68	15.92	+ 3.77
9	Florida	3.36	21	18.09	21.09	- 3.00
10	North Carolina	2.53	13	13.63	12.86	+ 0.77
11	Missouri	2.41	11	12.95	10.84	+ 2.11
12	Wisconsin	2.34	11	12.57	10.84	+ 1.73
13	Washington	2.33	10	12.52	9.84	+ 2.68
14	Tennessee	2.22	11	11.93	10.84	+ 1.08
15	Indiana	2.19	12	11.78	11.85	- 0.07
16	Maryland	2.16	10	11.62	9.84	+ 1.78
17	Kentucky	2.02	9	10.86	8.84	+ 2.02
18	Virginia	1.91	12	10.29	11.85	+ 1.56
19	Louisiana	1.85	10	9.95	9.84	- 0.11
20	Connecticut	1.60	8	8.62	7.84	+ 0.78
21	Iowa	1.56	8	8.37	7.84	+ 0.52
22	Oregon	1.53	7	8.22	6.85	+ 1.37
23	Colorado	1.48	8	7.93	7.84	+ 0.10
24	Georgia	1.47	12	7.89	11.85	- 3.96
25	Minnesota	1.32	10	7.10	9.84	- 2.74
26	South Carolina	1.26	8	6.78	7.84	- 1.06
27	Alabama	1.26	9	6.77	8.84	- 2.07
28	Arkansas	1.07	6	5.77	5.86	- 0.09
29	New Mexico	1.06	5	5.69	4.87	+ 0.82
30	Oklahoma	0.95	8	5.08	7.84	- 2.76
31	West Virginia	0.85	6	4.57	5.86	- 1.29
32	New Hampshire	0.83	4	4.45	3.89	+ 0.56
33	Montana	0.82	4	4.39	3.89	+ 0.50
34	Mississippi	0.78	7	4.17	6.85	- 2.68
35	Nevada	0.76	4	4.06	3.89	+ 0.17
36	Maine	0.73	4	3.91	3.89	+ 0.02
37	Delaware	0.65	3	3.51	2.91	+ 0.60
38	Kansas	0.63	7	3.41	6.85	- 3.44
39	Alaska	0.62	3	3.34	2.91	+ 0.43
40	Arizona	0.62	7	3.33	6.85	- 3.52
41	South Dakota	0.57	3	3.07	2.91	+ 0.16
42	Hawaii	0.52	4	2.77	3.89	- 1.12
43	Vermont	0.44	3	2.39	2.91	- 0.53
44	North Dakota	0.37	3	2.00	2.91	- 0.92
45	Massachusetts	0.33	13	1.78	12.86	-11.08
46	Utah	0.32	5	1.75	4.87	- 3.12
47	Wyoming	0.27	3	1.46	2.91	- 1.46
48	Nebraska	0.26	5	1.42	4.87	- 3.45
49	Idaho	0.19	4	1.02	3.89	- 2.87
50	Rhode Island	0.12	4	0.64	3.89	- 3.25

^aPercentage of times the state occupies the pivotal position.

^bPercentage of times the state occupies the pivotal position multiplied by the total number of electoral votes (538).

^cDifference between power measures.

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spatial position. These states are New Jersey, Missouri, Washington, Maryland, and Kentucky.

In contrast, some states show substantial variation in their power based on election type. New York shows an increase of almost 30% in its power between the ideology and mixed sectors. The southern states as a group account for almost 25% of the total power in the mixed sector, approximately 21% in the ideology sector, and approximately 18% in the party sector. Massachusetts and Rhode Island, both extremely weak in the ideological and mixed sectors, are mildly advantaged in the party sector. Overall, while there is considerable consistency in power across sectors, real change does occur for some states.

The Power of Individual Voters

To the extent that a citizen's right to vote is debased, he is that much less a citizen. The fact that an individual lives here or there is not a legitimate reason for overweighting or diluting the efficacy of his vote. The complexions of societies and civilizations change, often with amazing rapidity. . . . But the basic principle of representative government remains, and must remain, unchanged—the weight of a citizen's vote cannot be made to depend on where he lives. (Warren, *Reynolds v. Sims*, 1964)

Since the 1962 decision in the case of *Baker v. Carr* the Supreme Court has unequivocally supported the one-man one-vote ideal. Yet, this principle is regularly violated in the contest for the presidency. In presidential elections some citizens, by virtue of their physical location in a given state, are in a far better position to determine presidential outcomes than others.

In his widely cited article, "One Man 3.312 Votes," Banzhaf (1968) was the first to argue mathematically that substantial differences exist between the power of the most and least advantaged citizens. His analysis is based on the Shapley value and the assumptions that (1) the election involves only two competing candidates,

and (2) the probability of a voter selecting one or the other candidate in the pivotal state is exactly the same (0.5). Under these assumptions the power of voters in one state (state A) in comparison to voters in another state (state B) is equal to the ratio of the power of each of the states divided by the square root of population size. That is:

$$\frac{\text{power state A} / \text{square root of population size state A}}{\text{power state B} / \text{square root of population size state B}}$$

Based on the 1960 census and using this formula, Banzhaf found that the citizens of New York had more than three times the power of the citizens of Washington, D.C., to determine the outcome of presidential elections.

Based on the Shapley value, the Banzhaf formulation does not take account of the political competitiveness of states. Therefore, we replaced the Shapley value as the measure of state power with our measure of state power in the modern sector, retaining the Banzhaf assumptions. When the relative power differentials are computed in this way, the results are far more striking. We find that a citizen of California is more than twenty times (20.2) as powerful as a citizen of Rhode Island.

These results are not solely dependent on the California and Rhode Island cases. The extreme ratio, ignoring both California and Rhode Island, is 13.4, between the citizens of Texas and Massachusetts. Even if we compare the state with the tenth most powerful citizens (Missouri) and the state with the tenth least powerful citizens (Mississippi), the power ratio exceeds two. This demonstrates that the unequal power of citizens in the United States is not merely due to idiosyncratic cases, but is generic to the Electoral College system.⁹ Indeed, citizens of Washington, D.C., who are the most power-

deprived, are not even included in this assessment.

The power differentials between citizens in the ideology, mixed, and party sectors are larger than in the combined modern sector. This makes sense, because as the election type becomes more structurally defined, the power differential between states should tend to increase.¹⁰ The ratios between the least and most powerful citizens are 41.4 for the ideology sector, 40.2 for the mixed sector, and 91.3 for the party sector. The extreme powerlessness of Georgia generates the very high ratio in the party sector. Similar powerlessness occurs for the citizens of several of the southern states. The low turnout in the South in the era of party-oriented elections is quite consistent with, though not necessarily caused by, the power deprivation of southerners in those elections. In sum, the results from the specific sectors serve to reinforce that in U.S. presidential elections, extreme inequities exist between the power of citizens living in different states.¹¹

Methodological Considerations

The criticism most likely to be directed against the power model we have constructed is that slight changes in the model could be associated with substantial changes in the power values. The questions we feel need to be addressed in this regard are the following:

- (1) What would happen if we reestimated the structural model, recognizing that the original election percentages might be biased because of home state effects?
- (2) How sensitive is the model to the decision to allocate half of the variance to previous structure and half to election-specific (random) factors?
- (3) Would the results change if we used different elections to determine the structure?

In order to address these questions we repeated the entire power analysis, first correcting for possible biases due to the home state of the presidential candidates.¹² We then repeated the power analysis letting state position be a function of 60% prior structure and also 40% prior structure, rather than the 50% used in the analysis we have already reported. We did this for both the home state adjusted and original structures. Finally, we repeated the analysis using just the 1960–1980 elections to generate the spatial structure, and performed a power analysis on the basis of that structure. We performed the 1960–1980 analyses using both the unadjusted and home state corrected percentages.

The stability of the results is striking. Table 5 shows the correlations between the power estimates in the modern sector using each of these methods. No correlation is below 0.99. Shown in parentheses are the correlations between the variables formed by subtracting the state's Shapley value from the power value for each state. This subtraction removes any state size influence which would tend to increase the correlations. Every correlation among these size-modified values is above 0.95. These correlations indicate the method is exceedingly robust, and the power values recovered extremely stable.

Political Observations

Mexican Americans

A full analysis of the demographic implications of the power of the states is beyond the intended scope of this article. However, the role of Mexican Americans is so obviously relevant that some discussion is warranted.¹³ Mexican Americans are becoming an increasing percentage of the U.S. population. As a group they display relatively low turnout; hence, if mobilized, they are capable of having a pronounced impact on elections. Mexican

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Table 5. Correlations Between Power Values^a

Model	Actual Measure 50% Random (Power 50)	40% Random (Power 40)	60% Random (Power 60)	1960-80 Data 50% Random (1960-80)	Home Adjusted 50% Random (Power 50A)	Home Adjusted 40% Random (Power 40A)	Home Adjusted 60% Random (Power 60A)	Home Adjusted 1960-80 Data 50% Random (1960- 80A)
POWER50	1.000 (1.000)							
POWER40	0.998 (0.993)	1.000 (1.000)						
POWER60	0.998 (0.980)	0.992 (0.975)	1.000 (1.000)					
1960-80	0.998 (0.980)	0.995 (0.968)	0.997 (0.980)	1.000 (1.000)				
POWER50A	0.998 (0.980)	0.998 (0.974)	0.997 (0.977)	0.998 (0.981)	1.000 (1.000)			
POWER40A	0.994 (0.969)	0.995 (0.974)	0.990 (0.955)	0.994 (0.967)	0.997 (0.993)	1.000 (1.000)		
POWER60A	0.997 (0.977)	0.997 (0.960)	0.999 (0.984)	0.997 (0.982)	0.998 (0.994)	0.991 (0.977)	1.000 (1.000)	
1960-80A	0.998 (0.981)	0.995 (0.974)	0.997 (0.976)	0.997 (0.994)	0.999 (0.991)	0.996 (0.982)	0.998 (0.986)	1.000 (1.000)

^aIn parentheses are the correlations between size-modified power values.

Americans live predominantly in the southwest. According to the 1980 census they composed 19% of the population of California and 21% of the population of Texas, the two most powerful states. These percentages, even with no further immigration, are sure to increase over the next several decades, since Mexican Americans as a group are both younger and have a substantially higher fertility rate than the non-Mexican American population. Because Mexican Americans are likely to be especially influential in the two most powerful states, they are likely as a group to have a disproportionate impact in future close elections.

Are the Republicans Smarter?

We would expect candidates from powerful states to be advantaged in presi-

dential elections for two reasons. First, the more powerful a state, the greater the strategic value of a home state boost. Second (and, we believe, more critical), large competitive states provide the political environment most comparable to presidential elections. Thus, it is likely that a political style successful in a powerful state will carry over nicely to a presidential election campaign. Candidates from powerful states are, of course, not guaranteed to run well. Other things being equal, however, we would expect them to be very competitive.

Over the 1944-1984 time frame the Republican Party has selected candidates from powerful states far more frequently than has the Democratic Party. Indeed, in every election in the period except for 1944 and 1964, the Republican Party has nominated a candidate from the stronger

state, and in 1944 both were New Yorkers.¹⁴ Likewise, in every election in the period except for 1944 and 1964 the Republican candidate has run ahead of the Republican congressional delegation. The Republican sensitivity to California has also been impressive; a Republican presidential or vice-presidential nominee has come from California in 8 out of the 11 elections in the period. The 1980 and 1984 Reagan-Bush tickets were optimal from a state power perspective, with the presidential candidate representing the most powerful state and the vice-presidential candidate the second most powerful.

Conclusion

The Electoral College is a uniquely American political institution. Because states are the units that choose the president in the Electoral College system, the relative power of the states becomes politically significant. We have developed a model of state power which takes advantage of the pivotal player notion critical in game theory analyses of power, but which at the same time recognizes that states differ in terms of competitiveness. The resulting analyses show California, Texas, Illinois, and Ohio to be substantially more important actors in modern presidential elections than one would expect, even taking into account their large size. Massachusetts and Rhode Island, in contrast, are substantially less powerful. Understanding disparities in state power is the necessary foundation for evaluating the power of demographic groups and presidential campaign strategies.¹⁵ These will be relevant subjects for further analysis.

A critical function of empirical political science is to inform the broader society on issues of normative importance. The question of the inequality of voters in presidential elections is such an issue. Our analysis indicates that far greater dif-

ferences in power exist across the voting population than previous research has shown. Using the common Banzhaf assumptions and taking competitiveness into account, we find a citizen of California is over 20 times more likely to determine the outcome of a modern presidential election than is a citizen of Rhode Island. Even eliminating California and Rhode Island, we find ratios above 13 between the power of the most and least advantaged citizens. Whether these departures from the one-man one-vote principle are viewed with alarm is, of course, a normative decision. That substantial disparities arise because of the Electoral College system is now apparent.

Appendix 1. Model and Data

If (1) the voting choices of individuals conform to a multidimensional spatial model, and (2) the distribution of voters in each of the 50 states is multivariate normal, with states having different mean locations but identical covariance structures, then principal components provide a least squares estimate of the spatial position of state means and the orientation of election vectors.

Notation

We will use upper-case letters to represent vectors and matrices, and lower case letters to represent scalars. Column vectors will be denoted with a '^t'; thus X is a matrix or a row vector, X' is the transpose of X , and x is a scalar.

Proof

Let θ_{re} represent the spatial location of the Republican candidate in election e , and θ_{de} the location of the Democratic candidate. Let $\Delta_e = \theta_{re} - \theta_{de}$. (Note: Δ_e is the election vector.) The equation of the bisecting hyperplane can be represented as

$$\Delta_e \cdot X' = k_e$$

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where k_e is an election specific constant. (See Thomas, 1963, pp. 620-21 for a discussion of the geometry.) Hence if V_i' is the spatial position of voter i

$\Delta_e \cdot V_i' > k_e$ implies voter i will vote Republican.

$\Delta_e \cdot V_i' < k_e$ implies voter i will vote Democratic.

Let \bar{V}_s be the mean location of the voters in state s . Now consider the distribution of the $\Delta_e V_i'$ in some state s . This distribution is univariate normal with the mean $\Delta_e \bar{V}_s'$. This follows from the fact that linear combinations of normal random variables are normally distributed. The variance of each of the state distributions would be identical across states because they have identical covariance structures; call that variance σ_e^2 .

The proportion of the voters in state s in election e who vote Democratic would thus be

$$Pr[-\infty < z < (k_e - \Delta_e \bar{V}_s')/\sigma_e]$$

where z is a standard normal variate.

Let $P_{s,e} = k_e/\sigma_e - (1/\sigma_e) \Delta_e \bar{V}_s'$. [Note: $P_{s,e}$ is the probit value associated with the above probability range.]

Now consider the 50 element vector P_e composed of the various $P_{s,e}$. $P_e = K_e^* - \Delta_e^* \bar{V}'$ where K_e^* is a vector of constant k_e/σ_e , \bar{V}' is the matrix formed with each of the \bar{V}_s' as columns, and Δ_e^* is $(1/\sigma_e) \Delta_e$.

We have not established an origin for the structural space. Let the mean of each row of \bar{V}' be zero, which puts the origin of the political space in the center of state means. The average value of the elements of the vector formed by $\Delta_e^* \bar{V}'$ would then be zero for any Δ_e^* .

Let Z_e be the standardized version of P_e , that is $Z_e = -(1/\sigma_{p_e}) (P_e - K_e^*)$ where σ_{p_e} is the standard deviation of the values of P_e .

$$Z_e = -(1/\sigma_{p_e}) \Delta_e^* \bar{V}' \text{ or}$$

$$Z_e = \Delta_e^* V' \text{ where } \Delta_e^* = -(1/\sigma_{p_e}) \Delta_e^*.$$

Now consider the matrix Z whose rows are the various Z_e and the matrix Δ^* whose rows are the various Δ_e^* . We then have:

$$Z = \Delta^* \bar{V}'.$$

Notice this model is specified only up to a unit of measurement on each row of \bar{V}' . Let the variance of the value of each row of \bar{V}' be equal to one. The model is now the classic component model and principal components provide a least squares estimate of Δ^* and \bar{V}' .

Data

The scaling model depends on the Δ_e vectors above being well defined, which implies that only two candidates, the Democratic and the Republican, are competing. Hence, in elections with a significant minor party vote the results need to be modified suitably. We used the following method to modify the percentages:

(a) In 1968 we divided each Wallace vote .685 for Nixon to .315 for Humphrey. This division was based on the thermometer preference ratio of Nixon to Humphrey among those respondents who indicated they had voted for Wallace in the 1968 national survey of the Inter-University Consortium for Political and Social Research (ICPSR). (The .685 assigned to Nixon is the sum of the percent preferring Nixon to Humphrey, plus one-half of the ties.) In 1980 an identical method was used to distribute the Anderson vote in the ratio of .555 for Carter to .445 for Reagan. We rely on thermometer ratings to distribute the Wallace and Anderson votes, since they are by far the best single predictor of vote available in the ICPSR national survey.

(b) In states other than Alaska and Hawaii, in which more than 7.5% of the total vote was for other than the two major party candidates (excluding Wallace in 1968 and Anderson in 1980), we calculated a regression estimate of the Democratic percent of the vote. The pro-

cedure involved first regressing the election outcomes for each of the years (other than 1952 and 1968) on the 1952 and revised 1968 Democratic percentages. These two elections had the desired characteristics of having no missing data and being quite different kinds of elections. We then used the regression coefficients to calculate the estimated vote, making sure each estimate was plausible in that the estimated Democratic percent had to be greater than the actual Democratic vote, and less than 100.0—the Republican percent. If the estimate was less than the actual Democratic percent, the actual percent was used. If the estimate was more than 100—the Republican percent, 100—the Republican percent was used. Estimates were used in the following cases and are reported as (original to estimated):

1948: Alabama (0 to 58), Louisiana (33 to 54), Mississippi (10 to 57), New York (45 to 51), South Carolina (24 to 53), Tennessee (49 to 52), Virginia (48 to 50);

1956: South Carolina (45 to 47);

1960: Louisiana (50 to 52), Mississippi (36 to 55);

1972: Idaho (26 to 30).

(c) For Alaska and Hawaii all votes prior to 1960 were estimated, based on regressions using 1960 and 1968 as the

independent variables, as was Alaska's vote in 1980. The estimates for Alaska and Hawaii by year are: 1944: (55, 47); 1948: (50, 50); 1952: (44, 39); 1956: (42, 37); 1980: Alaska (26 to 35). Since a substantial number of Alaska and Hawaii's vote percentages are estimates, their spatial placements should be treated with caution.

(d) The source for the 1944–1972 presidential election data was ICPSR study #0019 *State Level Presidential Election Returns (1824–1972)*, which was originally collected by Walter Dean Burnham, Jerome M. Clubb, and William H. Flanagan. (We corrected one keypunch error where the 1968 Democratic and Republican vote in Oregon had been reversed.) The 1976 and 1980 official returns were obtained from *Congressional Quarterly Weekly Reports* (1976: December 18, 1976, pp. 3334–3335; 1980: January 17, 1981, p. 138).

(e) In the analysis we used actual percentages rather than probit values. Probit values are virtually linear with percentages in the 20–80% range. The few cases in which the Democratic percentage fell below 20% were associated with ballot irregularities. These artifactually low percentages are only exaggerated by a probit transformation; hence we felt such a transformation was inappropriate.

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Appendix 2. Predicted and Actual State Orders from Most Republican to Most Democratic

1952 Party		1972 Ideology		1980 Mixed	
Predicted	Actual	Predicted	Actual	Predicted	Actual
Vermont	Vermont	Mississippi	Mississippi	Nebraska	Utah
Nebraska	North Dakota	Alabama	Oklahoma	Idaho	Idaho
Kansas	Kansas	Georgia	Georgia	Kansas	Nebraska
North Dakota	South Dakota	Alabama	Alabama	North Dakota	North Dakota
Idaho	Nebraska	Utah	Utah	Wyoming	Nevada
South Dakota	Maine	Oklahoma	South Carolina	Utah	Wyoming
Wyoming	Idaho	Louisiana	Florida	Vermont	Arizona
Maine	Iowa	Arizona	Louisiana	Arizona	South Dakota
Iowa	Wyoming	Idaho	North Carolina	Oklahoma	New Hampshire
Colorado	Nevada	Florida	Nebraska	Colorado	Alaska
Wisconsin	Wisconsin	North Carolina	Kansas	South Dakota	Oklahoma
Hawaii	Oregon	Nebraska	Tennessee	Iowa	Montana
New Hampshire	Colorado	Tennessee	Arizona	Indiana	Kansas
Utah	New Hampshire	Arkansas	Idaho	Virginia	Colorado
Nevada	Hawaii	Virginia	Virginia	Nevada	New Mexico
Indiana	Montana	Wyoming	Wyoming	New Hampshire	Indiana
Oregon	Indiana	Kansas	Arkansas	Montana	California
Montana	Utah	Indiana	Texas	Alaska	Florida
Arizona	Arizona	Texas	Indiana	Florida	Texas
New Jersey	New Jersey	New Mexico	Colorado	New Mexico	New Jersey
Alaska	California	Nevada	Alaska	Wisconsin	Virginia
Connecticut	Ohio	Kentucky	Kentucky	Oregon	Washington
Maryland	Virginia	Alaska	New Hampshire	New Jersey	Iowa
New Mexico	New York	North Dakota	North Dakota	Maine	Oregon
New York	Maryland	Montana	Nevada	South Carolina	Ohio
Virginia	Connecticut	Colorado	West Virginia	Tennessee	Connecticut
Ohio	<i>Michigan</i>	New Hampshire	New Mexico	Ohio	<i>Illinois</i>
Michigan*	Minnesota	Missouri*	Vermont	Texas	Pennsylvania
Illinois	Alaska	Delaware	New Jersey	Mississippi	Michigan
<i>California</i> ^a	New Mexico	<i>California</i>	Maryland	<i>California</i>	Missouri
Oklahoma	Washington	Washington	Hawaii	Illinois*	Vermont
Washington	Illinois	Iowa	<i>Missouri</i>	Maryland	Louisiana
Minnesota	Florida	Ohio	Montana	North Carolina	Wisconsin
Massachusetts	Oklahoma	Illinois	Ohio	Louisiana	Maine
Texas	Massachusetts	New Jersey	Maine	Washington	Alabama
Florida	Texas	South Dakota	Washington	Connecticut	New York
Pennsylvania	Pennsylvania	Oregon	Pennsylvania	Kentucky	Delaware
Rhode Island	Delaware	Pennsylvania	Delaware	Michigan	North Carolina
Missouri	Rhode Island	Maryland	Connecticut	Missouri	Mississippi
Delaware	Missouri	West Virginia	Iowa	Delaware	Kentucky
Kentucky	Tennessee	Vermont	Illinois	New York	Arkansas
Tennessee	Kentucky	Wisconsin	New York	Hawaii	South Carolina
West Virginia	South Carolina	Michigan	California	Pennsylvania	Tennessee
North Carolina	West Virginia	Connecticut	Michigan	Minnesota	Massachusetts
Louisiana	Louisiana	New York	Oregon	Alabama	Hawaii
South Carolina	North Carolina	Minnesota	Wisconsin	Arkansas	Minnesota
Arkansas	Arkansas	Maine	South Dakota	West Virginia	Maryland
Mississippi	Mississippi	Hawaii	Minnesota	Georgia	West Virginia
Alabama	Alabama	Rhode Island	Rhode Island	Rhode Island	Rhode Island
Georgia	Georgia	Massachusetts	Massachusetts	Massachusetts	Georgia

*Denotes state occupying the actual pivot position.

^aItalics denote state occupying the pivot positions in the column.

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Appendix 3. State Power in Electoral Vote Units by Sector

Rank	Ideology			Mixed			Party		
	State	Power	Difference from Shapley	State	Power	Difference from Shapley	State	Power	Difference from Shapley
1	California	66.36	+16.46	California	62.52	+12.61	California	61.32	+11.41
2	Texas	37.79	+ 8.18	New York	41.10	+ 3.81	New York	44.11	+ 6.83
3	New York	31.92	- 5.37	Texas	37.37	+ 7.77	Texas	35.35	+ 5.75
4	Illinois	31.34	+ 7.09	Illinois	30.01	+ 5.76	Pennsylvania	29.35	+ 4.04
5	Ohio	30.09	+ 6.90	Ohio	27.59	+ 5.40	Illinois	28.89	+ 4.64
6	Pennsylvania	27.59	+ 2.28	Pennsylvania	25.55	+ 0.24	Ohio	27.53	+ 4.34
7	Michigan	20.21	+ 0.17	Michigan	22.81	+ 2.76	Michigan	23.98	+ 3.93
8	New Jersey	20.00	+ 4.08	Florida	21.51	+ 0.42	Florida	23.87	+ 2.77
9	Florida	15.32	- 5.77	New Jersey	19.29	+ 3.37	New Jersey	17.62	+ 1.70
10	Missouri	13.42	+ 2.58	North Carolina	15.41	+ 2.56	Virginia	13.75	+ 1.90
11	Indiana	13.03	+ 1.19	Tennessee	13.11	+ 2.27	Massachusetts	13.18	+ 0.32
12	Washington	12.82	+ 2.98	Wisconsin	12.66	+ 1.81	Indiana	12.19	+ 0.34
13	Wisconsin	12.50	+ 1.66	Missouri	12.37	+ 1.53	Missouri	12.10	+ 1.26
14	North Carolina	12.19	- 0.67	Maryland	12.23	+ 2.39	Washington	11.91	+ 2.08
15	Kentucky	11.42	+ 2.58	Washington	12.14	+ 2.31	Maryland	11.66	+ 1.83
16	Maryland	11.13	+ 1.30	Louisiana	11.75	+ 1.92	Minnesota	11.45	+ 1.61
17	Tennessee	10.97	+ 0.13	Virginia	10.26	- 1.59	Tennessee	10.87	+ 0.03
18	Virginia	10.32	- 1.53	Indiana	10.22	- 1.63	North Carolina	10.35	- 2.51
19	Georgia	10.14	- 1.71	Kentucky	10.17	+ 1.33	Kentucky	9.91	+ 1.07
20	Iowa	9.82	+ 1.98	Connecticut	9.32	+ 1.48	Wisconsin	9.83	- 1.01
21	Colorado	9.43	+ 1.59	South Carolina	8.96	+ 1.11	Connecticut	8.90	+ 1.06
22	Louisiana	8.48	- 1.35	Minnesota	8.33	- 1.51	Oklahoma	8.81	+ 0.97
23	Oregon	8.45	+ 1.60	Oregon	7.93	+ 1.08	Oregon	7.33	+ 0.48
24	Connecticut	8.05	+ 0.21	Alabama	7.68	- 1.16	Louisiana	7.04	- 2.80
25	Arkansas	6.69	+ 0.83	Mississippi	7.13	+ 0.28	Arizona	6.91	+ 0.06
26	Minnesota	6.11	- 3.73	Iowa	6.56	- 1.28	Colorado	6.73	- 1.12
27	Alabama	6.04	- 2.80	Colorado	6.08	- 1.76	New Mexico	5.85	+ 0.97
28	New Mexico	5.96	+ 1.08	Oklahoma	5.82	- 2.02	Iowa	5.80	- 2.04
29	Kansas	5.29	- 1.56	New Mexico	5.36	+ 0.49	South Carolina	5.57	- 2.27
30	West Virginia	5.18	- 0.68	Georgia	5.10	- 6.74	West Virginia	5.21	- 0.65
31	South Carolina	5.02	- 2.82	Arkansas	4.63	- 1.23	Utah	4.65	- 0.22
32	New Hampshire	4.98	+ 1.09	Maine	4.51	+ 0.62	Montana	4.19	+ 0.30
33	Montana	4.85	+ 0.95	Hawaii	4.01	+ 0.12	Rhode Island	3.98	+ 0.09
34	Nevada	4.52	+ 0.63	Montana	3.82	- 0.07	Nevada	3.97	+ 0.08
35	Oklahoma	4.49	- 3.35	West Virginia	3.81	- 2.05	New Hampshire	3.81	- 0.08
36	Delaware	3.78	+ 0.86	New Hampshire	3.79	- 0.10	Hawaii	3.78	- 0.11
37	South Dakota	3.54	+ 0.63	Nevada	3.49	- 0.40	Alaska	3.39	+ 0.47
38	Alaska	3.53	+ 0.61	Arizona	3.18	- 3.67	Delaware	3.21	+ 0.29
39	Arizona	3.45	- 3.40	Delaware	3.17	+ 0.26	Maine	2.82	- 1.07
40	Maine	3.43	- 0.47	Alaska	3.11	+ 0.19	Kansas	2.38	- 4.47
41	Vermont	3.30	+ 0.39	Massachusetts	3.02	- 9.84	Arkansas	2.09	- 3.77
42	North Dakota	2.87	- 0.05	South Dakota	2.50	- 0.42	Wyoming	2.01	- 0.91
43	Nebraska	2.25	- 2.63	Utah	1.66	- 3.21	Mississippi	1.92	- 4.93
44	Wyoming	1.90	- 1.02	Vermont	1.25	- 1.66	Idaho	1.90	- 2.00
45	Utah	1.82	- 3.06	Kansas	1.08	- 5.77	South Dakota	1.82	- 1.09
46	Mississippi	1.78	- 5.07	Rhode Island	0.99	- 2.91	Nebraska	1.65	- 3.22
47	Hawaii	1.77	- 2.12	North Dakota	0.92	- 2.00	North Dakota	1.09	- 1.82
48	Idaho	1.51	- 2.38	Wyoming	0.91	- 2.00	Alabama	1.09	- 7.75
49	Massachusetts	0.79	-12.07	Idaho	0.42	- 3.48	Vermont	0.59	- 2.33
50	Rhode Island	0.36	- 3.53	Nebraska	0.40	- 4.43	Georgia	0.32	-11.53

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Notes

We would like to thank Jeffrey Obler and the Comparative Politics Discussion Group of the University of North Carolina for useful comments on an earlier version of this manuscript.

1. The two attempts that have been made to assess state power more systematically, Merrill (1978) and Levesque (1984), both have substantial drawbacks. Merrill's analysis suffers from a naive view of the electoral process. Levesque's analysis is based on a probability model which is seriously flawed and which yields assessments lacking even surface validity. Levesque assesses the probability that a state, by voting one way or another, might sway an Electoral College outcome. In calculating this probability he does not consider the likelihood that the state will actually be close to the 50-50 split. Indeed, he gives no consideration whatever to the probable behavior of the key state. The measure is akin to assessing the likely hero in a football game by seeing how evenly balanced the two teams are given a certain player is excluded, but ignoring the quality of that player. Thus, it is not surprising that Levesque's results are counter-intuitive. For example, in 1952 (Levesque only provides estimates for 1952 and 1968) he finds Georgia, Alabama, Mississippi, and North Carolina more powerful per electoral vote than either Illinois or Ohio, even though he calculates the probability of each of the four southern states voting Republican to be zero to four decimal places.

2. The spatial (structural) approach we take in analyzing state power is similar to that used by Shapley (1977) in a theoretical discussion of legislative behavior. In constructing our empirical model of state power we developed this approach independent of Shapley. We are indebted to one of the anonymous reviewers for alerting us to the Shapley paper.

3. This result is consistent with individual level structural (spatial) analyses of the U.S. electorate. See, for example, Weisberg and Rusk (1970), Rabinowitz (1978), Enelow and Hinich (1984), and Poole and Rosenthal (1984).

4. More generally, principal component based methodologies are proving useful for spatial analysis. In particular see Enelow and Hinich (1984) and Aldrich and McKelvey (1977).

5. The exact method used to construct the party variable was to average the Republican David Composite B index for each state over all of the off year elections from 1942-1978. The Composite B is itself an average of Senate, House, and Gubernatorial election data. The liberal-conservative variable was based on the difference between the Americans for Democratic Action (ADA) and Americans for Constitutional Action (ACA) rating of each state congressional delegation in each year such ratings were available (1960-1980). The ADA-ACA difference

was calculated for each member of the state delegation, and these differences were averaged over the entire time span to form the single state liberal-conservative value. The Composite B and liberal-conservative variables were then used as dependent variables in regressions where the spatial dimensions were the independent variables. The axes drawn in the space are those in which the state projections most closely (in a least squares sense) approximate the original two variables. The R^2 for the party regression was .793, while that for the liberal-conservative regression was .741. Both of these values indicate very good fit.

6. Asher (1984) provides a good summary of the literature on presidential elections from 1952 through 1980. The party orientation of the early elections corresponds exactly to standard survey findings for those elections. The three elections in which the ideological component was predominant are all elections in which issues played a substantial role. That an ideological dimension can summarize issue effects well, even when mass publics tend not to be ideological, makes sense if elites behave in an ideologically consistent fashion. This idea is discussed at greater length in Rabinowitz, Gurian, and Macdonald (1984).

7. We state the boundary margins with such precision because we will estimate state power within sectors; hence, the sectors must be well defined. Our particular choice of these boundaries rests on the presumption that the 1964 and 1976 election axes set realistic bounds for any election in the near future. The reason that boundary changes have little impact is that the projections are quite stable for proximal axes. For the same reason, the method is not sensitive to the distribution of axes within sectors.

8. Including Washington, D.C., there are an even number of electoral votes (538). This means that ties are possible. In the event of ties, the state which brings the Democratic side to 269 and the state which brings the Republican side to 269 are both counted 0.5. By assigning 0.5 to each side in tied cases and 1 to the pivot state in untied cases, we are entirely consistent with the Shapley method. The Shapley method would count 2 for each untied case (the permutation and its mirror image) and 1 for each side in tied cases.

9. Recently Margolis (1977, 1983) has criticized Banzhaf's equal probability assumption, and shown that if the actual probabilities deviate only slightly from 0.5, the divisor (square root of population size) may be inappropriate. The implication of Margolis' critique is that more modest power differences may exist between individual voters than those computed by Banzhaf. While it is not within the scope of this paper to treat the question fully, we did compute a more conservative measure of the relative power of individuals. In order to be as conservative as possi-

ble, and in keeping with the Margolis critique, we found the divisor between the population size and the square root of population size which minimizes the relative advantage of any one citizen in comparison to any other citizen.

To obtain the minimizing divisor, we considered sets of divisors of the class:

$$\text{divisor} = a \cdot \text{population} + (1-a) \cdot \text{square root of population}, [0 \leq a \leq 1].$$

In particular we considered the 101 sets of divisors associated with "a" values from 0 to 1 separated by .01 (i.e., $a = 0, .01, .02, .03, \dots 1.0$). We calculated the maximum power ratio for each set of divisors, and found the set which had the smallest maximum power ratio; this occurs when $a = .51$. Note that when $a = 1$ the set of divisors is the population size of each state, while when $a = 0$ the set of divisors is the square root of state population. The maximum power ratio is a nicely behaved function of "a" monotonically decreasing until its minimum, and monotonically increasing thereafter.

Using this minimizing divisor, we find the power of the citizens of California, who are the most powerful, to be more than eleven times (11.6) that of the citizens of Massachusetts, who are the least powerful. Setting aside these two states the ratio is 9.1. Clearly, these ratios are lower than those obtained with the Banzhaf assumptions. However, even using this very conservative approach, when the competitiveness of the states is taken into account, the differential power of citizens is substantial.

10. This follows directly from the fact that as the election axis changes orientations, states that are quite far from the center in one orientation will often tend to be closer to the center in another orientation.

11. Using the minimizing divisor (see footnote 9 above) the ratios between the least and the most powerful citizens are 27.2 for the ideology sector, 19.0 for the mixed sector, and 60.0 for the party sector.

12. Our bias correction entailed the following procedure. We regressed each election outcome on the ADA/ACA ideology variable and the David Composite B party variable. We then calculated the residual from the regression estimate for each state with a home state presidential candidate, except for 1944 and the Eisenhower cases. We eliminated 1944 because both candidates were New Yorkers; we eliminated the Eisenhower cases because he had no electoral history in any state.

The calculated residuals are reported below. Positive numbers indicate that the home state candidate ran better than the regression predicted, while negative numbers indicate the candidate ran worse. The residuals are: 1948: Missouri +6.45, New York +0.58; 1952: Illinois +4.71; 1956: Illinois +0.45; 1960: Mass +6.69, Calif -1.08; 1964: Texas +7.73, Arizona +2.94; 1968: Minn +6.22, Calif

+0.95; 1972: South Dakota +10.31, Calif -3.19; 1976: Georgia +8.66, Michigan +1.55; 1980: Georgia +6.86, Calif +2.38.

Only Nixon in 1960 and 1972 ran worse in his home state than predicted by the regressions. We adjusted the actual vote in each state by the amount of the regression residual less 0.305, except in the 1960 and 1972 Nixon cases (0.305 is the sum of the two Nixon residuals divided by 14). In the ensuing home state adjusted factor analysis, no state with a candidate running had a residual in excess of 3.0 percent, except for Nixon in 1972 (4.1 percent).

We may have slightly overcorrected the home state influence using this method. The mean residuals from the home state adjusted factor analysis were slightly negative for both parties (i.e., home state adjusted candidates were predicted to do better than their adjusted percentages). For the purposes of checking the robustness of the power measure, overcorrection is not a problem, since the overcorrection increases the difference between the unadjusted and adjusted percentages.

The corrections we made are generally consistent with those implied by Lewis-Beck and Rice (1983). They use quite a different approach in assessing home state effects over the last century.

13. See Sigman (1983) for an excellent discussion of the potential impact of Mexican Americans on U.S. electoral politics.

14. The statement is objectively true for Eisenhower—he was formally a New Yorker in 1952 and a Pennsylvanian in 1956, both party-oriented elections. Eisenhower, however, was clearly not a candidate with strong state ties.

15. That the inadequacy of game theory treatments has hampered the development of theories of campaign strategies is well recognized. See, for example, the Colantoni, Levesque and Ordeshook (1975) critique of the important Brams and Davis (1974) piece.

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LEGISLATORS AND INTEREST GROUPS: HOW UNORGANIZED INTERESTS GET REPRESENTED

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This paper derives a supply price for public policy using a constrained maximization model. In the model, three sets of agents each have preferences over outcomes: organized interest groups offer campaign contributions to improve their own wealth, voters offer votes to obtain outcomes closer to their most preferred outcomes, and legislators seek both campaign contributions and votes to obtain reelection. A given legislator's supply price for policy is shown to depend on the productivity of his effort, as determined by committee assignments, priority and ability, and by the preferences of his unorganized constituency in the home district. Two extreme assumptions about the effectiveness of campaign spending in eliciting votes are used to illustrate the comparative statics properties of the model. The prediction of the model is that interest groups will, in general, seek out legislators whose voters are indifferent to the policy the interest group seeks. Thus, voters who do have preferences over policy are in effect represented, even though they are not organized.

The extent to which organized interest groups can affect policy is a matter of theoretical debate. Early pluralist theory in political science asserted that competition between groups is the sole process by which policy was formed, with the Congress (in the U.S.) as the passive referee in this struggle (see Bailey, 1950; Bentley, 1907; Latham, 1952; Schattschneider, 1935; and Truman, 1958, 1959; Niskanen, 1971, takes this view in discussing bureaucracy). "The

legislative vote on any issue tends to represent the composition of strength, i.e., the balance of power among the contending groups at the moment of voting" (Latham, 1952, p. 36).

However, this view is clearly too simplistic. Congressmen are not ciphers, meekly responding to pressure-group activities. Partly as a reaction to the pluralist position, a dissenting school developed, marked by Matthews (1960), Bauer, Pool, and Dexter (1972), and

others. Bauer et al. (1972, p. 478) assert that

congressmen have a great deal more freedom than is ordinarily attributed to them. The complexities of procedure, the chances of obfuscation, the limited attention constituents pay to any one issue, and the presence of countervailing forces all leave the congressman relatively free on most issues. He may feel unfree because of the great demands on his time, but, consciously or unconsciously, by his own decisions on what he chooses to make of his job he generates the pressures which impinge upon him. He hears from voters about those things in which he himself chooses to become involved.

The view of Congressional dominance did not completely replace the pluralist theory as the orthodox model of policy formation, but it was widely accepted. The new approach was of great importance because of the explicit recognition that policy formation is affected both by those who seek a certain outcome, as the pluralists claimed, and by those legislators who have discretion over who receives the benefits. More recent work views Bauer et al. as having overemphasized "supply" forces, giving Congress nearly complete discretion over policy and ignoring interest groups as demanders of policy. Efforts to explore the middle ground, where lobbyists and congressmen both share and compete with each other for decision power (Hayes, 1981, p. 12) have spawned case studies and theoretical analyses involving "subgovernments," "policy subsystems," "iron triangles," and "clientelism," but offer few attempts to model these relationships formally, and provide few general conclusions.

One explicit model of the supply and demand components of the process is Miller and Moe's (1983) bilateral monopoly model. While conceptually appealing, the bilateral monopoly model has few predictions for outcomes beyond the fact that the greater a group's bargaining strength, the better able it is to improve its strength against the others. However, many political situations have competitive aspects that make a bilateral monopoly

model inappropriate. There are a number of national, or at least multidistrict, interest groups that may compete with other groups, or have the option of lobbying among several legislators.

Economists have generally used a different approach in attempting to discover, explain, and measure the effects that interest groups have in influencing legislators and legislation. For example, Stigler (1971), McPherson (1972), Pincus (1975), and Salamon and Seigfried (1977) all attempt to measure the effect of interest group power in obtaining favorable treatment. The same kind of technique has been used by Kau and Rubin (1982) and Peltzman (1982), using district-level aggregate data to measure the effect of demographic and other regularities of the constituency on roll-call voting behavior. While providing useful insights into the relationship between interest groups and congressmen, these studies are limited by the inability of survey or demographic data to measure accurately lobbying activity, constituent interests, and the preferences of legislators. Rather than explicitly modeling these disparate but important influences, the economic studies have simply identified regularities in the characteristics of interest groups and legislative voting behavior or policy outcomes.

This paper develops a theoretical model of the supply of public policy in the U.S. The model is shown to yield testable implications about which interest groups and which sets of voters are likely to be served. The basic logic of the supply model is as follows: Policy outcomes depend on the comparative advantages of the participants. Comparative advantage, in this context, depends on the value of what each set of agents in the model has to offer the others. We consider three sets of agents: voters, interest groups, and legislators. In order to simplify the analysis we focus on the implicit market for policy, in which interest groups offer contributions to legislators in exchange

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for legislators' efforts on each interest group's behalf. The contribution of this research is its derivation of an explicit *supply price* for policy. The amounts interest groups must offer a legislator for his services are shown to depend on that legislator's productivity and the preferences of the voters in his district.

Two extreme assumptions about voter preferences, which we distinguish by the names *rational ignorance* and *civics class*, are used to illustrate the comparative statics predictions of the model. The comparison of the results of these assumptions allows us to focus on the question of which factors influence the determination of groups to be served. Clearly, a variety of other comparative statics results are implied by the model, and these are discussed briefly in the conclusions.

The paper is divided into five parts. The first is a discussion of assumptions and definitions, followed by a version of the model that accounts for an incumbent legislator's choice of constituency. Section three examines the decision of interest groups to allocate their scarce resources among legislators. The fourth section examines the implications of the rationally ignorant and civics class assumptions about voter behavior for public policy outcomes. The last section is devoted to a summary and a discussion of extensions.

The Agents

Legislators

Legislators are assumed to maximize the number of votes they receive in the next election. While not literally accurate, maximizing votes is a sensible proxy, because a large margin in any given election affects the margin in the following election. That is, even if the legislator cares only about winning in each of the next several terms, he still acts as if he were maximizing votes in any contest except the last. As Mayhew (1974, p. 46) said, the goal of the elected official is to

stay in office over a number of future elections, which does mean that 'winning comfortably' in any one of them . . . is more desirable than winning by a narrow plurality. The logic here is that a narrow victory (in primary or general election) is a sign of weakness that can inspire hostile political actors to deploy resources more intensively the next time around. By this reasoning, the higher the election percentage, the better.

Thus, maximization of vote received is a concise way to capture the motivation of a candidate involved in a campaign in which he is unsure of the identity or even of the probable characteristics of his opponent(s). Under these circumstances, strategic choice of activities reduces to a game against nature: the legislator actively searches for new ways to serve constituencies, impelled not by an actual opponent, but by the knowledge that he will face an attractive, well-financed challenger if he does not behave in this way.¹

In order to influence votes received, legislators can provide services to any of n organized interest groups, or provide constituency services to the voters in their geographic district, by allocating a limited amount of effort to producing these services. This taxonomy was chosen over that of Löwi (1964) and others because their approaches involve a demand-oriented categorization of types of policy. In order to model more closely the sources of the services, we chose to use types of effort. Services to interest groups may include actual legislation, private bills, influence on the way bills are written (or vetoed) at the committee level, and the assurance that the "right" sources will be called on to testify at important hearings. Constituency services include casework, dealing with bureaucrats on a voter's behalf, and other individual services (Loewenberg and Patterson, 1979, pp. 187-90).

Voters

Votes come from voters, a heterogeneous, unorganized group. They respond

to the activities of the legislator, both in the legislature and in the district. In addition, because voters are assumed to be largely uninformed, resources devoted to advertising affect their responses. The type of advertising must be recognized as important. We acknowledge the legitimacy of Fiorina and Noll's (1979) criticism: "A major current area of inquiry in the legislative subfield is to explain the apparently overwhelming advantage of incumbency. Most . . . explanations seem to conflict with the basic spirit of rational choice majority rule models, in that citizens are implicitly assumed to respond to advertising regardless of content." It is our view that candidates do not accept this Galbraithian perspective, but rather have powerful incentives to control carefully the content of their advertising activities, both for unorganized groups and interest groups.

Interest Groups

Each interest group is assumed to be interested in only one policy. It offers a legislator campaign resources, in exchange for the expectation of future services. The definition of *resources* to be used here is Dahl's (1961, p. 226): "[A] resource is anything that can be used to sway the specific choices or the strategies of another individual." So resources include money, in-kind services, volunteer labor, etc.

Each of these three agents is assumed to be a constrained maximizer: Legislators maximize votes, subject to the constraint of total effort they can allocate. Voters' utility maximization, for our purposes, is completely embodied in the way they cast their one vote. Interest groups allocate scarce resources in the way that best furthers their goals.² All are constrained by the rules of the legislative institution, which is assumed to be a unicameral, majority-rule congress, with members elected from geographic districts.³

The Single Legislator Model

Consider an individual legislator who chooses activities that maximize the number of votes he expects to receive in the next election. Suppose there are n organized single interest groups, each of which can provide the legislator with campaign resources. That is, each group, i , can provide campaign resources, R_i , that the legislator will use in his next election campaign. In addition, there is a large unorganized group, U , each member of which has a vote to cast; aggregating these votes gives us the total voting resources, V_U , of the unorganized group. The unorganized group is assumed not to contribute campaign resources. The legislator then can choose from $n+1$ activities or types of policy services: constituency services— P_U —to group U , or policy services— P_1 through P_n —to the n single interest groups.

In order to decide which groups to serve, the legislator considers the total votes he will receive for each possible strategy. That is, he calculates whether he will devote effort, E_i , to produce each of the categories of service, P_i , where $i = 1$ to n . In making these calculations, he considers the response of each group to his activities:

for single-interest groups:

$$V_i = 0, i = 1 \text{ to } n \quad (1)$$

$$R_i = R_i[P_i(E_i)], i = 1 \text{ to } n; \quad (2)$$

total campaign resources:

$$R = R_1 + R_2 + \dots + R_n; \quad (3)$$

for the unorganized group:

$$R_U = 0 \quad (4)$$

$$V = V[P_U(E_U), P_1(E_1), P_2(E_2), \dots, P_n(E_n), R]. \quad (5)$$

The explanation of the first four functions is straightforward. Equation (1) reflects the assumption that interest

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groups are nonvoting.⁴ The second expression means that campaign contributions from interest group i depend on the level of services provided, where service P_i is a function of the effort E_i devoted to it. For example, E_1 is the legislator's effort devoted to serving the first group, $P_1(E_1)$ is the policy outcome the legislator can produce by E_1 , and $R_1[P_1(E_1)]$ is the amount of contributions from group 1 the legislator would receive if he produces $P_1(E_1)$. If group 1 is the Sierra Club, E_1 might represent time spent in committee on an Acid Rain Prevention Bill; $P_1(E_1)$ the impact the legislator has on the bill through this effort; and $R_1[P_1(E_1)]$ the campaign contributions the legislator expects to receive from the Sierra Club as a result. The identity (3) claims that the total campaign resources the candidate receives are simply the sum of the resources contributed by each of the n interest groups. The assumption that the unorganized group contributes no campaign resources is reflected in equation (4).

In summary, the first five equations represent the assumptions of the model. The analysis is simplified greatly by dividing the activities of voting and of contributing campaign resources. Voters do not make contributions, and interest groups do not vote in this model. Interest group contributions are determined only by the promised level of effort of the legislator in the single dimension the group seeks to influence; there are no multiple-issue groups in the model.

The last function requires more careful scrutiny. Intuitively, equation (5) tells us that the unorganized group votes in response to three separate influences. First, effort devoted to producing constituency services, P_U , affects their votes. Next, the unorganized group responds also to all other policies 1 through n . That is, an individual, though not a member of the Sierra Club, may have strong preferences over the Acid Rain Prevention Bill.

The vote cast by that individual may be affected by the legislator's activities in that direction, be it writing the bill, debating, or casting a roll-call vote. Third, the unorganized group responds to advertising or campaign expenditures made from R , the total resources contributed by interest groups, assuming each candidate uses the resources he receives in the optimal way.

Having developed the response functions of the groups, the next step is to characterize the strategy of the legislator to maximize votes, given a limit, \bar{E} , on the effort he has to allocate. For our purposes, *effort* is simply the amount of the legislator's time, the time spent by staff, and the amount of office resources a legislator allocates to an activity, and \bar{E} is the total amount of effort he has available to allocate. The problem, then, is to choose the $n + 1$ levels of effort so as to maximize

$$V[P_U(E_U), P_1(E_1), \dots, P_n(E_n), R],$$

providing that

$$\bar{E} = E_U + E_1 + E_2 + \dots + E_n,$$

(total effort equals the constraint \bar{E}). For simplicity, let this symbol denote derivatives: ' ; let superscripts denote partial derivatives; and suppose there are but three groups, U , 1, and 2. The necessary conditions for a solution imply:

$$\begin{aligned} \lambda &= (V^U)P_U' = (V^R R_1' + V^1)P_1' \\ &= (V^R R_2' + V^2)P_2', \end{aligned} \quad (6)$$

where λ is the *shadow price* of effort. The overall meaning of equation (6) is quite sensible: a unit of effort has the same marginal effect on total votes, regardless of which activity it is devoted to—the usual result in maximization problems. If \bar{E} is large enough, effort has no alternative use ($\lambda = 0$) and the terms in equation (6) equal 0, meaning that the legislator gets every vote he possibly can. If the constraint is binding, then $\lambda > 0$, and devoting effort

to one activity now has a positive opportunity cost in terms of votes foregone by not engaging in some other activity. In this case, the maximizing strategy is to devote effort to each activity until the marginal gains are all the same.⁵

Each term in parentheses in equation (6) is the marginal vote productivity of a type of policy—that is, the change in vote received in response to a change in policy. The last two are the most interesting: $V^R R_1'$ represents the vote-producing effects of a change in resources (V^R) multiplied by the total change in resources in response to a change in services to group 1 (R_1').⁶ The term V^1 is the vote-producing effect of policy service to interest group 1. Each P_i' , the change in the amount policy i produced with respect to the change in effort devoted to producing it, is a measure of the legislator's relative ability to produce policy services for each of the three groups. Thus, the equality of the four expressions means that the productivity of effort in producing outcomes multiplied by the productivity of outcomes in producing votes is the same, for all types of effort. In addition, each expression equals λ , reflecting the fact that devoting effort to one activity has the opportunity cost of not devoting that effort to some other activity.

The legislator's decision problem is to allocate effort, given the vector of effort productivities, so as to maximize the probability of winning the election, subject to the constraint that total effort expended, $\sum_{i=1}^n E_i$, is less than or equal to \bar{E} . There are two potential difficulties with this formulation: the question of how the limit is fixed, and the possibility that the legislator's staff provides much of the services that affect votes and resource contributions. Fenno (1978, p. 34) claims that

time is a House member's scarcest and most precious political resource. If there is an exemplary congressional complaint, surely "there isn't

enough time" must be it. In deciding how to spend his time . . . a member confronts his most difficult allocative dilemma. . . . Different representatives make different allocative choices and different allocative trades.

The setting of the effort level is beyond the scope of this paper. However, the existence of a binding constraint on the legislator's effort is not implausible. The implication is that the candidate's time has some opportunity cost and is not valued at zero at the margin.

Still, we must consider the question of staff effort. As Fenno points out (1978, p. 34), it is not true that

"all" the congressman has is time and himself. The office carries with it a large number of ancillary resources—a staff, office expenses allowances, free mailing privileges, personal expense accounts. . . . Each congressman chooses how he will use these resources. The most significant of these choices involve the use of staff.

The choice of staff size, like the constraint of effort, is not treated here. Staff effort is included within \bar{E} , the total effort the legislator has available. In sum, legislators choose how much time they themselves will spend working for reelection, and how large a staff they will retain once this choice is made. All of the available effort is allocated in such a way as to maximize the likelihood of reelection.

Choice of Constituency

It is important to understand the definitions of *constituency* used in this paper. The usual definition is those groups providing votes or campaign resources to the legislator. However, this definition does not really identify the choices available to the legislator in the decision about which set of groups to serve actively. There may well be groups that vote for, or donate resources to, the legislator, even though he does not actively serve them, and so these groups are not in his "constituency." In order to identify and separate the various constituencies, we will use the following definitions:

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- a) The *geographic constituency* is that part of the unorganized group of voters which votes for the legislator (see Loewenberg and Patterson, 1979, pp. 170-72; Fenno, 1978, pp. 1-8).
- b) The *service constituency* is that set of groups the legislator actively devotes effort to serving.
- c) The *resource-supplying constituency* is that set of groups supplying the legislator with campaign resources.

Notice that these are not necessarily the same: interest groups provide no votes, but may be served in exchange for resources; voters may not be actively served, but may vote for the legislator anyway. Further, interest groups may choose to offer resources to a legislator simply because of his views. Such a legislator receives resources not because of any expected quid pro quo, but because the group would like to see him stay in office.

Let $\hat{E} = (\hat{E}_U, \hat{E}_1, \hat{E}_2)$ be a solution to the three-group maximization problem. Suppose that, in addition to serving group U , the legislator also provides policy services to group 1, but not group 2. Notice that since the heterogeneous voter group responds to all policy dimensions, the *unorganized group* can affect the legislator's decision on whether or not to provide policy service to *interest groups*.

Let us examine next the case where one group is served and another is not. For example, if, contrary to equation (6), $\hat{E}_1 > 0 = \hat{E}_2$, we know that:

$$(V^1 + V^{RR_1'})P_1' > (V^2 + V^{RR_2'})P_2' \quad (7)$$

(policy service effort for group 1 produces more votes than it does from group 2). There are several cases where this might occur, the simplest of which is an absolute advantage in producing policy for group 1 ($P_1' > P_2'$, or, the marginal productivity of the legislator's effort is greater for P_1). That is, the legislator has some extra ability to produce P_1 as a committee member or chairman, a leadership posi-

tion in the Congress. Also, he may have more knowledge about group 1 policy service because of prior experience, the location of his district office, etc.

If the legislator has no such advantage ($P_1' = P_2'$), equation (7) reduces to

$$(V^1 + V^{RR_1'}) > (V^2 + V^{RR_2'}) \quad (7')$$

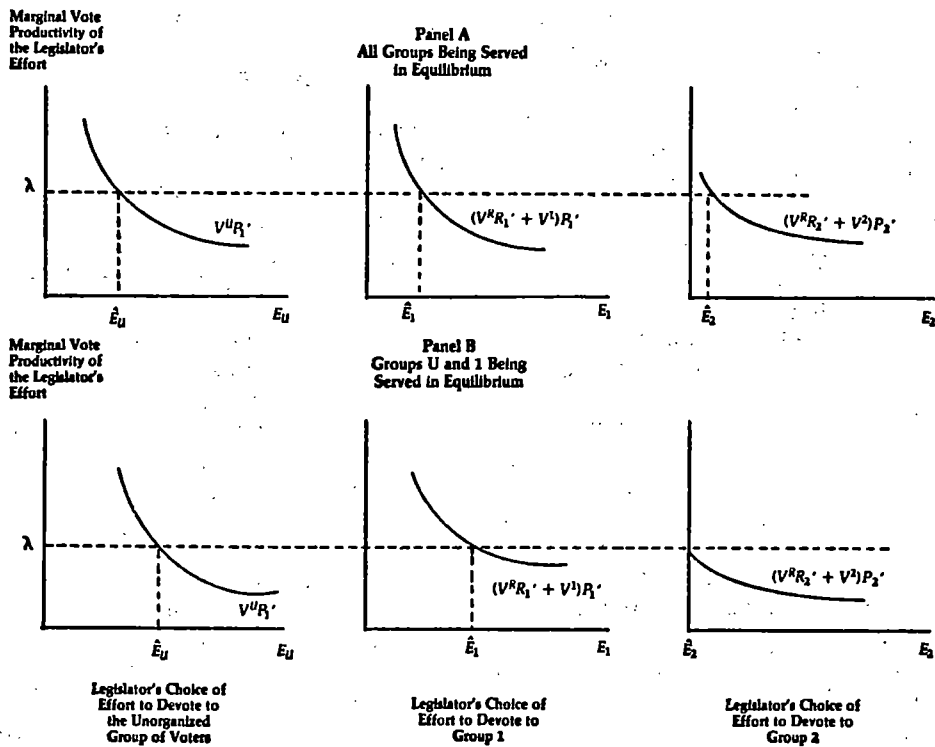
Each of the remaining cases will be examined separately.

($V^1 > V^2$): The voting response of the unorganized group is greater for policy 1, completely separate from the interest group response in providing resources. Thus, in deciding which interest groups to serve, the legislator must consider the response of the large, unorganized group.

($R_1' > R_2'$): Interest group 1 will provide more campaign resources in response to policy service than will group 2. In this simple case, we are assuming that there is no backlash from the unorganized group against a legislator who accepts campaign contributions other than that summarized in V^1 , which can be large and negative. That is, the unorganized group makes its voting decisions based not on whether the legislator accepts money, but on what he does in order to elicit the contributions in the first place. The implication is that $V^{RR_1'} > V^{RR_2'}$, or that the votes the legislator can get from the unorganized group by advertising, multiplied by the marginal resource response he gets by producing policy services, is greater for group 1 than for group 2.

We can graphically depict the overall decision process of the legislator in choosing which group(s) to serve, assuming no two interest groups seek to influence the same policy. This graphical solution is shown in Figure 1, Panel A: at a maximum, the marginal vote productivity of each type of effort must be the same, and it must equal λ , the opportunity cost or shadow price of using effort for an alternative. In Panel A, all groups are served with positive levels of effort, but this need not be the case. If the marginal vote pro-

Figure 1. Legislator's Choice of Effort Allocation



ductivity of serving some group is very low (always less than λ , the opportunity cost of using that effort somewhere else), then that group will not be served. This situation is depicted in Figure 1, Panel B, where the legislator provides no policy service to group 2. The previous discussion showed that this can occur when (a) the legislator is not very good at producing P_2 , whether because he is not on the "right" committee, is not knowledgeable, etc.; (b) because the unorganized group is indifferent or opposed to policy 2, offsetting the benefits from providing it; (c) interest group 2 provides few campaign resources in exchange for policy 2.

A Special Interest Group's Choice of Legislators

Thus far, we have modelled interest groups as supplying resources and votes in exchange for the policy and constituency services the legislator can provide. However, interest groups can be viewed as active seekers of those services from legislators. If interest groups are not passive, but actively seek legislators whom they believe can help them, then the situation looks quite different from that of the previous sections.

Suppose each interest group desires to achieve a set of goals, and makes its cam-

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paign resource contributions in the way it believes best accomplishes these goals. For example, the group may simply wish to have a legislator who will sponsor a bill when asked, which may be more effective if that legislator is on a particular committee or subcommittee. Alternatively, the group may want to be the first to know when legislation inimical to its interests is being considered. The group might want to be able to block or amend legislation. For other groups, it might suffice to be certain that the "right" people will be called to testify at committee hearings.

Recalling the earlier formulation, the maximization problem from the point of view of the legislator is: Maximize

$$V[P_U(E_U), P_1(E_1), P_2(E_2), \dots, P_n(E_n), R]$$

subject to

$$\bar{E} = E_U + E_1 + E_2 + \dots + E_n$$

The first order conditions of this problem relating to interest groups are:

$$(V^R R_i' + V^i) P_i' - \lambda = 0, \quad i=1, \dots, n. \quad (8)$$

From the earlier discussion, recall that the terms inside the parentheses are the vote productivities of the n types of policy service. P_i' is then the productivity of effort for each policy. If we again consider only a single legislator, these expressions can be manipulated to illustrate the minimum price the legislator will accept to provide a unit of effort. This is done by solving for R_i' in the above equation. Given the usefulness of campaign resources (V^R), the marginal productivity of effort (P_i'), the reaction of the voters (V^i), and the opportunity cost of effort (λ), the quantity of resources a legislator will require to provide one unit of effort to the i th group is:

$$R_i' = \frac{\lambda - P_i' V^i}{V^R P_i'} \quad (9)$$

This expression can be separated into two parts:

$$R_i' = \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R}; \quad (9')$$

(a)
(b)

Part (a) of expression (9') is fairly easy to understand. Since λ is constant across interest groups, and assuming V^R is constant (voters everywhere react the same way to advertising), only P_i' differs across legislators. Remember that P_i' is the marginal productivity of the legislator's effort in policy service to group i . Since P_i' appears in the denominator, the better the legislator at producing the i th policy service (i.e., the fewer hours he must devote to producing the same amount), the lower is the minimum price he will accept to provide that service. If we compare the legislator's price for services to groups 1 and 2, and look only at part (a), then if $P_1' > P_2'$ (the legislator is better at producing P_1 than he is at producing P_2) we have

$$R_1' = \frac{\lambda}{V^R P_1'} < \frac{\lambda}{V^R P_2'} = R_2'.$$

The more adept the legislator is at producing policy services for a group, the lower the minimum price he will require for doing so.

Part (b) adds an additional consideration; it is the ratio of the vote productivity of P_i to the vote productivity of resources. The larger this ratio, since it is subtracted, the smaller the supply price of that policy. Intuitively, this is what we would expect. The greater the voting response of the unorganized group to policy i , the fewer resources the legislator will demand to produce policy i . We might think of legislators whose geographic constituency is in favor of P_i as having a comparative advantage in producing P_i , at least from the point of view of interest groups.

Alternatively, if the unorganized group is very much opposed to policy i (that is, if $V^i < 0$), then this raises the supply

price. Thus, the disadvantages of serving unpopular interest groups by producing policy helpful to them is built into the model.⁷ Only if the interest group can provide large resource contributions will publicly unpopular policy service be provided.

A concrete extension of the problem of the interest group's choice of legislators is the case in which the interest group hopes to build a majority coalition of some group (a committee, subcommittee, etc.). If, for example, group 1 hopes to get a bill past the committee stage, it would form a ranking of the supply prices of the members of the committee. If there are k committee members, the ranking is from 1 (the lowest price) to k (the highest price). Notice that some of the lowest prices may be *negative*, meaning that the unorganized constituency of that legislator is so much in favor of that bill that he would have to be paid not to vote for it. Clearly, group 1 need allocate no resources to these legislators, because their support is "free." Beginning at the first price above zero, group 1 pays this price, and the next, until it has secured a majority. Suppose for example that there are seven committee members, and the supply prices are ranked as follows:

Legislator	Supply Prices
1	-\$5000
2	- 2000
3	- 500
4	100
5	750
6	1500
7	2000

A majority on this committee can be had for \$100; only this contribution to the fourth committee member need be made in order to gain a majority and ensure passage through the committee.

Unfortunately, the example thus far is flawed in an important respect: it assumes there are no competing interest groups, so that interest group 1 can act as a monopolistic purchaser of policy services. Sup-

pose there is a diametrically opposed group 2, so that $P_1 = -P_2$. That is, any policy service favorable to group 1 is unfavorable to group 2. For example, suppose the committee is the House Interior Committee, group 1 is the Sierra Club, and group 2 is the Chemical Manufacturers Association (CMA). The supply prices are the minimum the legislators will accept to provide P_1 or P_2 ; the actual price paid is likely to be bid up by the two competing groups.

To illustrate this, suppose the payoffs are the same as above, and that the committee must either report the bill to the floor ($P_1=1$, $P_2=-1$), or not report the bill ($P_1=-1$, $P_2=1$). Then the ranking is as follows:

Legislator	P_1 supply prices (lowest to highest)	P_2 supply prices (highest to lowest)
1	-\$5000	\$5000
2	- 2000	2000
3	- 500	500
4	100	- 100
5	750	- 750
6	1500	- 1500
7	2000	- 2000

If group 2 did not exist, then group 1 could buy $P_1=1$ for \$100, because then L_1 , L_2 , L_3 , and L_4 would vote for it. If group 2 did not exist, then the CMA could buy $P_2=1$ for free, because L_4 , L_5 , L_6 , and L_7 all would have to be paid not to vote that way anyway. If, however, both groups do exist, and actively bid, it is quite conceivable that the legislators who are relatively indifferent (L_4 , then L_3 and L_5 , and so on) will receive a considerable amount of campaign resources.⁸ The point of this example, however, is that although the unorganized group appears nowhere as an explicit agent in the bargaining over the policy outcome between the legislator and interest groups 1 and 2, the preferences and expected voting reaction of the unorganized group are implicitly embodied in the schedule of supply prices above, as we saw from (9) and (9').

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The results of this section can be summarized as follows. Interest groups choose among legislators based on the supply price of the service each group seeks. The supply price is determined by the "cost" to the legislator of providing the service. This cost depends first on the productivity of the legislator's effort. Productivity is affected by committee assignments, seniority, and the legislator's skill in using legislative institutions. Thus, the first component of cost is whatever the legislator gives up by serving an interest group and not using his time on something else. The second component of cost reflects voter preferences. A legislator seeking reelection must be compensated for votes lost by serving an interest group before he will provide such services. The amount required varies with the distaste of voters for the policy. In sum, the supply price or cost of policy to an interest group varies across legislators. Legislators have a comparative advantage in supplying policy on issues in which either (1) their effort is highly productive, or (2) their own voters are relatively more favorable toward increases in the policy. The following section illustrates a comparative statics implication of the model, based on the reactions of voters to changes in policy.

Rational Ignorance vs. Civics Class/Full Information

The discussion to this point has been concerned with legislators and interest groups struggling to achieve their own goals, and the ways in which their success can be affected and constrained by the large unorganized group of voters. No dominance or control has been ascribed to any of these agents; instead, the general framework encompasses the possibility that any one group or more may have the major influence on policy outcomes. In the context of the model, outcomes are determined by the prices, or costs, at

which policies are offered by legislators. In order to illustrate the comparative statics predictions of the model, this section considers the supply price of policy under two alternative extreme assumptions about voter behavior. The two assumptions we consider are (a) rational ignorance, and (b) civics class/full information.

Rational Ignorance

One possibility for voter behavior is that voters invest very little in political information gathering. Resource contributions are the primary vehicle for determining policy outcomes. Voters in the unorganized group are ignorant of the activities of legislators in serving interest groups. According to this view (Becker, 1976, p. 37):

since each person has a fixed number of votes—either 1 or 0—regardless of the amount of information he has and the intelligence used in acting on this information, and since minorities are usually given no representation, it does not "pay" to be well-informed and thoughtful on political issues, or even to vote . . . candidates for many offices, such as the presidency and state governorships, must have enough resources to reach millions of voters. Many groups that would like to compete for these offices do not have sufficient resources to reach large numbers of voters.

Thus, legislators must depend primarily on interest groups to provide them with enough resources to be able to make themselves known to the uninformed voters. By actively serving interest groups, legislators ensure interest group primacy in policy. Because voters are completely ignorant (until provided with biased information—Calvert, 1982), legislators can use these resources to advertise themselves with little concern about adverse voter response.

In the context of the model developed in this paper, *rational ignorance* implies that voters respond only to resource expenditures, because they are "rationally ignorant" of everything else. Thus, since

only resources can mobilize votes, each legislator maximizes the resources he receives from interest groups, each interest group chooses legislators to contribute to on the basis of their policy productivity alone, and voters respond passively to advertising expenditures. The response functions of the agents look as follows: for voters,

$$V = V(R)$$

(policy has no effect; votes exchange for resources); for special interest groups,

$$R_i = R_i[P_i(E_i)], i = 1 \text{ to } n$$

(payment for legislator's effort); and for legislators,

$$R_i' \geq \frac{\lambda}{VRP_i'} - \frac{V^i}{VR} = \frac{\lambda}{VRP_i'}$$

(because $V^i=0$; no voter policy response). Only P_i' differentiates legislators, because λ and VR are assumed constant. Notice that, as before, voters do not make contributions and contributors do not vote.

In this model, interest groups can almost completely control the legislator's activities. Those who act to further what they believe are the interests of their geographic constituency are never reelected, unless it happens that some interest group is in favor of the policy as well, and contributes resources. Legislators who act against the interests of their geographic constituency, but in accordance with interest group wishes, can lose an election only if the challenger has a higher policy productivity of effort in those policies, and can thereby win resources away from those less able, or with a smaller endowment of valuable committee assignments. The predictions of the model for desired committee assignments and voting patterns are quite simple (see Peltzman, 1982): assuming voters are rationally ignorant, both committee assignments and voting patterns chosen by legislators will be those which maximize the campaign resources received. In fact, the best

strategy for a legislator is to keep his service constituency and his geographic constituency completely disjoint, so that voters are never served at all, unless they benefit from interest group resources spent in their districts.

The rational ignorance assumption model provides an interesting benchmark in extreme interest group influence, but is not a very useful view of the world. Notice that if voters are not ignorant about a legislator's policy activities in any policy category, interest group influence is affected. Putting this point in the context of the price an interest group must pay a legislator to provide effort on its behalf, let us reproduce (9'):

$$R_i' \geq \frac{\lambda}{VRP_i'} - \frac{V^i}{VR} \quad (9')$$

Voter's policy preference appears in the V^i term, the change in vote received in response to a change in policy i . If, for some geographic constituency, this term is negative (voters are not ignorant and are violently opposed to this policy), the interest group may find it either very expensive or impossible to purchase that legislator's services. This point can be extended to account for the role of the media or potential competitors: even if voters are currently ignorant of the activities of a legislator in serving an interest group, the legislator's actions may be constrained by the knowledge that the media or a competitor will expend resources to make voters aware. That is, the legislator must consider not only the reaction of voters given their present knowledge, but also the expected reaction if voters were to find out. The role of potential competitors and the news media in policing the actions of incumbents in this way has been noted by Mayhew (1974) and others, including MacKuen (1981, 1983).

Thus, another consideration enters the interest group's choice of which legislators to contribute to—the preferences of a legislator's geographic constituency.

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Legislators whose geographic constituency has strong, informed preferences about some policy will receive resource contributions from an interest group only if voters and the interest group want the same thing; otherwise, the interest group will search out some other legislator whose geographic constituency is either ignorant or indifferent (Weingast and Marshall, 1984). Further, given the incentives for challenges and the media to monitor and report on the activities of legislators, the ignorance of the typical voter may still provide little room for discretionary behavior on the part of the legislator. In any case, the empirical prediction that the service constituency and the geographic constituency are disjoint is simply wrong: the intersection of the two groups is exactly those voters who are informed, have policy preferences, and who vote. The size of this group is an empirical matter; it is probably wrong to suggest *a priori*, as Becker (1976) seems to, that the group does not exist.

Civics Class/Full Information

The *civics class* model of voter behavior developed in this section is conceptually the opposite of the rational ignorance model just described.⁹ The overriding concern of the legislator, under the assumption that voters act the way they are depicted in a high school civics class, is to discover the preferences of what we have called the geographic constituency, and then to act so as to serve these preferences. As shown below, interest groups have very little influence in such a scheme. Fully informed voters imply a model of legislative behavior where the service constituency and the geographic constituency are identical; interest group service is at most an artifact or coincidence resulting from the preferences the legislator is actually serving.

If voters are well-informed and have strong preferences, advertising has little effect, so that V^R , the partial derivative of

the vote function with respect to resource expenditures, is near zero. The response functions are then as follows: for voters,

$$V = V(P_L, P_1, \dots, P_n, R);$$

for special interest groups,

$$R_i = R_i(P_i(E_i)), \quad i = 1 \text{ to } n;$$

and for legislators,

$$R_i' \geq \frac{\lambda}{V^R P_i'} - \frac{V^i}{V^R}.$$

V^R goes to zero, and R_i (the legislator's minimum supply price) becomes very large, moving toward either positive or negative infinity. No legislator can be induced to serve an interest group he would not already be serving. But this is an obvious consequence of our assertion that legislators maximize the number of votes they receive, and that effort is limited. If advertising resources produce no votes, and the legislator is prevented from using these resources for his own consumption, then he will not devote any effort to obtaining resources.¹⁰

Implications of the Models

Having illustrated the implications of the alternative assumptions of complete voter ignorance and full information, it is possible to address the question of the behavior implied by each. First, if voters are ignorant, it is clear that preferred committee assignments for legislators bear no resemblance to geographical constituency interests. Seniority, committee chairmanships, etc., are important because of their value as capital assets, capable of generating resource flows that are indirectly translated into votes through advertising. If voters are informed, however, there are very different implications for committee preferences. The skills of the legislator, prior knowledge, and other factors are of secondary importance. The primary goal

is a committee position which enables the legislator to serve his geographical constituency. Interestingly, however, seniority and committee leadership positions are still very valuable, even if they are not capital, or resource-producing, assets: these things can provide votes directly, because they increase the effectiveness of the effort devoted to serving the geographical constituency.¹¹

The behavior of interest groups can be compared by examining the supply price of a legislator implied by each assumption. Uninformed voters imply that a legislator's willingness to exchange effort devoted to legislative services for resources is affected only by the response of his geographic constituency to advertising, the opportunity cost of effort, and the productivity of the legislator's effort:

$$R_i'(\text{uninformed}) = \frac{\lambda}{VRP_i'}$$

In this model, interest groups determine policy outcomes by bidding for a legislator's effort.

Interest group behavior under full information (assuming $VR=0$, or that advertising is ineffective) is quite different. A legislator's supply price, R_i' (when voters are fully informed) is simply:

$$\lim_{VR \rightarrow 0} \frac{\lambda}{VRP_i'} - \frac{V^i}{VR} = \pm \infty$$

(depending on the sign of $\lambda - P_i'V^i$), and thus interest groups can have no independent effect whatsoever. Interest group preferences and offers of resources have no impact on legislators' behavior.

Thus, neither assumption is completely consistent with the two most noted "facts" about the relations among legislators, special interest groups, and geographic constituencies. First, legislators choose committees which have jurisdiction over matters important to the geographic constituency (Niskanen, 1971; Weingast and

Marshall, 1984), contrary to the predictions of the rational ignorance assumption and consistent with full information. Second, as Dexter (1956, 1963) points out, interest groups choose legislators whose geographic constituencies have similar demands. Legislators elected from homogenous geographic constituencies will value committee assignments that are of direct importance to their voters: e.g., legislators from Kansas seek seats on the Agriculture Committee. To the extent that legislators elected from heterogeneous geographic constituencies need more resources in order to advertise, these legislators will pursue committee seats that help them affect legislation wealthy interest groups will notice. That is, optimal election strategies must vary at the margin with the composition and preferences of the voters in the geographic constituency.

Interest groups have a complicated problem in seeking out the legislator with the lowest supply price. However, as we have demonstrated, an interest group need not search very far for legislators with relatively low supply prices of effort, because those whose geographic constituencies are most in favor, or least opposed to, a policy are likely to be the cheapest source of interest group legislative services. The result, then, is that interest group policy manipulation is constrained by the preferences of the geographic constituency.¹² This constraint is imperfect, however, because voters are less than perfectly informed. Legislators themselves act in ways that do not perfectly represent their geographic constituency, in order to gain campaign resources from interest groups; these resources are used by each legislator both to portray himself in an attractive way and to inform voters. The voting group is not "organized" in Olson's (1965) sense, but legislators acting as political entrepreneurs can use the unorganized group to countervail interest group pressure (Wagner, 1966, p. 164).

The conclusion is that interest groups

do not control this process, but neither are special interests powerless. Contributions can have some influence on policies about which voters are divided, ignorant, or indifferent. The geographic constituency is not represented to the exclusion of all other groups, but departures by legislators from their voters' interests are constrained by the strong preferences voters have on some issues, and by the threat of informing and mobilizing public opinion that the news media and potential competitors always represent. In short, each of the extreme information assumptions offers a useful view of a small portion of the legislative process some of the time. Yet each is only part of a larger view of the complicated process exhibiting hidden, but very real, incentives that impel the activities of the political agents.¹³

Summary and Conclusions

This paper presents a model of the supply of government policy. The agents in the model include legislators, voters, and interest groups. The supply price, or cost, of policy is derived assuming that legislators seek to maximize votes, which can be obtained either by providing policy that voters favor or by serving interest groups in exchange for vote-producing campaign resources.

The price of policy for an interest group is shown to depend on the comparative advantage of the legislators offering to help provide the policy. The two factors determining comparative advantage are productivity and the preferences of the voters back home. Specifically, the more productive a legislator's effort, or the less hostile that voters are to a given policy, the lower the minimum price an interest group must pay in exchange. Since legislators differ in their productive capabilities (because of differences in committee assignment, seniority, and competence), and since the voters in their geographic constituencies differ in their preferences,

these prices vary in predictable ways across legislators. Interest groups can be expected to choose among alternative legislators on this basis.

Finally, an explicit comparative statics result of the model is derived. We ask under what circumstances will unorganized interests (i.e., voters) be represented. Two extreme alternative informational assumptions are considered. First, voters are assumed to be completely (though rationally) ignorant of the effects of a given policy on their welfare. The implications of this assumption are contrasted with a situation in which voters are fully informed. The supply price observed is demonstrated to differ, in a predictable way, between these two situations. To the extent that any voters are informed and opposed to a policy, an interest group seeking favorable treatment must pay a higher price than if all voters were indifferent or uninformed.

The conclusion is that unorganized, noncontributing voters may be effectively represented even in a situation in which interest groups are well organized and active. The representation of such voters, deriving from the institutional requirement of periodic reelection for legislators, tends to reduce the influence of organized but nonvoting economic interests in the political process. Thus, unorganized groups can shape and constrain decisions to a greater extent than that predicted by simple demand-oriented group theories of collective action.

Several extensions of the model presented above merit future consideration. The model is capable of addressing comparative statics questions such as the effects of changes in committee assignments or jurisdictions, or of changes in district boundaries. Committee assignments influence the policy productivity of legislators; redistricting modifies the vote functions that influence legislator and, implicitly, interest group decisions.

In addition, the model itself could be

extended in at least two ways. First, the source of the vote functions, or what others have called "political support functions," can be explicitly derived using a model of consumer voter behavior similar to that of Denzau and Parks (1979).¹⁴ Second, the equilibrium properties of the model, with voter preferences more clearly defined, can be explored in the context of a formal game to illustrate the implications for policy of interest groups competing directly against one another.

Notes

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1. Fiorina (1974, p. 48) demonstrates that when election is "assured," votes are not maximized if candidate resources have an opportunity cost. However, assuming the candidate hopes to win at least two elections, and that the constraint on effort accounts for opportunity cost, the election is not assured in Fiorina's sense; legislators in our model are "maximizers" rather than "maintainers."

2. At a more detailed level, this outcome is itself the outcome of a game involving members of the interest group. The very existence of one pattern or set of interest groups is an equilibrium of a complicated process that we take as exogenous. We will allow, however, for the possibility that an interest group has zero members, or that no organized group actually exists, for the reasons Olson (1965) discusses.

3. A model using assumptions similar to those described here has been developed by Cox, McCubbins, and Sullivan (1983). Using the reelection motivation for legislators and Fenno's taxonomy of constituent groups, they examine technological constraints on legislators' ability to choose forms of public policy implementation that improve their reelection chances. The orientation in the present paper, though arising from the same assumptions, is on the constraints placed on legislators' and interest groups' activities not by "technology," but by voters through the electoral mechanism itself.

4. The fact that interest groups provide no votes is not crucial to the results presented here. An earlier version of this paper included interest group voting, adding considerable complexity, but no qualitative difference in the results. Interest group voting was thus dropped for expositional simplicity. Dougan and Kenyon (1984) present a model in which interest groups provide both money and votes, and they obtain results qualitatively similar to ours.

5. Suppose this were not true, and that the last hour of effort devoted to service to the unorganized group produces 9 votes, while that devoted to group 1 produced only 6 votes. Then the legislator could increase his total votes by working one less hour on P_1 (he loses 6), one more on P_0 (he gets 9), so that there is a net gain of 3 votes.

6. This formulation is used for notational simplicity. Since $R = \sum R_i$, but only R_i is affected by a change in E_i , R_i' is the change in total resources in response to a change in the services provided to group i .

7. From the first order conditions, we can explicitly derive the conditions under which the candidate will "sell out." If $V^R R_i' P_i' > \lambda - V^i P_i'$, then $\hat{E}_i > 0$. Suppose $V^i P_i' < 0$ (geographic constituency is opposed). Then if serving group i produces more votes (through use of resources gained by $\hat{E}_i > 0$) than the sum of (1) the opportunity cost of legislator's time, and (2) the negative reaction of voters to policy i , the legislator will act directly against the interest of his geographic constituency. We would expect to see this occur when (a) the interest group being served is willing to pay large amounts, and (b) the legislator's marginal vote value of resources is high. Thus, poor campaigns are more likely to serve interest groups, to the detriment of voters.

8. William Riker points out that this example bears a striking resemblance to the reasoning used by Shapley and Shubik in deriving an abstract "power," or value index for use in predicting outcomes in small numbers bargaining games (see Schotter and Schwodiauer, 1980, for a review of the development of this concept, and for references). While a game theoretic treatment of the bargaining between interest groups and legislators is beyond the scope of this paper, the similarity in results suggests both the generality and usefulness of Shapley and Shubik's result, and an interesting direction for further research.

9. Fenno (1973), and Shepsle (1978) portray legislators as seeking committee assignments that tend to further their personal goals, one of which is reelection. Since we have abstracted away from non-election goals of legislators, this view is not here incorporated. A more sophisticated treatment, with legislators trading off votes against the ability to act on their own preferences, given some chance of reelection, could be incorporated as an extension of the present model (see note 1 above). For our purposes now, however, Fenno's (1978) description of a legislator's behavior is what we assume legislators believe about voters: that each has strong and informed preferences. In the present model, this problem is ignored: the $V(\cdot)$ functions are known. Clearly, an extension of the model is to allow some combination of the two extreme informational assumptions made in the text, perhaps allowing preferences to be unknown to the legislator, or else associated with some stochastic function.

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10. The consideration of "political advertising" given here is at most a preliminary treatment of a complicated problem. If, as has been suggested in the economics literature (Benham, 1973, and other articles reviewed by Comanor and Wilson, 1979), advertising does not persuade, but instead activates or informs existing latent preferences, then the usefulness of campaign resources depends crucially on what is being advertised. While this effect is implicitly built into the present model, the role of advertising on outcomes requires a more sophisticated theoretical treatment than that developed so far.

11. Weingast and Marshall (1984) suggest that the present system has evolved because of the recognition by legislators that political "gains from trade" can be realized. Drawing on the industrial organization literature, Weingast and Marshall suggest that the costs of enforcing implicit contracts between voters and legislators create incentives for legislators to seek the "best" committee assignments out of personal self-interest. Earlier versions of our paper included a rather long section on committee assignments and optimal assignments rules. However, the section was removed for reasons of length. For an application of the idea, see Munger (1985).

12. Mancur Olson (1965) demonstrates that opposing interest groups, either existing or potential, cannot be expected to constrain interest group activity. The "free rider" problem arising from the

shared benefits of organizing opposing interest groups implies the interest equilibrium of pluralist theory may well not occur at mean or median of all preferences; some groups will benefit at the expense of others, even if those others are partially organized.

13. An empirical implication of the model is that if it is cheaper and more effective for an interest group to try to affect those policies that the unorganized voters are in favor of, we would expect a pattern to develop over time in the growth of an interest group. A nascent interest group with a poorly developed resource base will try to affect those policies that are publicly popular and widely supported in the jurisdictions in which it organizes. As the interest group grows in the quantity of resources it can command, it should then begin to concentrate on less popular policy directions that provide more benefits for interest group members. In sum, interest groups may be public policy-oriented at the outset, but over time move toward more personal favors for interest group members. This proposition is to be developed and tested in future work.

14. The Denzau and Parks model expresses public sector preferences in terms of the private consumption possibilities set that they imply. The properties of these derived preferences appear quite tractable for incorporation within the present model.

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A POLICY-ORIENTED THEORY OF CORRUPTION

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*P*erspectives from political science and economics are drawn on to suggest an integrated theory of governmental corruption. The theory is oriented toward policy choices, and corruption is viewed as a product of individual and structural variables that interact to produce both positive and negative consequences. Individual-level considerations such as greed and the likelihood of detection and prosecution suggest one set of policies for reducing corruption. Bureaucratic constraints, citizen participation, and the congruence of legal structures and social demands offer a competing set of concerns that must be dealt with in analyzing corrupt practices. We show that corruption, as a process, influences the optimal level of social welfare. Alternative conceptions are examined, and a model is developed to evaluate policy choices related to corruption from the perspective of welfare optimization. Applied to the analysis of corruption, the model integrates general equilibrium theory, deterrence theory, and structural conditions. Finally, policy implications are considered.

*D*espite its frequent occurrence, governmental corruption has undergone surprisingly little systematic investigation. The theoretical literature has adopted either broad classifications of administrative corruption (Heidenheimer, 1970) or categorical definitions of a wide range of corrupt activities (Peters and Welch, 1978), and has relied largely on descriptive analysis in examining the causes and effects of corrupt behavior. As Werner (1983, p. 152) notes, "If the field of administrative corruption is to become more theoretical and less descriptive, it must develop a framework

and methodology that will permit comparative analysis." There remains a need for an internally consistent theoretical model and an analytical definition which would lead to important policy considerations.

This article examines corruption from the combined perspectives of political science and economics to develop a theoretical framework and methodology for policy analysis. Political science tends toward anecdotal accounts of individual cases of corruption that are useful in defining the concept, but provide little systematic analysis. Economics views

corruption within a model of rational individual choice, with little concentration on the overall impact on society. While both disciplines provide insight into differing aspects of corruption, neither alone produces a sufficiently satisfying explanation of the pervasiveness of corruption in society. In this study, by incorporating both perspectives we develop a coherent theoretical framework from which the causes and societal consequences of corruption can be assessed.

The first section examines previous interpretations of corruption, develops a definition appropriate to an interdisciplinary approach, and provides a dichotomous conception of the effects of corruption as being either beneficial or detrimental. In the second section the effects of corruption on social welfare are placed in the context of a general equilibrium model. This is followed by a description of a formal model that integrates general equilibrium theory, deterrence theory, and structural variables relevant to corruption. Public policy implications derived from this model are then explored. The major implication of examining the problem in these terms is that society may need to alter substantially its approach to dealing with corruption.

Problems of Definition

One of the difficulties with this topic stems from the lack of a widely accepted definition of corruption. Heidenheimer (1970, pp. 3-9) describes three types of corrupt behavior definitions: public office-centered, market-centered, and public interest-centered. Public office-centered definitions revolve around the violation of the public trust placed in the official. Market-centered corruption refers to the situation in which the official sees the position as an authority to maximize personal gain by dispensing public benefits. A public interest-centered definition of corruption stresses the violation of the common interest in favor of special interests

that provide direct or indirect benefit to government officials. In a similar vein, Peters and Welch (1978, p. 76) develop a four-part classification (official involvement, actual favor granted, payoff to official, and the donor of the payoff) in a survey of state senators to rank particular acts as more corrupt or less corrupt.

These classifications have merit in the description of a specific corrupt event, yet they lack the generality essential for an analytically useful definition of corruption. To fill this gap, our analysis employs a definition that incorporates alternative interpretations that have been developed in the literature. We define a corrupt act as any illegitimate use of public power or authority for private benefit. Such a concept subsumes all of the previous definitions and permits the analysis of a wide range of corrupt practices. It should be clear that this definition excludes "pork barrelling" and other forms of economically inefficient but politically attractive distributive policies, since they use legitimate (legislative bargaining) means to influence governmental behavior (Weingast, Shepsle, and Johnson, 1981).

A second problem with the literature on corruption is that it is largely descriptive and anecdotal. Sinclair's portrayal of the meat packing industry and Gardiner's assessment of organized crime in an American city are representative of the problem (Gardiner, 1970; Sinclair, 1908). While both works provide tremendous insight into specific cases of corruption, they provide little coherent or systematic theory concerning the topic.

What is lacking in most of the published material is a systematic mechanism for linking the causes of corruption with its consequences to generate public policy alternatives. Regardless of the debate over definition and outrage over specific examples of corrupt behavior, the question remains, "What should our society do about corruption?" To attempt an answer it is necessary first to examine the causes and consequences of corruption.

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Causes of Corruption

The causes of corruption can be viewed from two distinct perspectives; one involves individual characteristics while the other concentrates on structural influences. The individual-level explanation tends to view the corrupt act as the result of greed or the inability to withstand temptations on the part of weak or insufficiently ethical officials. Greed, or desire for gain, is indeed a strong motivator of human behavior, as the literature suggests. The Knapp Commission's study of police wrongdoing in New York City classified corrupt officers as either "grass eaters" or "meat eaters" depending on the extent of their personal greed (Knapp Commission, 1973). Gardiner and Olson (1974, pp. 274-81) discuss the inability to withstand temptation and problems of individual value systems as two of the major explanations of corruption. Rogow and Lasswell (1963, pp. 45-54) classify corrupt machine bosses as having either "gain" or "game" orientations toward politics. While these individual explanations provide valuable insight into why some officials can become corrupt, they are not satisfying explanations for the extent of corruption in our society, because they lack general applicability.

In order to provide a broader level of analysis we explore three categories of structural-level explanations: (1) bureaucratic or organizational (δ), (2) quality of citizen involvement (γ), and (3) congruence of the legal system to social demands (π). The first two are derived from the existing literature while the third is described in this paper for the first time.

The organizational or bureaucratic approach to corruption has a number of variations, but the essential theme is the inability to accomplish public purposes due to impediments created by bureaucratic organization. One variant of this theme has been labeled the "bottleneck

theory." The idea is that bureaucratic inertia can cause people to attempt to circumvent official channels through bribery or other illegitimate means to achieve a desired result (Johnson, 1982, pp. 22-24). Rose-Ackerman (1978, pp. 85-186) devotes nearly half of her book to examining how various bureaucratic arrangements influence the level of corruption.

The quality of political participation is also described as a deterrent of corruption. Nonpartisan ballots and at-large municipal elections were aimed at improving the quality of participation and breaking electoral support for ward politicians and political machines. In recent years, reelection of corrupt officials again raises questions about the quality of democratic participation (Rundquist, Strom, and Peters, 1977, pp. 954-63). If officials are not closely scrutinized, it would seem more likely that they would feel unconstrained in seeking their own benefit over that of the public.

A third structural view of corruption involves problems caused by the lack of congruence between the legal system and social demand, and can be seen in the cases of prostitution, gambling, and drugs. The law in the vast majority of the United States prohibits prostitution, gambling, and certain recreational drugs, yet at the same time there is a substantial demand for such services and products. If the legal system does not adequately respond to demands from the public or an intensely interested subgroup, then the likelihood of corruption increases. Officials are susceptible to being bribed to overlook violations that are desired by a segment of the public. In this situation, the political system, or more correctly, the lack of congruence between social demands and political outcomes, can be a cause of corruption.

A somewhat different problem concerning the legal system occurs if property rights are unclear, as it is possible for people to attempt to influence an official

illegitimately in a cause in which they feel they are protecting their property. An example of this type of situation is in the illegal pollution of a waterway by a company. Ownership of a river may be unclear, while the company is clearly owned by the shareholders. The pressure would be on the company to dispose of hazardous waste as inexpensively as possible. An environmental official is required to safeguard the public's interest in the river, yet the interest is not clearly defined. It is easy to envision a situation where an official does not seek aggressive enforcement in exchange for private benefit from the company. If property rights concerning the river or other natural resources were more clearly defined it would be expected that the owners of the property would vigorously protest its violation and require enforcement of the laws.

Consequences of Corruption

In addition to the causes of corruption, an integrated public policy approach requires an examination of the consequences of such behavior, so that scarce governmental resources can be efficiently utilized. The bulk of the corruption literature has described the consequences as being routinely negative. A 1966 article, "Corruption, The Shame of the States" (Wilson, 1966), is illustrative of the traditional self-righteous approach to the topic. There are a few notable exceptions to this approach, such as Merton's (1972) analysis of the social service functions provided to otherwise ignored immigrants by apparently corrupt political machines. Similarly, Nye (1967) attempts to evaluate the possible positive consequences of corruption for third world economic development. Both studies attempted to evaluate the consequences of corruption rather than assuming the consequences were negative.

Debate over the effects of corruption on society has lately been revived. A recent

study suggests that corrupt practices have a "spillover" effect on the remainder of society. The "spillover" takes a number of forms that lead to a lessened respect for the government on the part of the public. This effect is negative and is the putative consequence of administrative corruption (Werner, 1983, pp. 149-50).

To reduce the confusion over the effects of corruption upon the overall welfare of a society it is necessary to evaluate systematically the negative aspects of a corrupt activity in comparison with the advantages derived by the participants in the illegitimate exchange. With this approach it is possible to interpret corruption as having either a beneficial or detrimental net effect on society.

Beneficial Corruption

The concept of beneficial corruption is counter-intuitive and thus requires elaboration. When the production of desired goods and services in society is inhibited by structural inefficiencies (bureaucratic, legal, and political), corruption could serve as a means of attaining the desired outcome. The individuals participating in the illegitimate activity directly benefit from the transaction, while simultaneously raising social welfare by creating opportunities for production that would otherwise be restricted.

An example will be useful at this point. Prostitution is nearly universal, yet the activity is illegal in most locations. The demand for prostitution is at least extensive enough to raise large sums of money, which in turn generates lucrative bribes for vice control officers. The causes of prostitution are not clearly understood but are related to a number of societal conditions concerning sexual activity among consenting adults. Given the sexual revolution of the 1960s and 1970s, it may not be unreasonable to suggest that the laws prohibiting the performance of sexual activities for money lag significantly behind the actual level of tolerance

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in society. Assuming this to be the case, it is easy to see how a police officer could overlook his or her duty to enforce an outdated law because of a bribe or payoff.

The primary benefits in this example are derived by the customer of the service, the prostitute, the procurer, and the official who accepts the bribe. These benefits can be grouped into two categories: (1) the direct gain of the participants in the illegitimate transaction, and (2) the reduction of the likelihood of additional negative sanctions upon individuals performing a widely demanded service.

Possible externalities of corruption and equitable distribution of the benefits in society must also be taken into account. The negative externalities may produce long term disenchantment with the legal system and thereby harm society. Because these negative effects are distributed in a diffuse way with little direct damage to any particular individual, it is difficult to evaluate the extent of the harm.

The distribution of direct benefits in society is also an important determinant of the desirability of a corrupt transaction. If the outcome of the transaction differentially benefits an income group not requiring preferential treatment according to the distributive equity standards of the society, then the social desirability of the corruption would be lowered. In the case of prostitution it is difficult to determine the distributional impact with any specificity. One could argue, however, that the distribution resulting from corruption may be favorable because of the dual benefit that a prostitute can derive by being able to earn income and avoid imprisonment. Using a Rawlsian perspective, this distribution can be justified if it is assumed that the prostitute is among the least advantaged segment of society (Rawls, 1971). The distributional equity question must be examined individually for each situation in which beneficial corruption exists.

The idea of beneficial corruption does

not imply that these activities should be considered legitimate, but rather that they provide a net increase in social welfare. What is implied is that there must be a serious structural flaw for any corrupt act to lead to increased welfare. This is where the policy alternatives that will be described later come into play.

Detrimental Corruption

The concept of detrimental corruption is easier to reconcile with the bulk of the literature on the subject; however, our interpretation requires further elucidation. A particular corrupt activity will be considered detrimental in this analysis if the net impact on society is negative. An example, concerning the bribery of a mine safety inspector, will help to illuminate our conception of detrimental corruption.

A mine safety inspector is required to certify that conditions in a particular mining operation are safe. The cost of actually maintaining a safe mining environment is extensive for the mineral extraction company. The company might cut costs on its safety equipment by bribing a safety inspector to incorrectly certify that the mine is safe when in fact it is dangerous. The company and the corrupt official would profit from the transaction, but the risk of disaster for the miners would be enormous. Even if the profitable transaction between the company and the official were to last many years before a disaster actually did occur, the loss of life resulting from a mine cave-in or explosion would exceed any monetary considerations. The situation would be categorized as detrimental in our analysis, regardless of the income transfer among the parties, because of the health risk caused by the corrupt transaction.

The conception of beneficial and detrimental corruption described above is useful in exploring the nature of corruption intuitively, but it is hardly systematic. To develop a systematic mech-

anism for analyzing the impact of corruption on social welfare and to derive public policy alternatives, this conception needs to be articulated within a general equilibrium framework, since such alternatives involve society's use of resources.

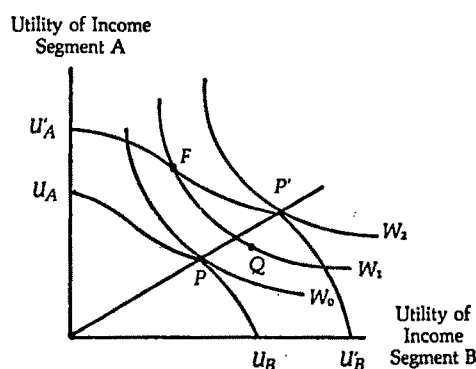
Corruption and Social Welfare

Consider a general equilibrium model that includes a social welfare function and the Pareto optimal conditions for the economy as a whole. In addition to the efficiency conditions most commonly stated for social welfare maximization, it will be assumed that society's production is organized in accordance with Pareto optimality; that property rights are sufficiently defined to assure every member of society the desired output mix; and that the interrelationship among production, consumption, and distribution is legitimized within a legal structure synchronized to the Pareto optimum setting.¹ The general feature of this model is that it extends the Pareto optimum and welfare maximum conditions to the organizational, political, and legal structures of a society. The public sector is assumed to act in direct response to society's preferences, and its involvement in the private market is limited to the conventional allocation, stabilization, and distribution functions that are largely exercised to maintain the Pareto welfare maximum.²

Under these conditions, any alteration in the public to private output mix will be assumed legitimate only if it is along the lines of the Pareto welfare maximum and is authorized by the prevailing legal system. In other words, changing a position on the Pareto welfare frontier will be a legitimate rearrangement if it results from a desired alteration in social preferences justifiable by the legal structure of the society; otherwise it will be illegitimate, which may dictate additional use of societal resources for its internalization.

This is demonstrated in Figure 1, where

Figure 1. Corruption and Social Welfare in a General Equilibrium Model



Key to Figure 1:

- $U_A U_B$ is a welfare frontier representing a possible distribution of real income (utility) between two income segments A and B.
- $U'_A U'_B$ represents the same distribution at a higher level of societal income.
- W_0 is the Bergson-Samuelson social welfare function.
- W_1 and W_2 the higher numbered curves represent society's distributional judgment at higher levels of social welfare.
- P and P' are allocations which simultaneously satisfy Pareto optimum and social welfare criteria.
- F and Q imply possible allocations and distributions which may result from beneficial corruption when society is initially at point P . Corruption will be detrimental if society can freely move to P' but is forced to move to F and Q or remain at P .

$U_A U_B$ depicts the welfare frontier and W the Bergson-Samuelson social welfare function.³ A line from the origin through point P represents the public to private output ratio most desirable distributionally. The allocation which corresponds to P simultaneously satisfies Pareto optimum and welfare criteria. Any deviation from this point would be a distributional (allocational if the welfare frontier moves outward) improvement only if it is desired by the society and accompanied by a suf-

ficient alteration in the organizational and legal structure. Thus, as shown in Figure 1, a move from P to P' would be an acceptable and legal one. Under the given assumptions, any other position off the frontier, say point Q , would be inefficient and illegal, leading to welfare losses shown as the difference between W_1 and W_2 on the diagram.

To add realism to the model, let us assume that society encounters obstacles in adjusting its organizational and legal structures to the preferred changes in the output mix.⁴ The public sector lags behind the private sector in adapting to new production technologies, and at the same time the legal structure fails to adjust quickly to the changing conventions, norms, and expectations of society.⁵

Given these changes, consider point P in Figure 1 and the following cases:

1. Society's preferences dictate an output mix other than P , yet the legal structure does not authorize such alteration;
2. Society is at point P , and, although the legal structure is permissive, there are bureaucratic obstacles which may delay a desired move towards another point on either $U_A U_B$ or $U'_A U'_B$;
3. Society is at point P , and, despite inefficiencies in the organizational and legal structures, there is no tendency for society to look for another resting point on either frontier; and
4. Society can freely move from P to any other point on either frontier without being affected by organizational and legal inefficiencies.

In all four cases initiating forces will be present to move society to another resting point, either through legitimate or illegitimate means. The forces which use illegitimate means will become the sources of corrupt practices CP . This means that transformation of societal resources and alterations in the public to private output ratio will result from corrupt practices only if they are caused by an unauthorized collaborated effort between public

and private sector producers. These practices correspond to the interpretation of corruption which is used in the current literature, and incorporate activities associated with the office-centered, market-centered, and public-centered types of violations reviewed earlier.

Note that the illegitimate means in case 1 and case 2 result in increased welfare in the Pareto sense only if the social change is on the welfare frontier and stays along the lines of the social welfare function. This would correspond to the notion of beneficial corruption presented above. In cases 3 and 4, illegitimate means will lead to welfare losses, which we define as detrimental corruption. It is likely that both welfare effects will be present in any corrupt practice. The determination of whether a particular corrupt transaction is beneficial or detrimental will be based upon the net impact on social welfare.

Beneficial corruption is viewed as a "good" in this model because of its positive net impact on society's welfare. Corrupt practices shift $U_A U_B$ outward as indicated in cases 1 and 2, and restore a public to private output ratio at a higher welfare level shown by the tangency point P' . This point will be maintained only if society incurs no additional costs related to corrupt practices. When such costs exist, society will restore the equilibrium at a welfare level lower than P' but higher than initial point P , primarily because of the costs associated with the corrupt actions and the spillover effects on the rest of the economy. Consider, for example, the welfare losses $W_L(SC)$ that may result because of the external diseconomies (SC) associated with beneficial corruption, such as the negative implications following from the violations of the rules and norms of a society. Also, assume that society incurs additional losses $W_L(R_1)$, due to resource use in corruption prevention, and $W_L(R_2)$, due to resource use in the corrupt sector to conceal corruption and minimize punishment if apprehended.

When these costs are significant the welfare frontier moves inward, restoring a public to private output ratio at a lower level of welfare. For corruption to be beneficial under these conditions the welfare gains from corruption (W_G) must exceed the welfare losses (W_L). This means

$$\frac{\partial W_G}{\partial CP} > \frac{\partial W_L}{\partial CP} \quad (1)$$

or more specifically,

$$\frac{\partial W_G}{\partial CP} > \left[\frac{\partial W_L(SC)}{\partial CP} + \frac{\partial W_L(R_1)}{\partial CP} + \frac{\partial W_L(R_2)}{\partial CP} \right] \quad (2)$$

Note that alteration of public to private output ratios could lead to welfare gains, but at the same time change the distributive equity. As shown in Figure 1, when the initiating forces in cases 1 and 2 restore a Pareto-efficient allocation at point F , income distribution improves in favor of income group A , but at the same time reduces social welfare to the level represented by W_1 . From an economist's perspective such changes in resource allocation are still beneficial as long as the new equilibrium F represents higher welfare than the initial allocation P .⁶

It should be emphasized that the magnitude of welfare loss due to distributional effects will depend on society's notion of fairness. From a utilitarian distributive equity approach, corruption will be beneficial as long as it adds to welfare despite its adverse impact on income distribution. An egalitarian will tolerate such corruption if it improves equality of income, and so will a Rawlsian, as long as the utility of the least advantaged group in society is maximized. Under each of these equity assumptions, the rule for beneficial corruption is still valid: corruption is beneficial as long as welfare gains exceed welfare losses; otherwise it will be detrimental.⁷

In either case, corruption is a product

of both individual characteristics and social influences, and its welfare impact will incorporate both costs and benefits that may warrant different emphasis in terms of policy preferences. The determinants of corruption, therefore, must be analyzed from the perspectives of individuals and of society. The analytic framework proposed below employs the rational individual choice model that has been widely used in the crime literature, and incorporates three additional structural variables to account for societal influences.

Model for Policy Analysis

Consider the social loss minimization function,

$$W_L = W_L(S, R, f', CP), \quad (3)$$

which has the same properties as the Becker-Ehrlich model used in the crime literature.⁸ S is the net social cost which results from corrupt practices; R is the direct cost of corruption; f' is the social loss per violator convicted; and CP is the number of corrupt practices.

The equations of the model are:

$$S = SC(CP) - SB(CP) \quad (4)$$

$$\begin{aligned} SC' &> 0 & SB' &> 0 \\ SC'' &> 0 & SB'' &< 0 \end{aligned}$$

$$\begin{aligned} R &= R(p, CP), R = (R_1, R_2) \\ R' &> 0 \end{aligned} \quad (5)$$

$$f' = kf, \quad k > 0 \quad (6)$$

$$CP = CP(p, f, Y_i/Y_1, \delta, \pi, \gamma) \quad (7)$$

$$\frac{\partial CP}{\partial p} < 0, \frac{\partial CP}{\partial f} < 0, \frac{\partial CP}{\partial(Y_i/Y_1)} > 0$$

$$\frac{\partial CP}{\partial \delta} > 0, \frac{\partial CP}{\partial \pi} > 0, \frac{\partial CP}{\partial \gamma} < 0.$$

In equation (4) net social cost (S) is stated as the difference between social costs (SC)

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and social benefits (SB). Social costs result from the external diseconomies of corruption; they include economic costs and social damages imposed on third non-consenting parties, such as the negative implications of violating the rules and norms of society. Social benefits include welfare gains which result from cases 1 or 2, described earlier, and account for the personal gains of the parties who are engaged in corrupt practices. Both SC and SB are related to the level of corrupt practices (CP) and tend to increase as CP increases. Since $SC'' > 0$ and $SB'' < 0$, it is assumed that social costs and benefits are subject to increasing and diminishing returns.

In equation (5) the direct costs of corruption (R), are expected to increase as a result of increases in CP and in the probability of apprehending and convicting the violators (p). Additional resources (R_1) will be used in discovering violations and convicting violators, and at the same time resources (R_2) will be used by the violators to conceal their corrupt practices and to minimize punishment if they are convicted. Equation (6) defines the social cost of punishment (f'). The size of punishment (f), such as the length of imprisonment, is translated into its social cost equivalent f' by a coefficient k indicating the form of punishment imposed on the violator.

Equation (7) relates the level of corruption to p , to f , to the proportion of illegitimate income Y_i to the level of legitimate income Y_L , and to three structural variables: δ indicating the size of bureaucracy; π the degree of ambiguity of the property rights legislation; and γ the degree of political participation in the production of public goods.

The deterrence variables, p and f , and the coefficient k are borrowed from Becker's crime model to explain some aspects of corruption that would be dealt with in a normal criminal activity. As specified in the model, varying p , f , and k

is expected to alter the nature of individual optimization. For example, public officials with relatively low pay are likely to react to these variables, and when society raises the values of these variables, they are expected to temper and systematize their practices of corruption.

The structural variables account for society's reaction to corruption under varying arrangements of organizational, legal, and political structures. The organizational variable δ is expected to vary with CP in the same direction. Increased bureaucratic inefficiency inflicts additional time and resource costs on the private sector, and this, in turn, presses potential violators to economize by initiating some form of corruption, such as bribery, to obtain expedited processing. Likewise, π , indicating the degree of vagueness and impotence of property rights legislation, is expected to vary directly with CP . An example would be the difference in interpretation of property rights concerning the ownership of groundwater and air resources. Public officials are more likely to participate in corrupt transactions involving air pollution, which is subject to vague property rights, than in the case of contamination of groundwater, where ownership expectations are vociferously exercised.

The third variable, γ , is the degree of political participation in the production of public goods. It indicates the probability of detecting violations and varies inversely with CP . Low political participation, rational ignorance, or absence of political pressure groups is expected to reduce the probability of detecting corruption. Nepotism and patronage as methods of awarding governmental jobs and contracts are practices that are difficult to maintain under intense public scrutiny. Political participation in elections and the degree of attention that society devotes to political and administrative processes will vary for many reasons. Wars or other major upheavals tend to divert the atten-

tion of society, and the expansion of economic activity that results increases the opportunity to engage in corrupt acts. Variation in the business cycle itself would be expected to affect the political participation variable as well. Many activities that might be condoned or ignored during good times become the focus of investigations when conditions worsen and budgets become tighter.⁹

Note that these variables correspond to the structural variables and initiating forces that determined the level of corruption and the resulting changes in society's welfare as described in Figure 1. The policy implications that follow from the deterrent variables, however, are more restrictive than in the case of crime control.¹⁰ As emphasized earlier, corruption results from a collaborative effort between public officials and private parties. The economic impact of corruption relative to crime as defined in criminal law is in the form of "public bad," and the extent of its adversity is likely to depend on the degree of resistance of subjected society. The deterrent variables are likely to be less effective, since corruption is a much more calculated effort, and if corruption is uncovered, often the liability is joint, a factor which may significantly reduce the *ex ante* effectiveness of the deterrent variables.

Thus, the individual rational choice model may be useful for the analysis of corruption that is clearly detrimental. However, when corruption results in both social costs and social benefits, the addition of structural variables pertaining to the resistance level of society is appropriate. In the corruption function introduced above, both public officials and the private parties involved are expected to react to δ , π , and γ . Just as p and f are deterrent variables in the utility functions of violators, δ , π , and γ will be additional incentives or disincentives, significantly determining the level and type of corruption in a society.

Policy Implications of the Model

The model suggests three distinct policy approaches that a society can employ regarding corruption. Maintenance of the status quo is the first policy option, with no additional resources employed to detect or punish violators. The second approach stresses individual-level deterrent considerations as a means of reducing corruption, with additional resources devoted to increasing the probability of detection and punishment of parties engaged in corrupt transactions. The third alternative concentrates on the alteration of the structural conditions relevant to corruption. Additional resources (which will be defined as R_3) would be utilized to clarify property rights, make legislation more congruent with social demands, reduce bureaucratic inefficiency, and increase political participation. In order to maximize social welfare it is necessary to tailor the public policy response to the type of corrupt activity in question.

Recall that corruption was dichotomized as being either beneficial or detrimental, according to its net impact on social welfare. The three public policy options must now be evaluated to determine which choice will maximize social welfare for a given type of corruption. The first option, maintenance of the status quo, can be employed for either type of corruption if certain conditions are present. To justify choosing this option, additional costs required for any change in policy must exceed the benefits derived from reducing corrupt practices. This means that the costs of the corrupt activity must be less for society than the costs associated with any method of reducing the illegitimate practice. Another condition that could justify the status quo would be the society's apparent contentment with the current level of corrupt practices, so that there was no incentive to seek another position. Society in this case can simply ignore the problem.

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This option represents an equilibrium position in which the marginal costs of corruption are roughly equivalent to the marginal social benefits attainable from corruption reduction.

The second policy alternative involves increasing p and f . This policy option is desirable only for detrimental corruption, and is justified by the same logic as the deterrent model in the standard crime control literature. Since the net social impact of detrimental corruption is negative, additional resource allocation to improve deterrent considerations is appropriate until the point at which $W_L(S, R, f)$ is minimized.¹¹ Notice that increasing the deterrent considerations is inappropriate for beneficial corruption, because the net social impact of the activity is positive to begin with, and any additional R_1 and R_2 would detract from the marginal social welfare.

The third policy option requires some combination of the following measures: improving bureaucratic effectiveness, legal changes clarifying property rights or increasing the congruence between social demands and political outcomes, and efforts aimed at increasing political participation. These structural alterations will involve resource utilization R_3 . To justify this option resource costs associated with corrupt practices (SC, R, f') must exceed R_3 . Symbolically, this means

$$dW_L(S, R, f') > dW_L(R_3). \quad (8)$$

Note that increased structural efficiency will also yield obvious external benefits to the noncorrupt society $W_G(R_3)$. If these benefits are significant, then

$$[dW_L(S, R, f') + dW_G(R_3)] > dW_L(R_3). \quad (9)$$

These three options suggest that social welfare is maximized when corruption control policy is directed at the appropriate type of illegitimate activity. Since beneficial corruption is caused by in-

efficiencies in the structures of society, the appropriate policy response will be to concentrate additional resources on making improvements in social structures. If instead individual deterrent considerations are enlarged to reduce beneficial corruption, then society will experience welfare losses due to increased social costs related to the additional R_1 , R_2 , and f' .

Conclusions

This research has explored the topic of corruption from the combined perspectives of economics and political science to develop an integrated approach for the development of coherent public policy. To that end it was necessary to link the causes (individual or structural) and consequences (positive or negative) of corruption, and examine the situation in the context of social welfare effects.

One of the theoretical improvements developed in this work is that corruption should not be thought of as a single phenomenon, but as a dichotomy characterized by the net impact of the illegitimate activity on social welfare. If the net impact is positive, corruption is categorized as beneficial; if it is negative, corruption is categorized as detrimental.

From this conception we develop a model in which policy choices related to corruption may be systematically evaluated from a welfare optimization perspective. This model has implications for public policy concerning corruption, and suggests that in cases of beneficial corruption the appropriate strategy may warrant additional resources to alter inefficient social structures. This is a significant departure from the traditional approach to administrative corruption, which treats all illegitimate transactions as though they were the same as most other criminal activity, and thus relies upon individual deterrent policies.

The model accounts for variations in the distributional impact of corruption as

well. However, the choices of welfare optimization strategies must ultimately be tempered by a society's notions of distributive justice and fairness. This research has provided a systematic framework for evaluation of public policy choices, and a basis from which the distributive questions can be approached. Further research is required before any definitive statements can be generated concerning distributive equity and corruption control policies. It is our hope that this research will stimulate further analysis that will answer some of the remaining questions.

Notes

1. The Pareto optimum setting is an output performance criterion in the neoclassical interpretation of economic efficiency. In this analysis it is used as a reference. Other constructs of normative output performance can also be used. For more on Pareto optimality and other forms of outcome performance criteria, see De Allesi (1983).

2. In this analysis we assume a society where government involvement in the economy revolves around public goods production. Individuals are rational, and through a market-oriented collective decision-making mechanism they reveal their preferences. Again, these assumptions are made to reveal the essential features of corruption in a market-oriented society. It is important to emphasize that a similar conception of corruption can be also used in centralized or authoritarian societies.

3. Every point on the welfare frontier $U_A U_B$ is Pareto efficient, and represents a possible distribution of real income (utility) between two income segments A and B . The Bergson-Samuelson social welfare function (W) is one of the most commonly used analytic devices, representing society's distributional judgment. In this study, W is used in its general form, $W(\cdot)$, to generate arguments on distributional consequences of corruption. Its point of tangency with the welfare frontier $U_A U_B$ determines the most desirable allocation among all possible Pareto-efficient allocations. For more on this see Tresch (1981, pp. 17-42).

4. Note that the generalized Paretian efficiency criteria extend to all types of individual optimizations and include additional constraints relating to the system of property rights, including transaction and adjustment costs. See De Allesi (1983).

5. From the economist's perspective, this implies that additional costs must be incurred to introduce structural changes. Usually these costs are substan-

tial because of the inefficiencies involved in collective decision making. Implementation of a cost-effective measure may be delayed due to political considerations.

6. When welfare maximization is divided into separate efficiency and distributional conditions, it can clearly be shown that F is Pareto optimal, and even though it is justifiable by society's welfare function, it is not a welfare maximum P' . In this context, corruption which moves society from P to F is beneficial. Compensation schemes such as non-distortionary taxes or subsidies can be introduced to move society to point P' . The transition toward welfare maximum can also be eased by clarifying and redefining property rights and by improving the political structure.

7. The distributive consequences of egalitarian, Rawlsian, and utilitarian formulations can be clarified on a generalized welfare frontier which consists of both upward- and downward-sloping sections. On a generalized $U_A U_B$ diagram, then, an egalitarian would select a point on a 45° line. A Rawlsian approach will favor distributional outcomes when a minimax point is reached on the welfare frontier, and the utilitarian will seek the tangency point on the downward-sloping portion of the welfare frontier. In this study, we use a Pareto welfare frontier which has a downward slope, as shown in Figure 1, with no intention of reasoning in favor of any of the three outcomes. Following the convention, we refer to the Pareto welfare frontier because of its theoretical convenience in dichotomizing efficiency and distributional problems. For more on this see Graaff (1975, pp. 59-70).

8. Without specifically referring to corruption, Becker (1968) and Ehrlich (1973, 1975) stress two cost variables: one associated with the probability of apprehension and conviction p , and the other with the extent of punishment f . By establishing a causal link between the crime rate and each of the two variables, Becker and Ehrlich argue in favor of the deterrent method of preventing crime.

9. Corruption is assumed to exist in both liberal and conservative administrations. What might be expected to vary is the location of the corrupt transaction. Liberal administrations tend to increase expenditures in the area of social programs, which would provide increased accessibility to corruption for traditional liberal constituencies such as the poor and labor. Conversely, conservative administrations tend to increase appropriations for business and military subsidies, and corruption in these areas would be expected to favor traditional conservative constituencies.

10. There are several empirical investigations in the literature that suggest the possibility of reduced participation in specific cases of crime as either the probability of apprehension and conviction p or the degree of punishment f increased. For a review of these studies see Tullock (1980), and Witte (1980).

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For studies which challenge the deterrent method see Bowers and Pierce (1975), Cook (1980), Forst (1976), Passell and Taylor (1977).

11. See Becker (1968).

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HEGEL'S CRITIQUE OF LIBERALISM

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A recent and perhaps surprising development in political theory has been the revival of neo-Kantian liberalism, with its doctrines of the neutral state and equal concern and respect. Critics of liberalism have suggested, however, that Kantian notions of rights and rationality are too thin a foundation on which to build satisfactory forms of community and political life. In this paper I examine the critique of rights-based liberalism by returning to the philosophy of Hegel. Hegel's position, I suggest, provides us with a much needed middle ground between liberalism and its contemporary critics. Like the modern communitarians he is critical of the individualistic and ahistorical conceptions of rights underlying the liberal polity, but like many liberals he is skeptical of the claims to recreate a democratic, participatory *Gemeinschaft* that would leave citizens defenseless before their particular communities. I conclude that like Montesquieu before him and Tocqueville after, Hegel looked to the "corporations" or intermediary associations to skirt the extremes of the market place and civic virtue.

The last decade or so has witnessed the revival of a kind of moral and political theorizing that owes an explicit debt to Kant.¹ The core strategy sparking this neo-Kantian revival has been an attempt to restore the political philosophy of liberalism on a rationalized and purified foundation. Clearly, the most powerful example of this purified liberalism has been John Rawls's *A Theory of Justice* (1971). Basing his arguments on Kant's distinction between justice and utility and on his conception of the "hypothetical contract," Rawls and others have attempted to "construct" a framework for choosing between moral principles that assumes nothing more than the existence of rational, self-legislating agents. By asking us to abstract from all contingently acquired and hence empir-

ical characteristics about ourselves, the aim of these theorists has been to derive a set of legal and political institutions that accord equal "concern and respect" to everyone's interests. Indeed, so successful has this strategy appeared that one of its proponents has called it "a new liberal paradigm" (Fishkin, 1984, p. 755).

This "new liberal paradigm" has characteristically adopted procedures and assumptions from a variety of very non-Kantian sources, especially neoclassical economic theory. There are at least four presuppositions concerning the nature of rationality and action that underlie these sources.² First, rationality is, on this account, exclusively a predicate of individual actors and actions; this is the principle of methodological individualism. Second, rationality is concerned with

means and not ends. It is a form of calculation that allows the agent to acquire the objects of his desire, rather than prescribing what kind of objects he ought to desire; this is the principle of value-neutrality. Third, the natural goal of each agent is to maximize the number of pleasures and minimize the number of pains; this is the principle of psychological hedonism. And fourth, the problem of politics is conceived as finding ways of limiting the infinity of human desires so that each can coexist with all under the rule of universal law. This is the principle of the social contract or the "natural condition," which is in turn said to generate the conditions for political legitimacy. Out of these purely hypothetical assumptions about human nature, contemporary political theorists have proceeded to elaborate rules of justice which claim to be binding on all rational beings everywhere and always.

And yet the revival of neo-Kantian liberalism has not gone unchallenged. The intellectual pendulum has now begun to swing the other way. The arguments expounded by thinkers as different as Michael Walzer (1983), Alasdair MacIntyre (1981), and Michael Sandel (1982) claim that the solitary, individualistic, and "unencumbered" self of Kantian liberalism is too thin a basis from which to generate political culture or community. In the first place, they deny the tenet of methodological individualism by arguing that rationality may be predicated not merely of individual actions, but of the institutions and even the political culture that make these actions possible. Second, rationality is valued not simply for its ability to achieve ends, but is an end in itself, something valued for its own sake. Third, the comprehensive good for human beings cannot be regarded as the satisfaction of private pleasures, but is intimately bound up with the achievement of the good of others. We are what we are by virtue of our membership in a

community of shared meanings and values. And finally, the political community is not on this account merely an aggregate of atomistic individuals peering through a "veil of ignorance," but is at least partially constitutive of what it is to be a human being. Political life is not just a burden to be borne or a cost to be calculated, but satisfies the human need to belong to a community of rational beings within which one's own moral and intellectual powers may develop. As one of the proponents of the new communitarianism has put it: "To say that the members of a society are bound by a sense of community is not simply to say that a great many of them profess communitarian sentiments and pursue communitarian aims, but rather that they conceive their identity . . . as defined to some extent by the community of which they are a part" (Sandel, 1982, p. 147).

It seems obvious to me that neither of these views is wholly correct. The first attempts to discover rules of social life that would be binding on all rational agents, but its own prescriptions are often so general and abstract as to be void of moral content. Only by abstracting from everything we already know about ourselves, our lives, and our histories will we be in a position to provide a solid and unimpeachable ground for choosing moral principles. I would argue, however, that the result of this operation of abstraction is not to provide a stronger foundation from which the self can choose, but no foundation at all, since there is literally nothing left to the self.

If the neo-Kantian paradigm suffers, then, from excessive abstractness, the new community or participatory model suffers from the opposite problem. In its assumption that everything we are we owe to the particular communities of which we are a part, the communitarians appear dangerously indifferent to the quite legitimate claims that individuals can make against their communities. All we are told is that

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particular communities are the outcome of our communal or political natures, which leaves the individual no room to criticize those communities except in their own terms. The result, often the opposite of what is intended, is merely to re-endorse those patterns of social and political life without asking whether they are rational or whether they ought to exist. These "re-endorsement theories," as Ernest Gellner has called them, tend uncritically to accept whatever forms of life or culture happen to exist. "Forms of life and cultures," he says, "are precisely what thought does not and need not automatically accept. Cultures must not be judges in their own case" (Gellner, 1974, p. 20).

It is not my purpose in this paper to attempt to pass judgment on this debate. Instead I would like to examine the genesis of the critique of rights-based liberalism in the philosophy of Hegel.³ Indeed, many of the criticisms that are made of liberalism today—its abstract, individualistic methodology, its excessive concern with private nonpolitical goals, its insensitivity to public issues of citizenship and civic virtue—were all decisively brought to bear near the beginning of the previous century by Hegel. In a sense the whole debate between contemporary liberals and their critics has something of the character of a reinvention of the wheel. It is my contention here that present-day critics of the culture of rights-based liberalism would do well to return to Hegel as the theoretical source of at least some of their beliefs (Plant, 1973; Taylor, 1979).

To cash this thesis I will have to show first what Hegel thought was defective with the tradition of rights-based liberalism as this was handed down from Hobbes and Locke to Rousseau and Kant. After outlining Hegel's methodological objections to this tradition, I will turn to what he takes to be the faulty political practices that it engenders. Only then will I examine Hegel's positive conception of a

community or political culture based upon the notion of *Sittlichkeit*, or the concrete ethical life of a people.

Hegel's position, I want to argue, provides us with a *via media* between the two alternatives outlined above. Like modern communitarians, he is critical of the individualistic and ahistorical concepts of rights underlying the liberal polity, but like many liberals in both his day and ours, he rejects any attempt to return to some form of democratic participatory *Gemeinschaft* based upon immediate face-to-face relations. Instead of rejecting liberalism out of hand, Hegel accepts many of the institutions of the modern world, especially its distinction between civil and political life, as uniquely capable of ensuring the citizen's rational devotion and informed consent. It is this distinction, I want to suggest, that provides Hegel's most lasting and defensible contribution to political philosophy.

The Critique of Natural Rights

Hegel's critique of modern natural rights theories is developed most powerfully in a book-length essay published almost twenty years prior to the *Philosophy of Right*, and called *On the Scientific Ways of Treating Natural Law*. Here he takes to task what he takes to be the two most important methodological approaches to the study of rights. The first he identifies with the "empirical" approach, by which he means the naturalistic theories of Hobbes and Locke (Hegel, *NL*, 1975, pp. 59–70). These thinkers are called empiricists because they sought to derive a theory of rights from certain purported facts or needs that human beings have. These needs, which were thought to be prior to the emergence of civil society, and therefore the real or genuine needs of man, could then be used as the primary building blocks out of which human rights could be constructed. Thus the Hobbesian-Lockean account of rights

derives from the root human need for self-preservation, or in Montesquieu's more expansive formulation, from "that tranquillity of mind which arises from an individual's opinion of his security" (Montesquieu, 1949, bk, XI, sec. 6, p. 151).

Still, the empirical approach to rights has its origins in the work of Thomas Hobbes, whom Hegel regarded as the "revolutionary" founder of the whole modern tradition (*LHP*, 1955, vol. 3, pp. 92-3). The novel and shocking character of Hobbes's insights consisted in the fact that he was the first to derive a teaching about political obligation from a doctrine of the will alone. The state for Hobbes is no longer conceived as it was by Plato and Aristotle, as a duplication of principles already "out there" in nature that can be discovered by dint of human reason. Rather, for Hobbes, the state is the result of an active choice by which we impose our wills upon nature. This emphasis on the active role of choice or will is what distinguishes ancient from modern conceptions of right (Hegel, *LHP*, 1955, vol. 3, pp. 114-15). Indeed, nature is no longer able to provide a ground for political obligation precisely because the idea of nature has undergone a radical change. Nature is no longer regarded as beneficently organized for human ends and purposes. Rather, it is seen as something indifferent or even hostile to human life, devoid of what Nietzsche would call "metaphysical comfort," and therefore something to be "conquered." It is necessary, then, to "construct" social reality out of the raw materials provided by nature, "to the end [of securing] as far as matter, and human force permit, such ends as human life requireth" (Hobbes, 1977, p. 478).

The upshot of this new activist or constructivist conception of the will is three-fold. First, it blurs the ancient distinction between doing and making, *praxis* and *poiesis*. Politics is no longer regarded as

an activity the point of which is the activity itself performed well. It becomes more like a technique in the modern sense of social engineering, which aims at the skillful production of a legal order whose aims are the protection of health, safety, and comfort (Habermas, 1974, p. 42). Second, Hobbes regards politics as capable of achieving scientific or epistemic status precisely because, and to the extent that, its subject matter has been made by us. The idea here is that nature provides no intrinsic standard of its intelligibility. Accordingly, what knowledge of either the social or natural worlds we possess is the product of a free construction whereby man, the knowing subject, imposes an order and design of his own making. Because, for Hobbes, there is no presupposition of a preestablished harmony between our knowledge and the world, the only test of the adequacy of our ideas is "operational"—whether they "work" (cf. Strauss, 1953, pp. 173-74). Third, Hobbes tried to demonstrate that only a society that has been artificially created along the lines of the modern science of nature is capable of imposing obligations upon its subjects. If, then, society is purely an artificial construct, the question for politics becomes not the ancient one—"what is the best life for man?" or "how ought I to live?"—but "why should I obey the law?" Questions of obligation, not virtue, have become primary in the new political science (Salkever, 1974, pp. 78-92).

Even while Hegel credits Hobbes with these epoch-making insights, he still claims to have detected a crucial ambiguity in the empirical approach to rights. Empiricism claims to discover the most universal features of human beings as such by means of a kind of thought-experiment, hypothetically stripping or peeling away everything we have acquired through the influence of history, custom, and tradition. But if empiricism is to remain faithful to its own epistemo-

logical premises, it cannot, strictly speaking, distinguish the natural from the conventional. In Hegel's terminology, empiricism is incapable of separating the "necessary" from the "contingent" without introducing extraneous assumptions: "Empiricism lacks in the first place all criteria for drawing the boundary between the accidental and the necessary; i.e., for determining what in the chaos of the state of nature or in the abstraction of man must remain and what must be discarded" (NL, 1975, p. 64). The problem is that rather than merely collecting data or counting facts, the empiricist is led to abstract some singular aspect of human experience, such as the drive for self-preservation, and to elevate it to the status of a first cause capable of accounting for society. Explanations of this sort, Hegel shows, tend to be vacuous, since they do no more than read back into nature what they have already taken out. Thus despite Hobbes's bold claims to have discovered the true ground of human rights in the most elementary needs of men, in the end he does no more than accept our empirically given needs as binding.

The more promising strand of natural rights theory, then, would seem to be represented by the "formalist" approach of Kant and Fichte, who were well aware of the difficulties with empiricism (Hegel, NL, 1975, pp. 70 ff.). Indeed, their formalism is no more than a deepening and a radicalization of the critique already levelled against Hobbes and Locke by Rousseau (1964, pp. 213-20). In the *Philosophy of Right* Hegel goes out of his way to commend Rousseau for "adducing the will as the principle of the state" and not "a principle, like gregarious instinct, for instance, or divine authority" (PR, 1972, par. 258R, pp. 156-57).

This remark is clearly an allusion to Rousseau's emphasis on the will in the *Social Contract* but perhaps even more importantly to the philosophical anthro-

pology of the *Discourse on the Origins of Inequality* in which he argues that freedom from natural causality is the distinctive mark of the human. Hobbes and Locke, Rousseau argued there, were correct to refer the standard of right back to nature alone. What they failed to clarify, however, is that nature provides at best only a kind of negative orientation from which we must break if we are to understand the true ground of rights. The source of right must be sought in that quality that escapes or transcends nature. This quality was described by him as the power of the will or free agency, one of the "essential gifts of nature" precisely because it lacks empirical direction or orientation (Rousseau, 1964, pp. 113-14, 168). This is the point that Hobbes had only imperfectly understood. While he correctly saw that the will is the basis for all obligation, he failed to distinguish radically enough the will from nature. Consequently, when he came to describe formally the function of the will, he simply turned it into "the last appetite in deliberation" (Hobbes, 1977, p. 54). But if the will is an "appetite," even if only the "last" one, it still remains bound by the laws of natural causality and fails to achieve the freedom that is its essence.

Even as Rousseau attempted to divorce the will from all naturalistic or teleological directives, it was Kant alone who at last succeeded in articulating a pure "formalism" of the will. By "formal" I mean here that Kant was more interested in the form or structure in which moral imperatives are framed than in their content or direction. This formalism led to an almost exclusive emphasis on the "inner" side of action, the purity of the motive or the intent, as opposed to the "outer" side, the accomplished result. Goodness, then, applies only to good intentions or the good will, which is the one thing that Kant calls "unconditionally" good (Kant, 1964, p. 9). Of course, the paradoxical conclusion of Kant's alleged moral for-

malism is that while he attributes absolute or unconditional goodness to the will alone, he is led to deny it to the external world of action and society, both of which are conceived as governed by causal, natural laws, and as such indifferent to moral praise and blame.

Kant's rigorous separation of the will from its actions is not as bizarre as it might at first appear. If we regard human action from the empirical or "phenomenal" side, we see that it is determined by a variety of heteronomous ends and purposes. Seen in this light, our motives and purposes are as much a part of the determined order of nature as are any other physical occurrences. And yet, as Kant tries to show in the famous "Third Antinomy" of the *Critique of Pure Reason* (1965, pp. 409-17) the all-embracing laws of physical causality cannot drive out completely the dimension of human freedom. Kant believes that if we abstract from all the diverse ends and purposes for which we strive, we will find underlying them all the capacity for willing and choosing, the capacity to be self-directing or self-determining. Kant refers to this capacity as the power of "pure practical reason," which alone is sufficient to distinguish us from the laws of physical causation. We alone possess a form of "moral causality," or the capacity for "spontaneity," which escapes nature. Thus our unique status as moral beings derives from the fact that we have the capacity not merely to live in various ways, but to make the laws by which we choose to live. It is this power of initiation, rather than any determinate end we might pursue, that testifies to the inherent "dignity and sublimity" of man and forms the only ground of right (Kant, 1964, p. 107).

The only legitimate limitation Kant can imagine on the will's capacity for self-direction is a purely negative or formal one. While the will, considered as a mere capacity for self-legislation, provides no

object or action that we can affirm or deny, it does at least provide a test for determining whether the action to which it points is a moral one. This is the principle of universalizability. According to this principle, the maxim informing our actions must be framed in such a way that it would obligate everyone who found himself similarly circumstanced. Thus the first and most famous formulation of the categorical imperative reads: "Act only on that maxim through which you can at the same time will that it should become a universal law" (Kant, 1964, p. 88). Or again: "Act as if the maxim of your action were to become through your will a universal law of nature" (Kant, 1964, p. 89). Moral action, then, must be characterized by the same abstract generality or universality as the laws governing the physical world. A law that cannot be consistent, that is, universally willed to apply to everyone who finds himself similarly circumstanced, cannot have moral or categorical validity. At best such maxims would be like Hobbes's councils of prudence—to be followed if they serve the agent's interests—rather than moral laws which obligate because of their very form (Kant, 1964, p. 86).

We need not continue with this account of Kant's moral formalism. More to the point are the difficulties to which this conception of morality gives rise. Specifically, by concerning himself almost exclusively with the form of moral judgment, that is, with how something is willed, Kant leaves himself open to the charge of indifference to what is willed. To what objects or set of objects is the moral law supposed to apply? According to Hegel, Kant can give no answer to this question, because the moment he attempts to specify some content for the law, he immediately violates its universality:

For practical reason is the complete abstraction from all content of the will; to introduce a content is to establish a heteronomy of choice. But

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what is precisely of interest is to know what right and duty are. We ask for the content of the moral law, and this content alone concerns us. But the essence of pure will and pure practical reason is to be abstracted from all content. Thus it is a self-contradiction to seek in this absolute practical reason a moral legislation which we would have to have a content, since the essence of this reason is to have none (NL, 1975, p. 76).

Hegel's charge here—that Kantian morality is an “empty formalism” that can generate no “immanent doctrine of duties,” and that it is therefore reduced to “preaching” an ethic of “duty for duty's sake” (PR, 1972, par. 135R, p. 90)—has been more or less accepted by generations of interpreters. To illustrate his point, Hegel refers explicitly to an example that Kant himself used on a number of occasions to illustrate the categorical imperative (NL, 1975, p. 77; cf. Kant, 1956, pp. 26–27; 1977, pp. 70–71). The example is that of an agent who has been entrusted with a sum of money, the owner of which has died and has left no evidence of its whereabouts. In one of his formulations of this example Kant even asks us to imagine that through no fault of his own the trustee has suffered complete financial disaster, while the heirs of the deceased are “rich, uncharitable, thoroughly extravagant, and luxurious, so that it would make little difference if the aforesaid addition to their property were thrown into the sea.” Even under these conditions, Kant claims, the trustee would be wrong to keep the deposit, for reasons that would be plain to “a child of around eight or nine years old.” To universalize the maxim of that action, that it is right to embezzle deposits whenever it is in my interests to do so or whenever I know that I will not be caught, could not be universalized without contradiction; for to universalize this maxim would be to destroy the conditions of trust and confidence that make not only deposits but any safety and security among men possible at all. Such actions are not only self-contradictory but

self-defeating, since they threaten any viable test of rights.

Hegel's answer to this application of the moral law is well known, but still deserves to be cited in full:

But where is the contradiction if there were no deposits? The non-existence of deposits would contradict other specific things, just as the possibility of deposits fits together with other necessary specific things and thereby will itself be necessary. But other ends and material grounds are not to be invoked; it is the immediate form of the concept which is to settle the rightness of adopting either one specific matter or the other. For the form, however, one of the opposed specifics is just as valid as the other; each can be conceived as a quality, and this conception can be expressed as a law (NL, 1975, p. 77).

The point of this passage is that Kant's universalization principle cannot establish what he would like it to. Universalization can ensure only consistency or rationality of action irrespective of social context or utility. Thus, to say that the embezzlement of deposits is wrong assumes that if such a practice were generalized it would result in a world where the continued existence of deposits, and hence property, would be thrown into question. Hegel's point, however, is that a world without property could not, even on Kant's account, be regarded as morally inferior to the existing world. It would only be a different world from the one in which we now live. There is only a “contradiction” if we expected others to respect our property and return deposits, but had no intention of doing so to theirs. Thus Kant's universalization principle may be an effective means of unmasking the moral hypocrite, but would be useless against the consistent evil doer. It might be useful in dealing with the Willy Suttons of the world, but would be silenced in the face of the Marquis de Sade.

Hegel's point seems to me to be twofold. First, because it operates at such a high altitude, the categorical imperative is incapable of demonstrating whether certain social institutions are valid or

invalid. As the example above illustrates, Kant can at best show what kinds of actions are ruled out if certain kinds of institutions are presupposed. In Hegel's view this testifies to a deeply rooted conservatism in Kant's thought, since by a kind of "sleight of hand" (Lukács, 1975, p. 160) he leaves the social and political world simply as it is.

Second, Kant's preference for sheer consistency, as opposed to moderation or compromise, seems to mask a moral zealotry where the most extreme policies are advocated so long as they are sincerely intended. Hence, in the appendix to *Perpetual Peace* (in *Political Writings*, 1977), Kant derides the "political moralists" who debase morality by making it serve partisan interests. Instead he wants politics to serve morality. The "moral politician" is one who upholds rights as the "limiting condition" of what is politically permissible. The result is to moralize political relations by insisting that "a true system of politics cannot therefore take a single step without first paying tribute to morality" (Kant, 1977, pp. 117-18, 125). What Hegel fears is that this elevation of morality as the judge of politics has liberated a kind of extremism that rules out policies based upon the prudential reconciliation of interests. As with Max Weber later on, the implication of what Kant says is that politics is best when commitments are at their highest. But, and this is crucial, there is no means of distinguishing between commitments except on the purely formal grounds of logical consistency (cf. Weber, 1949, pp. 10, 57).

In the final analysis, then, Hegel believes that the problem with Kant's formalist approach to rights derives from his attempt to identify some area of human freedom apart from the determined order of nature. Since freedom cannot be found within nature, it had to be located over and against it in the will, the sheer capacity for spontaneous action. The dif-

ficulty here is that by confining freedom to the activity of willing alone, we deny that it can be found in the external world, since there we see no unconditioned wills but only actions which form a series of links in an infinite chain of causes. Kant's constriction of freedom to the noumenal realm, the sphere of pure intentions, has the effect of rendering it altogether nugatory for practical purposes. Nietzsche was perhaps only the most ruthless in exposing the illusions of this purely Kantian or German "inwardness." The danger, he wrote, is that "the internal substance [the good will] cannot be seen from the outside, and so may one day take the opportunity of vanishing, and no one will notice its absence any more than its presence before" (Nietzsche, 1957, p. 26).

Rousseau and the French Revolution

Hegel's critique of modern natural rights philosophy is not merely methodological, but substantive. He regards it as revolutionary in its essence and responsible for the excesses of the French Revolution and the terror in particular. And yet while Hegel, like Kant before him, was chastened by the actual experience of revolution, he continued to celebrate, almost to the end of his life, the principle which he believed the revolution had vindicated.⁴ This principle, stated forcefully in his lectures on the *Philosophy of History*, is the right of reason or philosophy to restructure the world. In accord with the general outlook of German idealism, Hegel interprets the revolution as carrying out in practice what Enlightenment philosophers had already vindicated in theory:

Anaxagoras had been the first to say that *nous* governs the World; but not until now had man advanced to the recognition of the principle that Thought ought to govern spiritual reality. This was accordingly a glorious mental down. All thinking beings shared in the jubilation of this

epoch. Emotions of a lofty character stirred men's minds at that time; a spiritual enthusiasm thrilled through the world, as if the reconciliation between the Divine and the Secular was now first accomplished (PH, 1956, p. 447).

The French Revolution, then, is the final proof and legitimation of the principles of the natural rights philosophers advanced for over a century before. The core of their teaching was the belief that men should be free to make and remake their institutions and their world in keeping with their own critical rationality. Reason alone could liberate men from the inherited weight of custom, tradition, and history, and allow them for the first time to master the world by submitting it to their own conscious designs and purposes. Indeed, Kant only expressed the deepest longings of Enlightenment when he described its philosophy as "man's emergence from his self-incurred immaturity. . . . The motto of enlightenment is therefore: *Sapere aude!* Have courage to use your own understanding!" (1977, p. 54).

But even as Kant regarded his own "critical idealism" as carrying out a "Copernican revolution" in both knowledge and morals, he was reluctant to extend this into a doctrine of political revolt. In the *Contest of the Faculties* (in *Political Writings*, 1977), for instance, Kant looked hopefully to the revolution as proof that political action could be guided by moral reason rather than self-interest alone. Even while the revolution, he says, may be filled with "misery and atrocity," such that "no right-thinking man would ever decide to make the same experiment again," it still cannot help but inspire in all "disinterested spectators" a belief in a "moral tendency within the human race" (Kant, 1977, p. 182; translation modified). Yet even if the revolution revealed the openness of politics to moral factors, he regarded the regicide as an ethical sin even worse than murder. In the *Rechtslehre* he remarks that the act of

regicide "must arouse dread in any soul imbued with ideas of human right." This feeling of dread is not "aesthetic," which derives from pity for the executed man, but moral in the sense of a "complete reversal of all concepts of right" (Kant, 1977, p. 145). Thus, to the extent that political reform was possible, Kant preferred to begin from the top down. Only existing sovereigns open to moral appeal are valid agents of reform.

It is not surprising that Hegel and the generation that came of age during the French Revolution found Kant's half-hearted application of his philosophy to politics inadequate. For Hegel and his contemporaries like Schelling and Hölderlin, the revolution promised the recreation of a cohesive republican community founded upon Kantian morality. Kantian moral autonomy was regarded as the appropriate support for the cult of ancient virtue and active citizenship so prominent during the 1790s. As early as 1795 Hegel had declared himself a devotee of Kantianism because it vindicated the "rights of man" (BR, 1952, vol. 1, pp. 23-24). With the popularization of the idea of right, Hegel wrote to Schelling, it would only be a matter of time until the public began to demand treatment with dignity and respect. Awakened from their slumber, men would begin to think not in terms of "what is" but in terms of "what ought to be." On the basis of his early Kantianism, Hegel went so far as to predict: "From the Kantian system and its completion, then, I expect a revolution in Germany." Thus Hegel hoped to translate into political terms the "kingdom of ends" of the *Foundations of the Metaphysics of Morals* and the "ethical commonwealth" of *Religion Within the Limits of Reason Alone*.

Even while Hegel continued to regard the French Revolution (along with the Protestant Reformation) as one of the great watershed moments of modern history, he later came to see it more as a

great moral and political tragedy. The tragic and even nihilistic character of the revolution stemmed ultimately from the philosophical subjectivism that is its source. This is examined most comprehensively in a chapter from the *Phenomenology of Mind* entitled "Absolute Freedom and Terror," where Hegel shows how the revolution originated from an effort to overcome the division or diremption (*Entfremdung*) of culture, under the old regime (PHM, 1966, pp. 599-610). The old regime, Hegel argues, is a culture in alienation or estrangement. By estrangement Hegel means a discrepancy between seeming and being, between what a culture says and thinks about itself and what it in actuality is. Under the old regime this discrepancy between seeming and being became especially acute as the nobility's love of honor and prestige, which had earlier defined the ethical ideals of feudalism, became transformed into the spirit of the courtier, the flatterer, and the valet. In Hegel's memorable phrase the "heroism of service" was replaced by the "heroism of flattery" (PHM, 1966, p. 527).

Hegel's account of the old regime is clearly indebted to Montesquieu, who saw the sentiments of honor and service as the essential "principle" of feudalism.⁵ But he goes beyond Montesquieu in showing how this "noble consciousness" is not a kind of frozen ideal type, but is dialectically transformed by the power of wealth and money into relinquishing its position and accepting a life of comfort and enjoyment. The proud aristocrat, this once "haughty vassal" who sought glory and nobility for himself in battle, has become a property-owning bourgeois seeking the accumulation of goods and property to insulate him from the consciousness of death. This contradiction between the bourgeois consciousness defined by its fear of death and the older aristocratic or warrior consciousness defined by its contempt for wealth and

comfort marks the beginning of the end of the old regime.

The final stage of the denouement is revealed most powerfully in Hegel's analysis of Diderot's *Le neveu de Rameau*, the text which lays bare and reveals this contradiction in all its duplicity (PHM, 1966, pp. 543-48). This work is virtually the prototype of the prerevolutionary mind torn between the conflicting values of the aristocracy and the bourgeoisie. What emerges from this "disintegrated" consciousness is, of course, the demand for "absolute freedom" from all previous restrictions and cultural restraints. What Hegel describes here is a kind of freedom without context, a nihilistic yearning for oblivion or a "freedom of the void" that is often associated with the modern desire for total emancipation (cf. PR, 1972, par. 5R, p. 22). In an earlier section of the *Phenomenology of Mind*, Hegel describes how this modern yearning for radical freedom has emerged from the process of cultural disintegration that preceded it:

It is here as in the case of the birth of a child; after a long period of nutrition in silence, the continuity of the gradual growth in size, of quantitative change, is suddenly cut short by the first breath drawn—there is a break in the process, a qualitative change—and the child is born. In like manner the spirit of the time, growing slowly and quietly ripe for the new form it is to assume, disintegrates one fragment after another of the structure of its previous world. That it is tottering to its fall is indicated only by symptoms here and there. Frivolity and again ennui, which are spreading in the established order of things, the undefined foreboding of something unknown—all these betoken that there is something else approaching. This gradual crumbling to pieces, which did not alter the general look and aspect of the whole, is interrupted by the sunrise, which, in a flash and at a single stroke, brings to view the form and structure of the new world (PHM, 1966, p. 75; see also ETW, 1971, p. 152).

The French Revolution looked to Hegel like the attempt to recreate the conditions for social and political harmony that not only the old regime, but all post-classical culture had torn asunder. The revolutionaries, acting out of a desire to bring the

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doctrines of the philosophers down to earth, directed themselves to removing all traces of transcendence and otherworldliness (Strauss, 1953, p. 15). To bring this reconciliation about the radicals sought to recreate the kind of consensus and public spiritedness evinced by the ancient *polis*. The *polis* experience was based on a devotion to "public goods" at the expense of private interests, and elevated the virtue of the citizen over and above those of the private man or bourgeois. But since these conditions are no longer a part of the modern world, they had to be recreated artificially, that is, on the basis of violence or terror, through a radical, wholesale negation of what is.

Hegel's verdict on this attempt to realize the general will here and now is clear and decisive: It resulted in a "rage and fury of destruction" that had "no inner significance or filling" any more than "cutting off a head of cabbage or swallowing a draught of water" (PHM, 1966, pp. 604-05). The claim that we should will to be in all things, or that the general will should be the only legitimate ground of rights, would not only abolish all existing institutions and hierarchies, but would regard "all differences in talent and authority as being superseded" (PHM, 1966, p. 601). Nothing would be allowed to exist that is not a product of the general will. Thus even God, the "empty *être suprême*," is said to hover there "merely as an exhalation of stale gas" (PHM, 1966, p. 602). The culmination of the revolution, then, is "the sheer horror of the negative" in which all the "determinate elements disappear with the disaster and ruin that overtake the self in the state of absolute freedom" (PHM, 1966, p. 608).

The argument being made here is that while the general will can abolish, it cannot create. It can destroy the old regime, but cannot build a new one. The idea of the general will is that I am only free when I obey the laws that I have myself created.

But since the general will is the outcome of a collective decision, it cannot be decided by me alone. If everyone is to be free, then everyone must at least participate in the decision making. There is no sense here as, say, in Hobbes, of authorizing someone else to do the work for me (Hobbes, 1977, pp. 126-28). Freedom is attained for everyone when there is a complete unanimity of wills. But, as Hegel points out, the search for unanimity is a chimera less likely to result in community than in complete anarchy. Since each will necessarily regards itself as absolute, as the supreme expression of the *volonté générale*, there is no standard short of universal agreement that can guarantee the legitimacy of the outcome. Any halfway measure, such as representative institutions or rule by the majority, would be a violation of my inalienable right to self-legislation, to obey laws of my own making. The result is to create a permanent and implacable opposition between the people and their government, which will always appear to them as a particular corporate will, a "faction" interposing itself between them and their true interests (PHM, 1966, pp. 605-06).

Hegel demonstrates how this inability of the will to generate a stable political community is related to its inner principle of subjectivity. Since the will has been shorn of all its natural or empirical ends, the only guarantee of its authenticity lies in its inner purity or self-certainty. Sincerity thus becomes the essence of virtue. But herein lies the problem, for if purity of intent is the sole criterion of the worth of action, then men must be judged not according to the outcome of their deeds, but by their subjective convictions or the "law of the heart" (PHM, 1966, pp. 391-400). By elevating the purity of one's convictions as the standard of political right, the politics of the revolution became a relentless search to unmask those hypocrites who pursue their own private ends under the guise of public virtue. Just as

sincerity was the greatest virtue, so did hypocrisy become the greatest vice. Indeed, the reign of terror was nothing so much as the working out on the public stage of this obsessive concern with the purity of motives.

One might, of course, wonder why hypocrisy, what Judith Shklar (1984, pp. 45-86) has called a relatively "ordinary vice," would be responsible for such a wave of violence and fanaticism. Hypocrisy is, after all, the only vice that pays homage to virtue. Hannah Arendt (1965, pp. 91-104) has argued at length that this desire to root out hypocrisy stems from the revolution's own "favored simile" of itself as tearing the mask, the persona, off a corrupt French society to expose behind it the uncorrupted "natural man." For a theorist like Arendt, for whom politics is in the literal sense a kind of "play acting" where actors become the roles or legal personae that they assume, this search for the natural or authentic man behind the mask can only spell disaster. The natural man was, of course, an invention of Rousseau's, who in the *Discourse on Inequality* had described him as unspoiled, honest, and self-complete; only through society or history does he become corrupt, calculative, and dependent (Rousseau, 1964, p. 179). The object of Rousseau's scorn was, however, a particular species of civil or historical man that he was the first to identify as the modern bourgeois. The bourgeois was for Rousseau not an owner of the means of production, as this term was later used by Marx, but in the first place the possessor of a kind of self-love (*amour propre*) that leads him constantly to live both in and through the opinions of others. Unlike *l'homme naturel* who is fundamentally self-sufficient and drawn to others only through the instinct of pity or compassion, civil or bourgeois man needs others as the means to the satisfaction of his almost limitless desires. What repels Rousseau is that unlike both the natural

man and the classical *citoyen*, who are both directly what they seem to be, the bourgeois is "alienated" in the sense of the term described above: he is not what he seems to be; he is a dissembler, a role player, and worst of all, a hypocrite.

From their attempt to resurrect Rousseau's natural man as the norm and measure by which to judge civil man, the revolutionaries sought to recast society wholesale. Just as Rousseau had seen pity as the source of all morality, so did Robespierre regard virtue as the ability to identify oneself immediately with the immense poverty and suffering of the majority of the French *peuple*. Virtue thus became synonymous with the capacity to sympathize with the plight of others, the *malheureux*, or the wretched of the earth. The new "republic of virtue" would then turn its "compassionate zeal" on all those suspected of harboring an impure heart or a lack of identification with the nation as a whole. A heightened moral sensitivity thus unleashed a wave of unprecedented ruthlessness and terror. But out of this distrust of the motives of others, the revolutionaries came to distrust their own motives. Recognizing with Pascal that the heart has "*raisons que la raison ne connaît point*," the revolution could only end by devouring its own. As Arendt writes:

Robespierre's insane lack of trust in others, even in his closest friends, sprang ultimately from his not so insane but quite normal suspicion of himself. Since his very credo forced him to play the "incorruptible" in public every day and to display his virtue, to open his heart as he understood it, at least once a week, how could he be sure that he was not the one thing he probably feared most in his life, a hypocrite? (Arendt, 1965, p. 92).

The failure of the French Revolution, then, to create a moral republic stemmed from its transformation of an essentially private conception of virtue as sincerity into a test of citizenship. The problem with this immense privatization of the public realm is that it provides no means of harmonizing the freedom of the indi-

vidual will with the freedom of all except through recourse to force and coercion. Since men cannot be assumed to be virtuous, they must be "forced to be free." Rousseau's other solution for establishing the authenticity of the general will by including everyone within the decision-making process is no answer. Leaving aside the practical difficulty of convening even a relatively small city, the participation of each citizen in public deliberation and debate cannot create a community precisely because it already presupposes a community. Rousseau's only answer here was recourse to the semi-mythical figure of the "legislator" who, like Moses, Romulus, and Theseus, is capable of creating a people virtually *ex nihilo* (Rousseau, 1978, bk. II, Ch. 7, pp. 67-70).

Hegel's answer to this problem was to invoke the figure of a modern day Theseus, namely Napoleon, as the founder of the state. In his Jena lectures on the philosophy of mind Hegel remarks that, unlike earlier natural rights theorists, for whom the state has its origins in the rational deliberations of autonomous agents, for him, "all states are founded by the sublime power of great men" (JR, 1969, p. 246). As in his earlier study of *The German Constitution*, Hegel is obviously thinking here of Napoleon as the restorer of the state after its dissolution in the acid of revolutionary turmoil (cf. PW, 1964, pp. 145, 219, 245). "In this way," he says, "Theseus founded the Athenian state; also in this way during the French Revolution a terrible power held the state generally" (JR, 1969, p. 246).

While Napoleon, this "terrible power," is not mentioned by name in either the Jena lectures or the *Phenomenology of Mind*, it is clearly not the man but the principle he embodies that Hegel admires. This is the principle of modern constitutional government based upon the rule of law and the division of society into distinct public and private spheres. But when

the basis for this state has been laid, the work of its architect is made redundant. Such was the case with Robespierre, of whom Hegel says that "power abandoned him, because necessity abandoned him and so he was violently overthrown" (JR, 1969, p. 248). Thus like the original Theseus, Napoleon, the modern tyrant, is fated to disappear from the scene which he has helped to create. Hegel's final verdict, then, is that Napoleonic tyranny must disappear not because it is intrinsically evil, but simply because it has ceased to fulfill its "progressive" function.⁶

Hegel's Theory of Community

Hegel's answer to his natural rights predecessors of the seventeenth and eighteenth centuries is summed up under the concept of *Sittlichkeit* or "ethical life." By referring to the community under the category of social ethics, Hegel implies at least two things. First, "the standpoint of ethical life" is introduced in explicit contrast to "the standpoint of morality." Whereas Kantian *Moralität* applies to individuals qua individuals in abstraction from their specific social and political roles, ethics refers to the virtue of which a person is capable as a member of a specific community. Ethics refers not merely to the capacity for rational choice and action, but to "absolutely valid laws and institutions," to "custom," "habitual practice," and the "general mode of conduct" of individuals (Hegel, PR, 1972, par. 144, p. 105, par. 151, p. 108). Thus the sphere of social ethics has a content that is lacking from the individualistic and rationalistic moral theories of Hegel's immediate predecessors. Hegel's point is that despite the claim to speak universally and ahistorically, these moralities presuppose and are dependent upon a social context for their validity, and it is this context that the concept of *Sittlichkeit* intends to bring out.

Second, by speaking of a common way of life as a branch of ethics, Hegel harks back to the ancient world and its idea of the *polis*. Indeed, the German word for ethics, *Sitte*, derives from the Greek *ethos*, meaning the customary horizon or dwelling place within which life is lived. Ethics is, then, coterminous with politics, which is concerned with the just and the good or with the conditions of virtue or *eudaimonia*. The modern theorist who came closest to grasping this standpoint, Hegel says, is Montesquieu, whose "immortal" work introduced the concept of an "*esprit général d'une nation*" which approximates his own doctrine of ethical life (NL, 1975, pp. 128–29; PR, 1972, par. 261R, p. 161). Yet while Montesquieu, Hegel claims, operated with an essentially empirical and descriptive account of *esprit*, Hegel's is rational and necessary. It is not merely a covering concept used to describe the traditional rules, customs, and habits a society actually has. It is used to define what kinds of rules, customs, and habits it must have if a society is to be fully rational. Unlike Montesquieu, then, who never wholly freed himself from the methodological individualism of the Enlightenment, Hegel takes the bold step of predicating rationality not of individuals and their actions, but of the institutions and cultural context that makes action possible.

Hegel's reflections on the idea of political community underwent a number of changes. In his early works he tends to identify the community with an idealized image of the classical *polis*. For the Greeks, the *polis* was not merely a legal association created for the satisfaction of private ends but a way of relating that stressed shared values and common sacrifice even at the expense of individual interests. Hegel explicitly deprecates the emergence of the private realm, especially the longing for individual immortality, as a symptom of political decadence. "Only in moments of inactivity or lethargy could

[the Greeks and Romans] feel the growing strength of a purely self-regarding wish," Hegel writes. "Cato turned to Plato's *Phaedo* only when his world, his republic, hitherto the highest order of things in his eyes, had been destroyed" (ETW, 1971, p. 155). It is clear that throughout these works Hegel, like many of his generation, looked forward to a revival of classicism as an antidote to the division and diremption of modern life. And yet, as I have tried to argue, Hegel's early flirtation with radical republicanism did not survive the failure of the French Revolution to achieve anything remotely resembling classical *polis* democracy.

Hegel's mature conception of a community is based upon an awareness of the complex differentiation of society into distinct classes or estates (*Stände*). His earliest awareness of the importance of social classes as means of linking persons together in morally satisfying ways is drawn from the essay on *Natural Law* as well as the unpublished *System of Ethical Life*. Here Hegel continues to work within a more or less classical paradigm taking for granted the Aristotelian division of society into the private world of the household (*oikos*) and the public realm of the *polis*.⁷ While the former aims at the preservation and sustenance of life, the latter seeks to bring about the conditions of the good life or a life oriented toward the virtues. While the household serves the end of necessity, the *polis*, which grows out of necessity, serves the higher goals of nobility, honor, and excellence (Aristotle, *Politics*, 1280b; 1323a–25b).

Retaining Aristotle's distinction between *oikos* and *polis*, Hegel deduces the two major classes of the modern world. The first is the "absolute class" of "free men," or the military aristocracy (NL, 1975, pp. 99–100; SEL, 1979, pp. 152–53). This class is "absolute" because its activity is aimed not at the skillful production of artifacts, but at "the being and preservation of the entirety of the ethical organiza-

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tion." Following Aristotle he calls this class *πολιτεύειν*, or literally, "living in and with and for one's people" (NL, 1975, p. 100). The preeminent virtue of this class is courage exercised mainly in times of war. The second class Hegel identifies with the commercial bourgeoisie (NL, 1975, pp. 100-104; SEL, 1979, pp. 153-56). Reflecting his study of English political economy, Hegel for the first time speaks here of the "burgher in the sense of a bourgeois" (NL, 1975, p. 103; see also PR, 1972, par. 190R, p. 127).⁸ This class does not risk its life, but typically enjoys the fruits of peace to pursue the accumulation of wealth and comfort. Its characteristic virtue is honesty and fair dealing.

Yet even as he attempts to graft the categories of ancient political philosophy onto the realities of contemporary economic life, Hegel profoundly alters their content. First, political activity, which Aristotle had said was something "natural" to man, Hegel seems to see as heroic or supererogatory. Rather than justice, the paradigmatic political virtue for Hegel is courage, which is portrayed by him as something that does not complete or perfect but must literally "overcome" or "transcend" nature. Accordingly, he tends to see true politics only in terms of war or moments of great national crisis. Thus he records his deep skepticism over any plans for a "perpetual peace," which could only result in a "corruption" of the "ethical health" of peoples (NL, 1975, p. 93; PR, 1972, par. 324R, p. 210; see also Smith, 1983, pp. 624-32).

Second, the commercial classes are no longer tied to the narrow circle of domestic economy, as in the ancient world, but emerge fully within the public sphere, within the domain of political economy. Unlike the ancient *oikos*, the contemporary commercial class has acquired a position of full equality with the political class. In the essay on the *Natural Law* Hegel, still under the influence of Gibbon's *Decline and Fall*, regards

the emergence of the middle class as a product of the demise of the *polis* (NL, 1975, pp. 101-2). It would only be in the *Phenomenology of Mind*, at the end of Hegel's Jena period, that he would see the emergence of this class not as a symptom of political decline, but as an emancipation from the older ethical and religious restraints imposed upon individual conduct by the *polis* (PHM, 1966, pp. 229-40). Labor (*Arbeit*), which had previously been the work of slaves, has become an instrument of *Bildung* or the moral education and maturation of the species. The implication of this change is that future forms of community life will have to be sought not in the Platonic-Aristotelian model of the aristocratic citizen, but in the more mundane economic-poietic activity of the "burgher as a bourgeois."

It would not be for almost twenty years that Hegel, in the *Philosophy of Right*, would theorize the phenomenon of civil society (*bürgerliche Gesellschaft*) as a realm of activity distinct from the traditional categories of both *oikos* and *polis*. Civil society, Hegel makes clear, is a distinctively modern phenomenon (PR, 1972, par. 185R, pp. 123-24). It has no foundation in the ancient world where the "social," as we understand it, did not yet exist. In fact, civil society was still looked upon by Aristotle as identical with the political community (*koinonia politike*) (*Politics*, 1252a). Thus Cicero could define the citizen (*civis*) by the civic quality of "civility," or "knowledge of civic affairs and prudence" (*De Oratore*, 1.14.60). Indeed, this classical identification of civil with political even extends into the modern era where, for instance, Locke calls the seventh chapter of his *Second Treatise of Government* "Of Political or Civil Society" (Riedel, 1969, p. 143).

The novelty of Hegel's account of civil society rests on its distinction from the political state. While the state considers

persons in their public roles as citizens, civil society considers them as "private persons" who aim only at their own interests (PR, 1972, par. 187, pp. 124-25). Civil society, then, takes its point of departure from human beings considered simply as creatures of "need," who see one another as means to the satisfaction of their private desires. Civil society is bound together neither by the close-knit ties of sentiment that bind the family, nor by the more disinterested forms of public altruism that bind the state. Not only, then, does civil society present itself as a "system of needs" with its increasingly complex division of labor, but it has given rise to a new "science," namely political economy (PR, 1972, par. 189R, pp. 126-27). It is to the lasting credit of this science that has "arisen out of the conditions of the modern world" to have discovered the laws governing "mass relationships and mass movements in their complexity and in their quantitative and qualitative character." Hegel even compares the insights of social theorists like Smith and Ricardo to the discoveries of Kepler and Galileo, who found an orderly pattern to the solar system which displays to the naked eye only "irregular movements" (PR, 1972, par. 189A, p. 268).

What defines civil society in the *Philosophy of Right* as a civil society is its division into various classes and estates, each with its own distinctive outlook, interests, and way of life. These estates—the peasantry, the business class, and the "universal" class of state functionaries—provide the crucial links or "mediations" between the "natural society" of the family and the more abstract universality of the modern state. Realizing that in the modern world the state has become too large and impersonal to serve as an object for the citizen's immediate affections and devotions, such opportunity for civic virtue and community mindedness as exist will have to come from these features of civil life. Like Montesquieu before him

and Tocqueville later, Hegel understood the crucial role played in modern society by the "corporations," or intermediary groups. These intermediary bodies are especially important not only because they prevent the individual's submergence in the state or some abstract general will, but also because they provide some context out of which capacities, talents, and abilities may grow (PR, 1972, par. 255R, p. 154).

Hegel's theory of civil society, then, seeks to provide us with forms of civic virtue and shared values that might not, on first glance, appear to be forthcoming. It is a part of the "ethical" (*sittlich*) life of the community and plays, therefore, an important pedagogical or tutelary function in developing the individual's capacities for moral fulfillment and self-realization (PR, 1972, par. 187R, pp. 125-26). It is a kind of school for virtue which, as I understand it, seeks to chart a course between two equally powerful alternatives.

The first is the market understanding of politics adopted by theorists from Hobbes to Anthony Downs, which sees human beings as essentially egoistic calculators of pleasures and pains who regard political activity as a necessary evil to be endured for the sake of peace and, above all, "comfort" (PR, 1972, par. 191A, p. 269). This view of politics, which Hegel's Marxist critics attribute to him with an otherwise rare unanimity, is rejected emphatically by Hegel. While he regards the market and the liberation of individual self-interest from the constraints of communal life as an "achievement" of modernity, he is highly critical of those commercial republics, like England, where everything is judged in terms of exchange value alone. The particular virtue of civil society is that it provides a complex, articulated social structure within which human abilities can develop. The fact that we cannot develop in every direction does not indicate that Hegel's

views were "one-dimensional" or that he had an insufficient grasp of our protean nature. Virtue, for him, is developed in the conscientious and single-minded devotion to a single craft or activity (PR, 1972, par. 207, p. 133). Civil society provides the context within which one can not only find fulfillment in one's work, but can help to forge satisfying corporate and communal ties with others.

The second extreme is the democratic or participatory style of politics represented by Rousseau and the French Revolution. Democratic politics, we have seen, is couched initially in the demand for radical freedom or autonomy. This is not merely the freedom of the bourgeois who wants to pursue his own interests in the market place; it is the freedom of the citizen who desires liberation from private interests altogether. Radical democracy assumes that no one is free until everyone participates in the collective decision making of the community, or in the words of one of Hegel's communitarian rivals, until "the state and the constitution" are "ruled by a genuine communal spirit . . . indissolubly united by the holy chain of friendship" (PR, 1972, p. 6). Hegel rejects radical politics for presupposing what no longer exists. A community is not the product of the general will; rather, it presupposes an already existing community of opinions and customs that in the modern world cannot be assumed to exist. Attempts to create such a community can only result in terror, as in the French Revolution, or in the forced homogenization of society that one finds in contemporary totalitarian regimes. In the final analysis Hegel agrees with Oscar Wilde's dictum that the problem with democracy is that it simply takes up too many evenings.

Hegel's purpose, then, as Shlomo Avineri (1972, p. 83) has said, was to find a middle ground between Hobbes and Robespierre, between the market place and citizen virtue. Hegel believed he had

found such a middle ground in the existing class structure, with its corporations and professional associations comprising civil society. These institutions help to prevent further atomization and particularization, but also seek to connect the individual with some wider and morally satisfying forms of social life without merely submerging his identity or "personhood" within them. They provide, as it were, a "second family" between the *gemütlich* ties of the natural family and the more abstract, universal bonds of the state (PR, 1972, par. 252, pp. 152-53). To be sure, civic virtue no longer imposes the kinds of severe moral restraints demanded by either classical politics or biblical teaching. The grounds of civil society are altogether more mundane: respecting the law and following what is regarded as upright behavior (PR, 1972, par. 150, pp. 107-8). But Hegel takes the unheroic character of civil society to be its advantage. It provides for the rational, if somewhat bloodless, patriotism that the modern state expects.

Notes

Earlier versions of this paper were presented at the 1985 annual meeting of the American Political Science Association in New Orleans, and at the John M. Olin Center for Inquiry into the Theory and Practice of Democracy at the University of Chicago.

1. The most important of these are Rawls (1971), Nozick (1974), and Dworkin (1977).

2. The assumptions underlying this model are spelled out in Hollis and Nell (1975, esp. pp. 47-64) and Barry (1978).

3. The main texts consulted here and the abbreviations by which they are identified in the text are as follows:

- BR *Briefe von und an Hegel* (1952)
- LHP *Lectures on the History of Philosophy* (1955)
- PH *The Philosophy of History* (1956)
- PW *Political Writings* (1964)
- PHM *The Phenomenology of Mind* (1966)
- JR *Jenaer Realphilosophie* (1969)
- ETW *Early Theological Writings* (1971)
- PR *The Philosophy of Right* (1972)
- NL *On the Scientific Ways of Treating Natural Law* (1975)
- SEL *System of Ethical Life* (1979)

4. Hegel's views on the French Revolution have given rise to a vast and inconclusive literature. See Habermas (1974, pp. 121-41), Hyppolite (1969, pp. 35-69), Lukács (1975, pp. 10-16), Ritter (1972), and Shklar (1976, pp. 173-79).

5. On the debt to Montesquieu see Althusser (1959, p. 49), Lukács (1975, pp. 371, 492), Mosher (1984, pp. 179-88), Planty-Bonjour (1974, pp. 7-24), and Shklar (1976, pp. 142-46, 157-58).

6. For further evidence of Hegel's "Bonapartism" see BR (1952, vol. 1, p. 120): "This morning I saw the Emperor [Napoleon]—this world soul (*Weltseele*)—ride through the town on horseback." See also BR (1952, vol. 1, p. 185), where he refers to Napoleon as "the great constitutional lawyer (*der grosse Staatsrechtslehrer*) in Paris." For his later views on Napoleon's constitution making see PR (1972, par. 274A, pp. 286-87) and PH (1956, pp. 451-53).

7. For a suggestive treatment see Arendt (1958, pp. 28-38).

8. The influence of political economy is the central thesis of Chamley (1963), Lukács (1975), Plant (1977), Riedel (1969, pp. 75-99), and Ritter (1972).

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NATIONAL ENTANGLEMENTS IN INTERNATIONAL GOVERNMENTAL ORGANIZATIONS

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There has been a growing propensity among states to associate together in international governmental organizations, or IGOs, for a variety of purposes. Why do states join IGOs, and what are the consequences for states of membership in IGOs? In this analysis, an explanation is sought, drawing on the theory of functionalism espoused by David Mitrany and others, taking into account the number of years a state has had sovereignty, level of technology, extent of party competition, and overall power. For Third World states, membership in IGOs is associated with enhanced economic performance. An increasing number of IGOs in the system appears to lessen the states' mean proneness to war. Functionalist predictions are upheld. But functionalism needs to be supplemented both for comprehensive explanations and as a prescription for the future. Already there are so many IGOs that it is difficult for states to control them, which could make them progressively irrelevant or even jeopardize their existence.

The global political system now contains more than 1,000 international governmental organizations (IGOs) of one type or another, and states are deeply entangled in this expanding web. Denmark heads the list by belonging to 164 IGOs; 19 states are members of 100 or more, and the mean number of memberships held by member states of the Group of 77, the Third World Caucus, is over 61. The entanglement of states and international governmental organizations has rapidly increased. The

United Nations and its related agencies, the European communities, the Organization of American States, and the North Atlantic Treaty Organization are well known, but states have joined together and created a great many more formal institutional structures than those prominent examples. This study describes the growing web of IGOs, analyzes the propensity of states to join them, and assesses the broad consequences of a state's total IGO memberships for its economic performance and conflict behavior. It then

raises questions about the implications for international relations of a continuation of the multiplying entanglement between states and IGOs, particularly in light of the apparent resistance to this trend evidenced by the U.S. and British withdrawal from the United Nations Educational, Scientific and Cultural Organization (UNESCO).

Functionalism as an Explanation

The theory of "functionalism," developed by David Mitrany (1933, 1966) and his followers, remains virtually the only corpus of scholarship about IGOs that offers general explanations of why states create and join such organizations, and what the consequences of this would be. Mitrany and others also saw their version of functionalism as a prescription to guide the development of the global political system. Because their version of functionalism is the only theoretical persuasion available to guide this analysis, it will be the point of departure.

Functionalism maintains that states create and join international governmental organizations because of two broad historical tendencies that date from the nineteenth century: the extension and deepening of political participation within states, and the continual advance of technology. Functionalism argues that mass participation in political life will inexorably increase, that general populations everywhere are primarily interested in increasing their own standard of living, and that mass participation will make economic welfare the dominant concern of governments. Functionalism also argues that technology offers immense possibilities for improving living standards, but that international cooperation is essential to take full advantage of the opportunities provided by technology; states are simply too small. In the perspective of functionalism, IGOs are the

consequence of political pressures and technological opportunities.

Functionalism would expect and prefer that the membership and mandates of an IGO be determined by the problem at issue; the overwhelming majority of IGOs would and should have limited memberships and specific mandates. The point of establishing an IGO, according to functionalism, is to facilitate international cooperation with respect to a specific technical issue, not to establish a general political authority with broad scope and domain.

Functionalism postulates that entanglement in a web of IGOs will make states less bellicose. Given the pressures to join IGOs, and the economic benefits presumably gained from membership and participation in them, states would be loathe to jeopardize these benefits by escalating interstate disagreements to violent conflicts that would inevitably destroy IGOs. Moreover, the increased opportunities for communication that IGOs provide should make it easier for states to avoid or settle disagreements before they reach the stage of violent conflict. States ought to have an incentive to reach agreement because, presumably, they have a common interest in economic expansion.

To what extent do the international governmental organizations in the contemporary global political system and the interaction between them and their member states conform to these functionalist tenets? How adequate is functionalism as an overall explanation for the pervasiveness of IGOs? Does membership in IGOs affect the performance and behavior of states in the way functionalism suggests? This analysis examines the validity of functionalist tenets and assesses the extent to which functionalism must be supplemented for a full comprehension of the phenomenon of IGOs. It also raises questions about the adequacy of functionalism as a prescriptive guide to

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the future development of the global political system.

The Universe of IGOs

As a first stage in the analysis, it is necessary to describe the web of international governmental organizations as it currently exists. The basic source for information about the number and characteristics of IGOs is the *Yearbook of International Organizations*, which is published periodically by the Union of International Associations (UIA) in Brussels.¹ The data used in this analysis are derived from the nineteenth edition, which was published in 1981 (UIA, 1981).

Starting with the nineteenth edition, the *Yearbook* divides the organizations that are included into two major categories and four subcategories. The number of IGOs included in each subcategory in the 1981 *Yearbook* are:²

I. Conventional International Bodies	
A. Federations of International Organizations	1
B. Universal Membership Organizations	31
C. Inter-Continental Organizations	48
D. Regionally Delimited Organizations	264
II. Other International Bodies	
E. Emanations and Semi-Autonomous Bodies	405
F. Organizations of Special Form	287
G. Internationally Oriented National Bodies	39
H. Inactive or Dissolved Bodies	26
Total	1101

In addition, seven organizations listed in the *Yearbook* had been proposed, but were not yet in existence in 1981. Including these, 1108 IGOs are listed in the *Yearbook*, 1,075 of which were active in 1981.

The only IGO included in subcategory A is the United Nations. The United Nations' specialized agencies and other similar agencies comprise subcategory B. The International Exhibition Bureau and the International Olive Oil Council are

examples of the type of organization included in subcategory C. The European communities are the most prominent of the organizations included in subcategory D.

The 344 organizations included in subcategories A through D are indisputably IGOs. The 731 organizations included in subcategories E through G have a more ambiguous status. Subcategory E includes such organizations as the United Nations Conference on Trade and Development, which, though it is a creation of the United Nations, has a larger budget, staff, and program than most of the organizations listed in subcategories C and D. It also includes some bodies, such as the U.N.'s Joint Inspection Unit, that because of their small size or apparent lack of autonomy are more questionable cases. Organizations such as the Integrated Global Services System and the Joint Nordic Organization for Lappish Culture and Reindeer Husbandry are included in subcategory F. The first is an offshoot of UNESCO and the second of the Nordic Council, but both are more than sub-organs, and in their characteristics resemble many of the organizations included in subcategories C and D. Subcategory G includes many of the joint ventures set up by the member states of the Council for Mutual Economic Assistance (CMEA), as well as such bodies as the Nigeria-Niger Joint Commission for Cooperation.

Because the distinctions between active IGOs included in subcategories B through G seem blurred, because in the aggregate the IGOs listed in the *Yearbook* as "conventional international bodies" and those listed as "other international bodies" have similar broad characteristics, and because information that is not available for all organizations is in general, proportionately equally available for those in both major categories, this analysis will usually treat IGOs collectively, without reference to the two categories. If those 344 organizations included in subcategories A

through D alone were regarded as true IGOs, as those who favor strict criteria might prefer, the generalizations developed here using the larger universe of organizations about why states join IGOs and the consequences of membership would apply, though the numbers would obviously be smaller. How many "real" IGOs there are in the global political system is obviously a matter of definition; "reasonable" definitions yield numbers that are larger than 344 but less than 1,075.

True to the prediction and preference of functionalism, the overwhelming majority of the IGOs that are included in the data set and could be classified according to their function and membership have specific mandates and limited memberships: 96.8% have functionally defined specific mandates, and 80.6% limit their membership according to one or another criteria. Of the total, 54.1% have mandates related to economic matters, and 56.4% limit their membership according to geographic criteria.

The Dynamic Evolution of the Web of IGOs

The preceding description provides an initial guide to the nature of the web of IGOs, but it is necessary to go beyond this. A sense of the dynamic processes involved in the creation of the web of IGOs can be gained from an analysis of the past, and trends from the past can be projected to foreshadow likely developments if these trends continued unabated.

The analysis can be structured conveniently according to four periods. The first period starts in 1815, the year the Napoleonic Wars ended and the first IGO, the Central Commission for the Navigation of the Rhine, was created. It ends in 1914 with the outbreak of World War I. The second period starts in 1915 and ends in 1939, with the outbreak of World War II. The third period begins in

1940 and ends in 1959. This is the period of World War II and the construction of the postwar international order. The final period starts in 1960 and ends in 1981, the last year for which data were available for inclusion in this analysis.

The rationale for breaking the post-World War II period in 1960 is that 17 states, the largest number ever, gained independence in 1960. By 1960, it was clear that colonialism was doomed and the nation-state system would be extended to the entire globe. As of 1959 there were 90 independent states in the global political system, 69 of which had been in existence in 1945 when the postwar period began. Between 1960 and 1981, 70 more would be added. Starting in 1960, decolonization fundamentally altered the global political system, at least in terms of the number of independent states included within it. The emergence of the new states led to an explosion in the membership of those IGOs that had come into existence before 1960 and—as will be seen—in the number of IGOs.

With the use of this periodization, several trends become apparent. Before examining these trends, however, it is important to emphasize that most IGOs are relatively recent creations. Of the 880 IGOs for which the date of founding is available, 94.1% were established after 1939, and 70.3% were established from 1960 through 1981. This pace of multiplication is astounding, and showed no sign of slowing. Indeed, more than 40% additional organizations were created in the decade of the 1970s ($n=354$) than were created in the 1960s ($n=250$).

The first notable trend is that progressively relatively fewer IGOs met the Union of International Associations criteria for being "conventional international bodies"; that is, for inclusion in the first four subcategories in the *Yearbook*. Sixty-five percent ($n=13$) met these criteria in the period 1815-1914, 59.4% ($n=19$) in the period 1915-1939, 47.1%

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($n=98$) in the period 1940–1959), and 31.8% ($n=199$) in the period 1960–1981. By far the largest share of the increase in other international bodies was accounted for by IGOs in subcategories E and F, which tend to be organizations that owe their existence to decisions of organizations already existing.

Stating the trend in another way, with the passage of time IGOs created more and more offshoots. The obvious advantage of this practice is that often all that is required to create a new IGO is a majority vote, not a new treaty that would require signature and ratification to take effect. Governments of states may also believe that it will be easier for them to keep track of IGOs that are offshoots of other IGOs than those that are totally disconnected from any existing structure. Whatever the reason, a large portion of the increase of IGOs since 1960 has been in the UIA category "other international bodies." The pace of creation of IGOs in the UIA category "conventional international bodies" reached a peak in the 1960s when 110 were established; only 86 were created in the period 1970–1981.

A second notable trend is the significant difference between the distribution of types of membership criteria for the international governmental organizations that were founded prior to 1940 and for those that were founded starting that year. The proportion of IGOs founded before 1940 that have no criteria in their constitutions limiting membership to particular political, geographic, economic, or cultural groups of states is much higher than it is for IGOs founded later.

This does not mean that in the years since World War II began states have not formed a large number of IGOs with potentially universal membership. Twenty-five such organizations were founded prior to 1940, and 143 starting that year. The absolute number for the post-World War II era is impressively high. What it does mean is that after

World War II began, limited-membership IGOs multiplied much more rapidly than those with potentially universal memberships. Starting in 1940, more than 80% of the IGOs that have been founded and were still in existence in 1981 limited their membership according to some criterion. In total, 682 limited-membership organizations were established during this period, and the real number is undoubtedly higher, since the UIA does not have founding dates for all IGOs. Geography has always been the criterion most frequently used for limiting membership, and this continues to be the case.

This trend should be interpreted in light of the fact that the sovereign states of the nineteenth and early twentieth centuries were much more homogeneous than they are in the late twentieth century. A universal-membership organization formed in the earlier years had a much less diverse membership than it does now or would have if it were formed in the present period.

That the relative proportion of IGOs that could have universal membership should fall off is logical. It would not be surprising if there were some upper limit on the number of universal membership IGOs that could be included in a global political system, even though this limit might be flexible over time.

Another factor is that as decolonization proceeded in the post-World War II period, and the number of sovereign states grew at an explosive rate, the opportunities for creating limited-membership IGOs also expanded rapidly. Organizations could be created both among new states and among new and old states. International bodies could make possible the continuation and extension of activities that were organized within the framework of a single sovereignty in the colonial era.

A consequence of the trend favoring limited-membership IGOs in the post-World War II period is that organizations

established from 1940 on tend to have significantly smaller numbers of members than those established prior to this date. The mean number of member states of IGOs in the latter category is 42.9, while that of those in the former category is 20.4.

To explore further the trend of an increasing proportion of limited-membership IGOs, it is useful to categorize states in order to see the extent to which different types of states have formed exclusive IGOs. A threefold categorization based on broad political and economic alignments divides states among those that are members of the Organization for Economic Cooperation and Development (OECD),³ those that are members of the Warsaw Treaty Organization (WTO),⁴ and those that do not belong to either of these organizations, a group that for this reason is called "Other."

These three categories are mutually exclusive and roughly place states into the groupings that are used in conventional political analyses. The members of OECD are those that are customarily referred to as the "West," and the members of WTO are those that are usually referred to as the "Soviet bloc" or group. The residual category of "Other" includes those states that are referred to as the Third World.⁵

Of the 563 IGOs for which membership information is available, 103 were comprised exclusively of states that were members of OECD; 28 exclusively of states that were members of WTO; and 178 exclusively of other states. Most of the Western IGOs were the basic agencies and offshoots of: the OECD, the North Atlantic Treaty Organization, the European communities, and the Nordic Council. Beyond the Warsaw Treaty Organization itself, the 28 Soviet-group IGOs were primarily derivatives of the Council for Mutual Economic Assistance (CMEA).⁶ CMEA includes three non-European states, Cuba, Mongolia, and

Vietnam, that for most purposes of this analysis are included in the "Other" category. In 1981 there were 38 IGOs comprised exclusively of CMEA members; these included the 28 comprised exclusively of WTO members.

The IGOs that were comprised exclusively of states in the category "Other" were less likely to be derivatives of other organizations than those comprised exclusively of OECD and WTO states. Of the IGOs comprised only of states in the category "Other," 58.6% were in the UIA category of "conventional international bodies," while only 47.1% of the OECD-only IGOs and 42.9% of the WTO-only IGOs were in this category. The economically more advanced Western and Soviet-group states have been refining their existing relationships through establishing additional organizations, albeit often subsidiary ones, while Third World states have been establishing relatively more new relationships and consequently more new primary organizations.

Since 1960, more IGOs comprised exclusively of states in the category "Other" have been established than IGOs of any other type. In the 1960s and 1970s, they accounted for about 40% of the IGOs formed in each decade. During the 1970s, these states began to create substantial numbers of IGOs in the UIA category "other international bodies"; they created 39 of these and 35 "conventional international bodies." By the 1970s, Western states were creating more than three UIA secondary-category IGOs for every one primary-category IGO that they established.

IGOs comprised solely of states in the category "Other" offer the greatest potential for growth in the near future. In 1981, only slightly more than one such organization existed for each state in this category, while there were more than three WTO-only IGOs and more than four OECD-only IGOs for each state in those categories. States in the category

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Table 1. Distribution by Function of IGOs Comprised Exclusively of Members of Particular Political-Economic Groups Established from 1960 through 1981 (in percentages)

Function	Group ^a		
	OECD	WTO	Other
General	4.5	0	3.8
Economic	47.0	96.0	73.7
Social	47.0	4.0	18.8
Security	1.5	0	3.8
	100.0	100.0	100.1
No. of cases	66	25	133
Chi Square = 29.28			
Cramer's Phi = .26			
Sig. = .00			

^aOECD = Organization for Economic Cooperation; WTO = Warsaw Treaty Organization.

"Other" could expand considerably the number of "conventional international bodies" among themselves, and they could increase the ratio between IGOs of this type and "other international bodies," moving toward the level established by the Western states. The evidence of the 1960s and the 1970s is that states in the "Other" category are moving in these directions.

There were also 104 IGOs in existence in 1981 that were comprised of states from both the OECD and "Other" groupings; they did not include any members from the WTO group. Thirty-one of these organizations were established in the 1960s, in the immediate aftermath of decolonization, and 22 in the 1970s. Many of these organizations could be regarded as providing elements of the framework of the world market economy.

The third notable trend is a tendency toward greater differentiation and variety in the mandates of the IGOs that have been established since 1939. While this trend is not as pronounced as the other two, it nevertheless is important. In the 1940s and 1950s, an unprecedentedly

large number of organizations that were established—28, or 13.5% of the total—had mandates dealing with security. Starting in the 1960s, progressively larger numbers of newly formed IGOs had mandates in the social field. This expansion of the numbers of IGOs with mandates to deal with social issues was most marked among OECD-only IGOs, although the "Other" group also moved to establish relatively more IGOs with social mandates. The record of the Soviet group is somewhat different. Only one of the 28 WTO-only IGOs has a mandate to deal with social issues; it was established in the 1970s. The Warsaw Treaty Organization's mandate is security; the remaining 26 (92.9%) of the WTO-only IGOs have economic mandates. Table 1 shows the distribution by function of the IGOs that were established starting in 1960 and were comprised exclusively of members of one or another group.

The growing differentiation of IGOs reflects a widely observed tendency toward specialization in political institutions. Security organizations were so relatively prominent in the 1940s and

1950s because the post-World War II political order was being created. This order has been relatively stable since those years, as is reflected in the fact that only 3.1% ($n=19$) of the IGOs established in the period starting in 1960 have security mandates. The increasing focus on social issues mirrors the focus on such issues that developed within states, particularly the advanced industrial Western countries, in the 1960s.

With their overwhelming concentration on economic issues, WTO-only IGOs stand apart from the general trend. Beyond the basic security commitment of the Warsaw Treaty, the Soviet-group states appear to have been almost inexorably drawn into economic cooperation, but they either have little desire or little necessity for institutionalized inter-governmental cooperation in other areas. The CMEA-only organizations are also heavily concentrated in the economic area (92.1%, $n=35$).

In sum, the web of IGOs in existence in 1981 was dense and complex. Although functionalist tenets accurately describe the broad characteristics of the web and its dynamic evolution, one must go beyond functionalism for a more detailed description and for a fuller understanding of the growth of the web. The basic dynamic forces in the global political system—the urge to create a new order in the aftermath of a destructive war and decolonization—had a strong impact on the processes and course of institution building, as one would expect. IGOs are, after all, instruments of states, and states are likely to follow policies in this sphere that are similar and related to those that they follow in other spheres. The evolution of the web of IGOs also reflects an internal development within states: the greater attention paid by governments to social issues. An important exception to this trend, however, is the paucity of WTO-only IGOs directed toward social issues.

The Propensity of States to Join IGOs

The analysis so far has indicated that states belong to varying numbers of international governmental organizations. Now that the web of IGOs has been described, this varying propensity of states to join these organizations can be analyzed in detail. Functionalist tenets again provide the point of departure.

To give an indication of the varying propensity of states to be members of IGOs, Table 2 lists the 26 states that held the highest number of IGO memberships in 1981. For each state, it gives both the total number of full and associate memberships in all categories of IGOs, and the number of full memberships in "conventional international bodies." Denmark's leading the list is explained by the fact that its unique position as a member of both the European communities and the Nordic Council gives it an unusual opportunity to belong to a large number of "other international bodies."

Fourteen of the 26 states, including all of those in the top 10, are from Western Europe; 3 are from Eastern Europe and 3 from Asia and the Middle East; 2 are from North America and 2 from Latin America; and 1 is from Africa and 1 from Oceania. These membership data reflect the fact that the IGO web is most dense and complex in Western Europe, where it began with the creation of the Central Commission for the Navigation of the Rhine in 1815. What is most impressive about this list of 26 states and the participation of states in IGOs more generally, however, is the extent to which the web has become global. Even Vanuatu, which just gained independence in 1981, held 11 IGO memberships that year, 4 of which were in "conventional international bodies." Joining IGOs has become among the first actions that governments take as soon as sovereignty is gained.

Functionalism argues that states will be

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Table 2. States with the Highest Number of IGO Memberships

Rank Order	State	Full and Associate Memberships in All Categories of IGOs	Full Memberships in UIA ^a Principal Category IGOs
1	Denmark	164	91
2	France	155	95
3	Norway	154	86
4	Sweden	153	87
5	United Kingdom	140	83
6	Finland	139	78
7	Federal Republic of Germany	135	83
8	The Netherlands	131	82
9	Belgium	127	77
10	Italy	124	72
11	United States	122	67
12	Spain	113	76
13	Canada	110	69
14	Japan	106	63
15	Iceland	105	54
16.5	Australia	104	67
16.5	Soviet Union	104	67
18	India	102	61
19	Brazil	100	60
20	Poland	99	69
21	Algeria	96	57
22.5	Austria	95	62
22.5	Yugoslavia	95	58
25	Egypt	94	60
25	Mexico	94	56
25	Switzerland	94	65

^aUIA = Union of International Associations.

propelled to join IGOs because popular pressures to increase living standards will lead their governments to engage in international collaboration to take advantage of the opportunities that technology offers to respond constructively to these pressures. Following this argument, one would expect that states with more opportunities for popular pressures to be expressed and at higher levels of technological development would belong to a relatively greater number of IGOs. The extent of party competition is an appropriate indicator for the first variable, and per capita gross national product (GNP) is an appropriate indicator for the second. For this analysis, Freedom House's four-fold classification of states in 1980 as (1)

multiparty, (2) dominant-party, (3) one-party, and (4) no-party (Gastil, 1981) is used to indicate the extent of party competition. The per capita GNP figures used are those for 1980 published by the World Bank in its *World Bank Atlas 1982* (IBRD, 1982a).

True to the functionalist argument, party competition and per capita GNP do predict IGO memberships.⁷ An ordinary least squares regression with these two independent variables produces an equation with $R^2 = .29$ ($n=160$, sig. = .00). The functionalist argument is supported, but less than 30% of the variance is explained, which leads to a quest for further factors that might influence the propensity of states to belong to IGOs.

Since the phenomenon of decolonization proved so important in the growth of the web of IGOs, it could also be an important factor in explaining the propensity of states to belong to IGOs. At the most basic level, the longer a state has had sovereignty the more opportunities it would have had to join IGOs. Since more than 90% of the IGOs in the global political system were established after World War II, it seems appropriate to assume that the exact order of states coming to independence in the nineteenth century or early twentieth century, or even earlier, would have little bearing on their propensity to belong to IGOs in the late twentieth century. Thus years of sovereignty—or of membership in the global political system—are measured starting in 1945. Since the data used in the analysis are for 1981, this independent variable has values from 0 to 37, 0 being for territories that did not have sovereignty in 1981, and 37 being for states that gained sovereignty in 1945 or earlier.

Traditional thought about world politics has always accorded special status to great powers. Given their presumed propensity to be extensively involved in world politics, one would expect greater powers to have more IGO memberships than lesser powers. GNP is the most convenient single indicator of power. The GNP figures used here are also taken from the *World Bank Atlas, 1982*.

Adding date of entry into the global political system and power to the two independent variables used previously sharply improves the explanatory power of the ordinary least squares equation. With the four variables included, the equation is:

$$\begin{aligned} \text{IGO Memberships} = & 36.29 + 1.43 \text{ System Years} \\ & (5.33) \quad (.14) \\ & + .002 \text{ Per Capita GNP} \\ & \quad (.00) \\ & - 4.43 \text{ Party Competition} + .00002 \text{ GNP} \\ & (1.34) \quad (.00) \end{aligned}$$

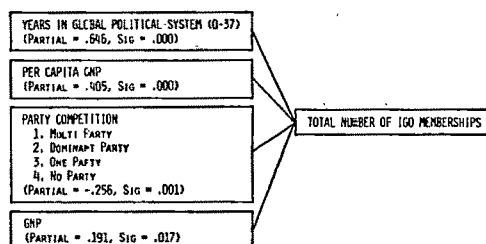
$R^2 = .61$; standard error of estimate = 1961; and level of significance = .00. (The partial coefficients are unstandardized; numbers in parentheses are standard errors.)

This equation explains more than 60% of the variance.⁸ The relationships are shown in Figure 1.

The four independent variables are listed in the equation and shown in Figure 1 in the order they were selected in a stepwise regression procedure. Years in the global political system is the most powerful predictor. An ordinary least squares regression equation with it as the sole independent variable yields $R^2 = .46$, larger than the R^2 of the equation utilizing the two independent variables suggested by functionalist tenets.

The effects of the four independent variables in the regression equation can be understood more clearly by interpreting the coefficients in terms of the measurement of the independent variables. According to the equation, each additional year a state has been in the global political system increases its total number of IGO memberships by 1.4; each additional 100 dollars in per capita GNP increases the total by 1.8; each step toward a multi-party system increases the total by 4.4; and, each additional 10 billion dollars in GNP increases the total by 1.6. Since the measure for party competition is ordinal, it is possible to obtain a better estimate of the effects of differ-

Figure 1. Factors Influencing the Propensity of States to Join IGOs (N=160)



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Table 3. Propensity of States Belonging to Particular Political-Economic Groups To Join IGOs (in percentages)

Relationship of Actual to Predicted IGO Memberships	Categories of States		
	Members of OECD	Members of WTO	Other
Underachievers	16.7	0.0	17.0
Normal Achievers	50.0	100.0	72.1
Overachievers	33.3	0.0	10.9
	100.0	100.0	100.0
No. of cases	24	7	129
Chi Square = 11.988			
Cramer's Phi = .194			
Sig. = .017			

ing degrees of competition by creating dummy variables. When this is done, it is clear that the largest difference is between those states that have multi-party systems and the others, but that there is also a fairly important difference between states that have a one-party system and those that do not have any parties.

The regression equation can be used to establish a "predicted" number of IGOs to which a state would be expected to belong on the basis of its characteristics according to the four independent variables. To give some examples, using the predicted values resulting from the regression equation, Denmark would be expected to be a member of 109 IGOs rather than the 164 to which it actually belongs; the United States a member of 145 rather than 122; Ethiopia a member of 71 rather than 46; and Algeria a member of 55 rather than 96.

It is instructive to examine the characteristics of the "outlying" states; those that belong to a total number of IGOs that is more than a standard deviation either above or below the predicted number. Those in the former category can be regarded as "overachievers," and those in the latter as "underachievers." Table 3 shows the relationship between the three categories of states that were used pre-

viously in this analysis and the predicted and actual membership in IGOs. Not surprisingly, the members of the OECD are more likely to be overachievers than are the members of the Soviet group or the "Other" category. The bulk of the OECD members are in Western Europe where the IGO web is most dense. Stating the relationships shown in Table 3 in a different manner, the mean actual number of IGO memberships of OECD members ($n=24$) was 116, while the predicted mean number was 106. The mean actual and predicted numbers of IGO memberships for members of the Warsaw Treaty Organization ($n=7$) were respectively 88 and 86, and for "Other" states ($n=129$), 59 and 61.

That there appear to be pronounced relationships between the type of economic system a state has and its propensity to belong to IGOs is more worthy of note. Table 4 shows the relationships. The categorization of states as having capitalist, state capitalist, mixed capitalist, mixed socialist, and socialist economies is taken from Freedom House's assessment as of 1980 (Gastil, 1981). Table 4 shows that states with mixed capitalist economies are particularly likely to be overachievers. This group includes not only Denmark, Finland, the Netherlands, Nor-

Table 4. Propensity of States with Various Economic Systems to Join IGOs (in percentages)

Relationship of Actual to Predicted IGO Memberships	Type of Economic System				
	Capitalist	State Capitalist	Mixed Capitalist	Mixed Socialist	Socialist
Underachiever	16.7	16.7	6.2	5.9	28.0
Normal achiever	75.0	69.0	43.8	88.2	64.0
Overachiever	8.3	14.3	50.0	5.9	8.0
	100.0	100.0	100.0	100.0	100.0
No. of cases	60	42	16	17	25
Chi Square = 19.690					
Cramer's Phi = .2791					
Sig. = .012					

way, Sweden, and the United Kingdom, but also Senegal and Tunisia. This tendency fits with the technocratic, planning undertones in functionalism.

Beyond these eight states and France and Italy, which have state capitalist economies, the overachievers are, like Senegal and Tunisia, developing countries. They include five countries with capitalist economies (Cameroon, Ivory Coast, Kenya, Niger, and Upper Volta), four with state capitalist economies (Ghana, Mauritania, Nigeria, and Bangladesh), one with a mixed socialist economy (Sudan), and two with socialist economies (Algeria and Tanzania). These developing countries have in common relatively activist foreign policies.

Table 4 also reveals a slight tendency for states with socialist economies to be underachievers. An examination of the other characteristics of the seven states in this category—Afghanistan, Albania, Ethiopia, Laos, Mongolia, the People's Democratic Republic of Korea, and the People's Republic of China—suggest that political factors may be as important as the type of economic system.

Again, functionalism has provided the first step toward understanding the propensity of states to belong to international governmental organizations, but as in the task of describing the web IGOs, it needs

to be supplemented with more traditional explanations of international politics.

IGO Memberships and the Performance and Behavior of States

Does belonging to international governmental organizations make a difference to the economic performance and conflict behavior of states, as functionalist tenets asserted that it would? Establishing causality definitively with respect to such complex issues is beyond the scope of this analysis, yet it is possible and worthwhile to examine the question and reach some tentative judgments about the extent to which expectations based on functionalist tenets have been met.

The essential tenets of functionalism with respect to the performance and behavior of states are simply the assertions that joining IGOs would enable states to improve their economic welfare, and that entanglement in a web of IGOs would tend to make states less bellicose. There is ample evidence relevant to these assertions. Their merits can be examined by exploring the association between the number of IGO memberships held by states and their economic performance and conflict behavior. The absence of an association would lead to a presumption

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against the validity of functionalist expectations. Although finding an association would not establish causality along the lines posited by functionalist tenets, it would create a favorable disposition toward the validity of these tenets.

Since, according to functionalism, states join international governmental organizations primarily in the expectation of obtaining economic benefits, it is appropriate to begin an analysis by examining whether or not better economic performance is associated with IGO membership. Economic performance can be and is measured in a variety of ways that are linked, but are not perfectly correlated. The ultimate goal driving the functionalist theoretical structure is increased individual economic welfare. The measures used here—growth in gross domestic product (GDP), manufacturing, and exports and imports, and improvements in terms of trade—are related to that goal, because they could make possible improvements in individual welfare. They are, at the same time, aggregate phenomena on which IGO participation could have a more direct and immediate impact than individual welfare.

The test used in the analysis will simply be to see if larger total numbers of IGO memberships are associated with higher rates of growth in GDP, in the manufacturing sector, and in exports and imports, as well as with better terms of trade. To posit that they should be is the baldest statement of functionalist tenets; putting the matter so starkly facilitates testing these tenets. The data used are from the World Bank's *World Development Report, 1982* (IBRD, 1982b). These and other available data could sustain more sophisticated analyses, but the crude tests used here can give adequate first approximation answers.

For the purposes of this analysis, the relative rank order of states according to the number of IGOs to which they belonged is assumed to have remained

relatively constant during the years from 1960 through 1981. The only measure of IGO membership is that for 1981, it is assumed that the rank order pertaining in that year was not very different in the preceding 20 years. The statistical tables in the several editions of the *UIA Yearbook* support this assumption. Thus if a state belonged to a large number of IGOs in 1981, it most probably belonged to a relatively large number throughout the period.

With respect to the member countries of the OECD and WTO, the data show no association between number of IGO memberships and economic performance. States in both categories belonged to a relatively large number of IGOs. Compared to the universe of states and territories in the global political system, all OECD and WTO members were relatively affluent. For these states, factors other than the relative number of IGO memberships appear to have been the predominant determinants of economic performance in the two decades starting in 1960; the relative number of IGO memberships seems not to have made much difference.

For states in the category "Other," in contrast, the number of IGO memberships is associated with all measures of economic performance used in this analysis: growth in exports and imports, and improvement in terms of trade. The last factor is measured by the index number for the terms of trade in 1980, an index which used 1975 as the base. Table 5 shows the results of ordinary least squares regression equations measuring the association between the total number of IGO memberships that a state held and the various measures of economic growth. The associations are significant, and the range of variables covered is impressive. Both GDP growth in the 1970s and manufacturing growth in the 1970s are also significantly correlated with the years that a state has been in the global political system, and adding this

Table 5. Association Between Total Number of IGO Memberships and Various Measures of Economic Performance: States in the Category "Other" ($n=129$)

Measure of Economic Performance		OLS Regression Equation ^a	R ²	Standard Error of Estimate	Level of Significance
GDP Growth, 1960s	=	- 1.219 + .072 IGOs (.706) (.011)	.25	2.93	.000
GDP Growth, 1970s	=	- 1.788 + .078 IGOs (.663) (.010)	.31	2.76	.000
Manufacturing Growth, 1960s	=	- 1.849 + .071 IGOs (.835) (.013)	.19	3.47	.000
Manufacturing Growth, 1970s	=	- 2.911 + .094 IGOs (1.004) (.016)	.22	4.17	.004
Export Growth, 1960s	=	- 2.293 + .113 IGOs (2.005) (.031)	.09	8.32	.001
Export Growth, 1970s	=	- 1.819 + .053 IGOs (1.250) (.020)	.04	4.42	.007
Import Growth, 1960s	=	- 1.281 + .079 IGOs (.970) (.015)	.18	4.03	.000
Import Growth, 1970s	=	- 3.231 + .104 IGOs (1.553) (.024)	.12	6.45	.000
Terms of Trade, 1980	=	-31.402 + 1.581 IGOs (10.268) (.161)	.43	42.67	.003

^aCoefficients are unstandardized; numbers in parentheses are standard errors.

variable to the ordinary least squares regression equations increases the R^2 in each case from .31 to .39 with respect to GDP growth in the 1970s, and from .19 to .22 with respect to manufacturing growth in the 1960s. In each case, however, more of the variance is explained by IGO memberships.

Given the predominance of IGOs with economic mandates and the fact that on the average 67.0% of the organizations to which states in the category "Other" ($n=129$) belonged were of this type—the average percentages for OECD ($n=24$) and WTO states ($n=7$) were respectively 65.7 and 77.7—extensive IGO membership implies extensive participation in an international political economy. The more IGOs a developing country belonged to, the more opportunities it would have to articulate its economic interests and to make efforts to defend

these interests. It would also have more potential sources for economic and technical assistance.

Analyzing the coefficients in the equations in terms of the way in which the dependent variables were measured illustrates the indicated effects of IGO memberships. GDP growth is measured in average annual percentage rates during the two decades, the 1960s and 1970s. For states in the category "Other," during the 1960s the lowest average annual rate of growth was -2.0%, the highest was 24.4%, and the mean was 2.4%. During the 1970s the lowest rate was -9.2%, the highest was 12.1%, and the mean was 2.1%. The coefficients in the two ordinary least squares regression equations indicate that membership in an additional 10 IGOs is associated with an increment of more than .7% average annual increase in GDP during each of the decades.

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Table 6. Mean Values for Various Measures of Economic Performance for States in the Category "Other" Belonging to Fewer or More than the Predicted Number of IGOs

Measure of Economic Performance	States Belonging to Fewer than Predicted Number of IGOs	States Belonging to More than Predicted Number of IGOs	% Difference Between Means
GDP Growth 1960s	2.5	3.7	51.7
GDP Growth 1970s	2.2	3.3	46.2
Manufacturing Growth 1960s	2.1	2.6	25.4
Manufacturing Growth 1970s	2.0	3.4	68.0
Export Growth 1960s	3.2	5.6	174.6
Export Growth 1970s	0.9	1.9	121.1
Import Growth 1960s	3.0	3.8	28.2
Import Growth 1970s	2.3	3.5	57.0
Terms of Trade 1980	49.2	76.5	55.5
No. of cases	65	57	

Viewed in this way, IGO membership had the greatest consequences for growth of exports during the 1960s and growth of imports during the 1970s.

States in the category "Other"—and indeed in the other two categories as well—as a group performed less well according to virtually all measures in the 1970s than they did in the 1960s. The differences in the associations between IGO memberships and most of the measures of economic performance suggest that in the difficult years of the 1970s, belonging to more IGOs was even more beneficial for developing countries than it was in the palmier years of the 1960s.

Another way of trying to assess the relationship of memberships in IGOs is to compare the economic performance for those states in the "Others" category belonging to more than the number of organizations predicted by the regression equation developed in the preceding section with those belonging to fewer than the predicted number. Table 6 gives the mean value for the same variables that were included in Table 5. In all cases the mean value is higher for those states that belonged to more than the predicted number of IGOs. This confirms the

impression gained from Table 5 that the economic performance of states in the "Other" category was positively associated with the number of IGOs to which they belonged.

Demonstrating this association, of course, does not prove that belonging to more IGOs caused better economic performance. It could well be that particular economic strategies resulted in both better economic performance and relatively more IGO memberships. For example, pursuit of an outward-looking development strategy that emphasized production for and competition in the world market could be the underlying explanation for both phenomena. Unraveling the thread of ultimate causality, however, is exceedingly difficult: governments of states could be attracted to such strategies because of their extensive participation in international governmental organizations. Whether or not IGO memberships caused better economic performance, the strong association indicates that at a minimum they facilitated this.

This analysis of economic performance thus partially supports functionalist tenets, but not for the group of states with which functionalist prescriptions original-

ly were most concerned: advanced industrial states. The findings indicate that if functionalist expectations about economic performance were valid, they may be more so for earlier rather than later stages of industrialization. They could also indicate that the marginal benefits of additional IGO memberships may decline, or that there may be a threshold beyond which further benefits do not accrue.

If these crude analyses regarding the economic performance of states give credence to at least a qualified version of functionalist tenets, what about the conflict behavior of states? The expectation based on functionalism would be that the more states became entangled in a web of international governmental organizations, the less inclined they would be to engage in interstate violence.

The measure of each state's inclination to engage in violence used here is the number of international—both interstate and colonial—wars in which the state has engaged since World War II, divided by the number of years that it has been independent during this period (Small and Singer, 1982, pp. 82–99). The result can be regarded as an index of proneness to war during the post-World War II period. The reasonableness of exploring the validity of functionalist tenets by examining the association between this index number and the total number of IGO memberships held by a state in 1981 again rests on the assumption that the rank order of states according to their IGO memberships has remained relatively constant over time: that those states that belonged to relatively more IGOs in 1981 also belonged to more earlier in the post-World War II period.

Testing the relationship in a bivariate manner reveals no association between the relative total number of IGO memberships held by a state and its proneness to war; belonging to a greater number of IGOs does not make a state less war

prone. Nor is any association discernable if states are divided into the three categories of OECD members, WTO members, and "Other." Perhaps, however, this test is inappropriate.

It could be argued that functionalist expectations about conflict behavior should be applied to the system as a whole, not to the individual states that comprise it. Phrased in system rather than state-level terms, the argument would be that the more IGOs there were in the international system, the greater the opportunities would be for conflict resolution. The data support such an interpretation. From 1815 through 1914 the mean level of the proneness to war of the 46 states in the international system was .05; for the 65 states in the system during the interwar period it was .03; and for the 156 states in the system during the post-World War II period it was .02. These numbers reflect the fact that the period since World War II—when the number of international governmental organizations has rapidly multiplied and attained its present high level—has been relatively peaceful compared to other periods of modern era (Jacobson, 1984, pp. 190–192, 198–199). International governmental organizations, however, have clearly been only one of several factors, including the awesome destructiveness of modern weapons and bipolarity, that may have contributed to this relative peacefulness, and they may well not have been the most important one. The only broadly supported conclusion of this analysis may be the obvious one that now and in the near future IGOs by themselves cannot be counted on to eliminate violence from the global political system.

In sum, as functionalist tenets suggested, some states at least appear to gain economic benefits from participation in IGOs. The consequences of this participation for the conflict behavior of states, however, are more opaque; international war has not been eliminated, but IGOs

may have contributed to diminishing its frequency.

Functionalism and the Future Evolution of the Web of IGOs

Functionalism provided a good point of departure for an examination of the multiplying entanglement between states and IGOs in the latter half of the twentieth century. It gave reliable initial guidance about the type of IGOs that would be established, the factors that would propel states toward IGO membership, and the consequences of IGO membership for the economic performance and conflict behavior of states. Yet functionalism fell short of providing a comprehensive explanation. It had to be supplemented by traditional explanations of international politics.

The evolving web of international governmental organizations has modified the global political system, as functionalism argued that it would, but it has not yet radically transformed this system, as functionalism hoped would happen. The radical transformation may yet come. In the meantime, however, international governmental organizations, in addition to modifying the political system, institutionalize aspects of traditional international politics.

The evolution of the web of IGOs has been affected by the broad historical currents of world politics, as well as by the dynamics foreseen by functionalism. Decolonization explains much of what has happened, and surely the exclusive organizations established by both Western states and members of the Soviet group owe their origins in part to the deep rift between these two groupings stemming from the Cold War.

Probably the multiplying entanglement between states and IGOs will continue in the immediate future, and perhaps even at the same dizzying pace. The complexity

of modern life creates many pressures for states to establish additional IGOs. What has happened so far demonstrates the overwhelming sense of governments throughout the world that states no longer provide large enough frameworks for tackling pressing problems. As for modalities, there are not many barriers to the continuing generation of IGO offshoots. Finally, simply on the basis of mathematical possibilities, there are enormous opportunities for creating IGOs among Third World states. The regression equation developed here would predict that as the GNP and per capita GNP of Third World states rises, and if the competition of political parties within them increases, the number of IGOs to which each Third World state belongs will increase substantially. If the IGO web were to become as dense in the Third World as it already is in the West, the total number of IGOs would have to be multiplied several times.

The mere statement of these possibilities raises an issue that will have to be faced. The functionalist persuasion was enunciated and became popular before a dense web of IGOs existed; it provided an important impulse toward creating this web. The evolution of the web, however, cannot continue indefinitely on the basis of early basic ideas. States in the contemporary global political system on the average already belong to one IGO for every 356,490 of their inhabitants. The ratio is considerably more extreme for countries with numerically limited populations. For New Zealand it is one IGO for every 42,466; for Cape Verde, one for every 11,571; for Jordan, one for every 43,108; and for Costa Rica, one for every 28,013. Even for as populous a country as the United States, that belongs to fewer than the predicted number of organizations, the ratio is one for every 1,865,900. If the United States finds it administratively trying to formulate constructive policies for the organizations to which it

belongs, as it does, what must the situation be like for countries that belong to proportionately more IGOs and have much smaller bureaucracies?

In most countries a relatively small number of bureaucrats along with a few delegates are charged with the responsibility of overseeing the work of and formulating policies for some 60 international governmental organizations. The impossibility of doing these tasks well in such circumstances is obvious. When the majority of states in an IGO are in such a position, the control of the direction of the organization can easily drift to the secretariat or to a minority of activist delegates who can muster majority support. States, and particularly those that provide the greatest financial support, can easily lose control. The United States and other Western countries allege that this has happened in UNESCO, and the U.S. and British withdrawal from UNESCO is a result of deep disagreement with the policies of the organization. Whatever the wisdom of the U.S. and U.K. decisions in this particular instance, there is a general problem. Somehow the multiplying entanglement of states and IGOs will have to take account of administrative realities and possibilities.

Functionalist theory expresses a preference for international governmental organizations becoming relatively autonomous from the states that comprise them. Functionalist theory sees such relatively autonomous IGOs gradually guiding states. UNESCO, which became relatively autonomous, could demonstrate the unreality of this vision. Given the fact of national control over resources, IGOs are at some risk when they ignore the preferences of the most powerful states. To do so may make them irrelevant to contemporary affairs, or, more seriously, could jeopardize their existence.

It is clear that creative ideas that go beyond functionalism are needed to guide

the future evolution of the web of international governmental organizations. Such ideas will have to take into account and build on the empirical evidence presented here. Ways need to be discovered for effectively, constructively, and reliably engaging states in the web of IGOs that is continually being woven even more densely.

Notes

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1. The *Yearbook* contains information about both international governmental and international non-governmental organizations. The first edition of the *Yearbook* was published in 1909, and the most recent in 1983. The information contained in the *Yearbook* comes from responses to a questionnaire sent to the secretariats of international organizations by the Union of International Associations. The amount of information contained in the *Yearbook* about each IGO thus varies. There is a headquarters address for virtually all the IGOs that are listed, the date of founding for almost 80%, the member states for some 50%, the size of the staff for 12%, and the size of the budget for less than 3%.

2. Curiously, the numbers in the summary statistics included in the nineteenth edition do not correspond with the number of organizations that are actually listed. There are more IGOs listed in sub-categories D through F of the *Yearbook*, and fewer in categories C and G, than the summary statistics indicate. In addition, some organizations that have been inactive or are dissolved are included in categories B through G of the listing, and as mentioned in the text, these categories also include some IGOs that had been proposed but were not yet in

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existence in 1981. These latter organizations have been placed in a separate category, I, in the data set.

3. The members of the OECD are Austria, Australia, Belgium, Canada, Denmark, Finland, France, Federal Republic of Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, the United Kingdom, and the United States of America.

4. The members of the Warsaw Treaty Organization for the purposes of this analysis are Bulgaria, the Byelorussian Soviet Socialist Republic, Czechoslovakia, the German Democratic Republic, Hungary, Poland, Romania, the Ukrainian Soviet Socialist Republic, and the Union of Soviet Socialist Republics. Byelorussia and the Ukraine are not formally members of WTO, but they are members of several IGOs, and since they are part of the Soviet Union, which is a member of WTO, it seems appropriate to include them in this category.

5. It includes the 121 states that were members of the Group of 77 in 1981, and 50 other states and territories. Some of the 50 were not independent, but they nevertheless belonged to various IGOs, and thus should not be excluded from analyses. Were they independent, they probably would join the Group of 77, as indeed some of them have done after gaining independence in the years since 1981. The 50 also includes some small European sovereignties and other states such as Israel, Taiwan, and South Africa that are difficult to classify. All of these states need to be included in some group, and the latter group of states that are difficult to classify resembles the states in the "Other" category with respect to economic characteristics more than it resembles those in the OECD or WTO categories. The categories are used in the descriptions that follow, so that the inclusion of any particular state is not necessary: when IGOs composed exclusively of "Other" states are described, these bodies in fact seldom include those states that are difficult to classify.

6. The members of CMEA are Bulgaria, Cuba, Czechoslovakia, the German Democratic Republic, Hungary, Mongolia, Poland, Romania, the Union of Soviet Socialist Republics, and Vietnam.

7. In this analysis, the term "IGO memberships" is defined as including both full and associate memberships in organizations in both of UIA's categories. States' IGO membership using this

inclusive definition correlate almost perfectly the total arrived at using various narrower definitions. The generalizations derived from analyses using the most inclusive definition would also be valid if a more restrictive definition were preferred.

8. Using states' full memberships only in IGOs included in the UIA category of conventional international organizations as the dependent variable yields a very similar equation. The ordinary least squares equation is:

Full IGO membership in the UIA principal

$$\text{category} = 18.66 + .67 \text{ System Years} \\ (3.33) \quad (.09)$$

$$+ .001 \text{ GNP per capita} \\ (.00)$$

$$- 2.58 \text{ Party Competition} + .000009 \text{ GNP} \\ (.84) \quad (.00)$$

$$R^2 = .62; \text{ standard error of estimate} = 12.26; \\ \text{level of significance} = .00.$$

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THE POLITICAL ECONOMY OF TRADE: INSTITUTIONS OF PROTECTION

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Domestic support for a liberal commercial policy in the U.S. rests on a commonly accepted set of rules and norms. These rules and norms, best characterized as a defense of free and fair trade, arise from two different traditions. The first originated as a political reaction to high trade barriers preceding the Great Depression. Central decision makers took as a lesson from the depression period that short-term political forces should not determine the level of state intervention into the market. The second, to protect against unfair trade, emanates from a long history of state support for industries that claim foreign producers are pursuing unfair predatory practices. Established in statute before the Depression, these rules did not fundamentally change in the postwar period. Together, these decision rules are used to interpret American trade protectionism.

In the postwar period American commercial policy has centered on the creation and perpetuation of a liberal trade regime. The tenets of that regime call for opening national borders to the free flow of goods, services, and capital; allocating primary responsibility for distribution to the market; and consigning the provision of collective goods to international organizations. Although all aspects of the liberal regime were never incorporated into international trade relations, the United States has promulgated this liberal design and attempted, where possible, to inculcate a neoclassical view of optimal trade relations into international practice.

Continued domestic support for this liberal economic regime is due to the

acceptance of a set of rules and norms by the policy-making community. Although affected by nineteenth century British thought, America's version of liberalism is better described as a hybrid of ideas defending both free and fair trade. These ideas, encased in rules and norms, derive from both historical experience and institutional constraints. In particular, three behavioral rules determine the relationship between government and industry over the issue of protectionism.

Alternative Explanations

Currently popular are two approaches which offer alternative explanations for the extent of protectionism in the U.S.¹ The first, a structural approach, encap-

sulates protectionism within foreign policy goals. Structuralists argue that a nation's trade policy will reflect its foreign policy component; policy is associated with a nation's relative power position. Specifically, nations with preponderant power will have an interest in pursuing liberal trade (Keohane and Nye, 1977; Keohane, 1980; Krasner, 1976). The change in U.S. trade policy from support for high protective barriers against imports to its post-World War II support for openness is explained with reference to a change in America's relative international position. In the long term, policy always reflects the international configuration of power.

The second approach, an interest group or micro political economy analysis of politics, stresses the domestic component of trade policy. Analysis centers on the ability of interested groups to enact state policies that favor their product over foreign competitors in home markets. Although there is no clear consensus in the literature on the components of group activity necessary to assure translation of demands or interests into policy, there is agreement that explanation is rooted in group or economic interest, and in appropriate participation in the policy making process (Bauer, Pool, and Dexter, 1963; Lowi, 1964; Schattschneider, 1935). The shift in American policy in the 1930s to a less protectionist position derives from the same factor that explained protection in the first instance: a preponderance of social interest in one policy over the other.

With American economic and political power on the decline, and with American producers facing competition at home as well as in third markets, both approaches predict similar trends in U.S. trade policy. For one, increasing pluralist pressures from import-competing producers will lead to demand for policy change. For the other, the decline of American hegemony brings a decline in the benefits accrued

from support for an open trading system. According to both approaches, the U.S. should no longer be interested in providing the collective goods necessary to keep international trade liberal. If economic openness is contingent upon either societal support or the existence of a hegemonic power, both protectionism and economic closure should have increased in the U.S. in the 1970s. It is argued correspondingly, that as the U.S. moves further away from support for free trade, the world again will embark, as in the 1930s, upon that "slippery slope" which leads toward both high protective barriers and intense competition over scarce markets (for example, see Greenhouse, 1984).

Herein lies a paradox. The empirical evidence over the entire postwar period cannot validate either position. Although there has been a growing interest in the revision of America's trade policy, there is insufficient evidence to support the contention that the U.S. is moving down the slippery slope toward protectionism. Rather, American central decision makers continue to be as committed to liberal trade today as they have been in any previous period in American history. The evidence for the continued existence of a liberal trade bias is overwhelming. The tariff schedule is at an historically low level. The last piece of liberal trade legislation, the Trade Act of 1979, which implemented the Tokyo Round Trade agreements, passed Congress with minimal objections. In the 1980s the executive office has not only dismantled programs to help industries adjust to import competition, but has withdrawn a number of orderly marketing agreements (OMAs) on the grounds they constrain trade. This is not to argue that no impediments exist to the movement of goods into the U.S. Nontariff barriers continue to protect certain American industries, but they are less prevalent and less restrictive than they were 30 years ago. Quantitative restric-

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tions in the form of OMAs and voluntary export restraint agreements (VERs) have been increasingly used since the 1970s, but these agreements may not constitute evidence for a change in policy. Rather, given that the American executive is institutionally constrained as well as politically compelled to offer some response to beleaguered industries, OMAs may be understood to have arisen as an executive-centered alternative to more deterministic and protective forms of aid (Aggarwal, 1983; Yoffie, 1983).² In short, both theories lead an analyst to expect considerably more protectionism than is empirically evident.

If there has been so little change in America's general position on trade, why has an inordinate amount of intellectual energy and time been spent on worrying about the "slippery slope"? Since the early 1960s, dire warnings of an impending crisis in American trade policy have been heard from both academics and members of the government bureaucracy (Finger, Hall, and Douglas, 1982; Krauss, 1978; Viner, 1961). They warn of the potential for beggar-thy-neighbor policies; of the rise of powerful interest groups representing sunset industries no longer competitive internationally; and of unions which are responsible for high labor costs, the uncompetitiveness of American manufactures, and the slow adjustment of workers to changes in international demand. The continual reappearance of these predictions points to a series of flaws in the contemporary analyses of trade policy.

First, economists who investigate trade issues argue that aberrations in liberal trade result from pressures exerted by noncompetitive societal groups. Free trade conditions are the baseline. Deviations are due to political, albeit economically motivated, phenomena. Empirically, this leads to the study of the political strength of the less competitive traditional industries. Societal interest in changing general trade policy, as well as

existing variations in tariff rates, is reduced due to the undue political influence of these sectors. Since most trade economists are not interested in a positive theory of government, little thought is given to the process by which interest group pressure elicits a government response. The outcome is assumed. Undue attention, then, focuses on the "interest" in aid, rather than on the amount and effectiveness of aid received. Individual cases of requests for aid are studied, but the larger picture of relative openness is ignored. This is a failing both for interest group and economic approaches to protectionism. They both equate trade problems with protection and look at state responses to requests for aid as putting out brush fires, rather than as any systematic or positive choices by the state.

Secondly, for those who look to the configuration of individual or group interests that comprise a political coalition as the backbone of state policy, the prediction of protectionism by the late 1960s derived from the analysis that liberalized trade was a Democratic party issue in the 1930s. Liberalism, along with a range of other issues closely associated with New Deal policies, was expected to be abandoned with shifts in the political climate. Whether or not the majority coalition has changed, neither Democratic nor Republican leadership in the 1970s and 1980s has organized a coherent anti-free trade platform. If anything is peculiar to this issue, it is that the Republicans have become more vigilant free traders than ever were their Democratic party equivalents.

Thirdly, those who argue that trade policy is predicated on international rather than domestic constraints predicted a rise in protectionism in the late 1960s. For them, the Vietnam War was both causal and indicative of new difficulties facing the U.S. Reflective of the change in America's international position, foreign

policy in the 1970s was concerned with redefining vital interest and spreading the costs of maintaining the postwar alliance structure. As predicted, the U.S. did abandon the Bretton Woods system in 1971, and moved to a less U.S.-centered international monetary system. Such structural analysis would predict a deviation in the trade regime at least as substantive as that in monetary management. The absence of such a sharp break in policy has been explained away as due to the existence of a constraining trade regime (Krasner, 1979, 1982a, 1982b). Using regimes as variables, the hypothesis was introduced that the absence of trade shifts may reflect lags between interests and policy. Discontinuity between the two occurs when institutions, created to serve the powerful, take on a character of their own. However, there is yet to be a systematic theory that explains the longevity of institutionally based lags. Without such a theory, the concept of regime is a descriptive addition to structural analysis that does not abrogate the theory's general predictions.

In general, the overprediction of protectionism derives from the convergence of multiple theories that have given adequate explanations for the development of liberal policies, but have failed in their analysis of when policy shifts would occur. Although claims that protectionism has arrived, but in new forms, have been used to exonerate specific predictions, the more general claim that the U.S. is about to embark on that "slippery slope" is unfounded.

An Explanation for American Commercial Policy

How then should protectionism be understood in the U.S.? Protectionism, as a component of general trade policy, can be derived from a set of decision rules that are both historically and institutionally grounded. These decision rules reflect a

set of beliefs held by central decision makers, and are indicative of a liberal bias. This liberal bias contains components of the intellectual tradition from which it arose, but mirrors at heart the American experience with trade policy making. The existence of a bias is manifest in policy in which feedback from both the domestic and international environment is muted.

The primary historic experience that set the stage for liberal trade policy in the U.S. was the Great Depression. Although the liberal policies adopted by the British in the 19th century had earlier found their way into the American debate on trade issues, it was the unquestionable failure of the Smoot-Hawley tariff of 1929-30 which allowed the fundamental restructuring of trade policy. High tariffs did not clearly cause the depression, but Smoot-Hawley came to be closely linked with its onset. The utter bankruptcy of the policy, advocating high trade barriers as the means for dealing with domestic economic malaise, set up two necessary conditions for fundamental policy change. First, institutional responsibility shifted from Congress to the executive. Congress had to approve this reduction in its traditional and constitutionally mandated role in tariff making. The prerequisite for this action was the consensus that trade mismanagement was linked to electoral misfortune. Secondly, the depression caused the demise of the then dominant analysis of trade policy, and created the environment for the acceptance of an alternative approach. Intellectual traditions take hold at moments when prevailing analysis is shown to be deficient. As the political community searches for a new and, it is to be hoped, more successful approach, alternative theoretical frameworks are seriously examined and can be used to explain and codify policy decisions. The change in ideas about the utility of one foreign economic policy over the other cannot be reduced to dominance of the

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Democratic party. The change in ideas eclipsed any short-term change in government personnel. Rather, the 1930s was a period of fundamental restructuring of the role tariff barriers would play in solving American economic problems (Goldstein, 1983; see also Gourevitch, 1984; Odell, 1982; Taussig, 1914; and Tasca, 1938).

Economic decline breeds a myriad of explanations and remedies. The 1930s were no exception. Although the depression served to delegitimize protectionism, a coherent alternative position was not immediately available. Disenchantment with protectionism did not lead to the immediate opening of American borders. For policy makers, the lesson derived from the experience of the Depression was more modest. They concluded that the U.S. could no longer use tariffs to help non-competitive industries; the economic and political ramifications of ignoring market forces were too great. Politically this meant that short-term political forces, in the form of powerful and articulate industrial interests, could not dominate in trade matters.

This lesson, however, was not self-evident. The free trade position was argued by charismatic leadership in this period of Congress' search for an explanation for past failures. The passage of a piece of liberal trade legislation by Congress in 1934 did not herald the coming of America's liberal period. Rather, liberalism became ingrained as a policy bias because the return to abundance and growth was interpreted by intellectuals, especially academic economists, to be a result of the successive, although incremental, policy of reducing barriers to trade.

The American form of liberalism, however, incorporated pre-liberal norms, values, and, most importantly, institutions. Laws established to protect the American market from potentially predatory practices by foreign producers were in force by 1934. For example, anti-

dumping and countervailing duty regulations had already been established to protect America's position both at home and in third markets. In institutional design, these statutes service an earlier U.S. concern with issues more associated with economic nationalism than with later ideas of national gains from liberalization. These institutions changed little, even as America changed its general policy thrust. The existence of these laws, which remained bureaucratic and not executive-centered throughout the post-war period, points to the major American deviation from liberalism's intellectual roots. In short, the U.S. was never willing to allow cheating; if American producers were forced to rely upon market forces in order to be competitive, so too must foreign producers (see Skowronek, 1982).

If punishment of those who are competitive due to infringements of the rules of the market is a central decision rule, why did the U.S. sanction both the European Economic Community's (EEC) and Japan's explicit deviations from the rules of the General Agreement on Tariffs and Trade (GATT) trade regime in the 1950s and 1960s? The explanation most often given points to American security interests. High politics, which involved American security, took precedence over the low politics of economic policy making. Although it is correct to argue that American security interests and the liberal international economic design promulgated by the U.S. after the war are closely related, the onset of negative sanctions and political pressures on both the EEC and Japan for increasingly liberal policies after the mid-1960s did not signal a diminished security concern. Rather, the U.S. allowed Europe and Japan to cheat in the earlier period because their industries posed no threat to American producers. Cheating was allowed, but only in the abstract. As soon as producers from these regions became a threat, the U.S. did respond to defend home industries. Secur-

ity issues served to mute this nationalistic response only when institutionally possible: most of these statutes force the use of technical, and not political criteria when a case is adjudicated.

The difference between the first decision rule of denying aid to non-competitive industries and this second decision rule of not sanctioning predatory practices is that the former reduces to an issue of the relationship between state and society, while the latter is associated with earlier American foreign policy norms. At issue in the determination of whether to erect barriers to aid particular industrial sectors are a policy norm associated with the intellectual tradition of domestic laissez-faire and a policy rule adopted after the depression. In contrast, the decision rule which sanctions state intervention to forestall cheating derives from earlier mercantile norms. In short, liberal ideas existed side by side with an institutional structure created to service a far different set of foreign policy concerns.

A third and final decision rule derives from the policy-making style which has characterized American politics since the 1930s. With the Great Depression and the electoral dominance of the New Deal coalition, the norms of behavior between state and society over the issue of trade changed. Although intellectually liberalism was viewed as the correct direction for trade policy, its political counterpart of laissez-faire conflicted with the position that government must assure a hearing for plural interests. For reasons of domestic legitimacy, the American government had to look responsive. Although economic norms prescribed that constituent complaints go unheeded, political norms and existing institutions dictated the opposite.

The third decision rule for protectionism evolved from these dual demands. Policymakers in the U.S. cannot ignore domestic groups. However, that does not mean giving these groups what they want. The government needs to appear respon-

sive, not be responsive. Thus, if an industry articulates a need for aid the response can be only symbolic support. Although the U.S. was wedded to a foreign economic policy that assured some level of industrial disquiet, the government's response to these complaints could be symbolic. The third decision rule stipulates that if aid is to be awarded it is to be as unrestrictive as is politically feasible.

The observation that few petitioning industries were awarded effective aid—aid that effectively countered foreign market domination—can be interpreted in different ways. The continuation of import pressure could be argued to be due either to mistakes, that is, the application of a technically incorrect solution, or to the inability to make these weak American industries competitive. Both explanations are confounded by the existence of more effective agreements such as those found in textiles. The lack of a greater number of effective programs for trade relief instead suggests a coherence in American policy. Industries do not receive effective aid inadvertently. Rather, central decision makers have acted whenever politically feasible to limit aid.

In the postwar period, U.S. central decision makers pursued liberal foreign economic goals even though their policies met with resistance. Potential opposition was finessed, not ignored. Although logic suggests that America's economic decline should be reflected in changes in commercial policy, the U.S. is in much the same position as Britain in the early part of this century: the national state is biased toward a liberal future (see Blank, 1977).

Making the Case for Free and Fair Trade

Since 1934 there has been a dramatic liberalization in America's trade barriers. This successive decline has been well

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Table 1. Change in U.S. GNP, Imports, Exports, and Tariffs: 1948-1982 (%)

Years	GNP	Imports	Exports	Imports ^a	Tariff ^b
1948-1958	74	80	42	66	-20
1958-1968	93	160	93	176	+ 2
1968-1978	146	423	313	336	-47
1972-1982	162	339	325	274	-42

^aImport figure if petroleum is excluded.

^bDuties collected as percent of dutied imports.

Source: Calculated from *Historical Statistics of the United States: Colonial Times to the Present*, 1970, 1975, Part I, p. 224; Part II, pp. 884-900; *Statistical Abstract of the U.S.*, 1973, pp. 778-790; 1980, pp. 439, 872-884; 1982-83, pp. 833-844; 1984, pp. 448-449, 831-841.

documented (Destler, Sato, Clapp, and Fukui, 1973; Meier, 1973; Pastor, 1980; Preeg, 1973; Ratner, 1972; Tasca, 1938). This decline in trade barriers continued even as America's role in the world market shifted in the 1970s from its earlier dominant position. As displayed in Table 1, tariff reductions continue even as the growth of imports surpasses that for exports and the Gross National Product (GNP). In seven rounds of multilateral negotiations, American tariffs declined to an historically low 5% by the early 1980s. This was accomplished in the face of protectionist opposition fueled periodically by an overvalued dollar, by the perception of the use of unfair trade practices by trading partners, and by increasingly severe competition in the home market. Along with liberalization, American policy also has been characterized by increasing control of commercial policy by the executive, and by congressional delegation to a trade bureaucracy the authority to protect industries. In order to appease general and specific sectoral opposition, once the function of tariffs, Congress created and reinvigorated a myriad of mechanisms through which industries could articulate their needs.

Data are presented on five statutes that protect import-competing industries. Two deal with the problem of import penetration, three with the issue of unfair trade. All are argued to be facilitators of general

trade policy; all contain elements that both serve and deflect domestic opposition. In the period studied, the trade agencies monitoring these statutes grew in size and scope as a result of both congressional intervention and increased societal demands for aid. Their authority was, however, constrained both by executive intervention and by inordinately difficult criteria for the granting of protectionism. Even when aid was given, it often was too little, too late. Of the industries that did receive aid, few were able to re-establish a competitive American market share.

Escape Clause Cases

Since instituted by executive order in 1947 and incorporated into statute in 1951, the U.S. has always included safeguard provisions into trade treaties and legislation. These provisions allow an industry seriously injured by imports to be exempted from an American trade agreement.

Escape clause action is usually initiated by industry, although either the president or Congress may start the investigatory process. To receive protection under this clause, industry must be proved to be adversely affected by imports, not by other market conditions or inefficiencies. The final decision to use the safeguard provision always resides with the executive branch, which with advice from the

International Trade Commission (ITC—old Tariff Commission), decides if aid is to be given and the type. The remedies available include a higher tariff, tariff quotas, import quotas, orderly marketing agreements, adjustment assistance, or some aid combination. Congress can overturn a presidential decision that differs from an ITC recommendation, but it has never exercised this power.

Escape clause provisions are not intended to provide long-term protection from imports. Rather, the intent of the relief is to provide "orderly adjustment to import competition" for the affected domestic industry. In particular, safeguard provisions are not intended to help industrials "that fail to help themselves through reasonable research and investment efforts, or that fail to take steps to improve productivity and other measures that efficient industries must continually undertake to remain competitive" (Annual Report of the President of the United States on the Trade Agreements Program, 1976, p. 42). Escape clause provisions therefore have always included limitations on the length of time an industry may receive relief.³

Adjustment Assistance

Adjustment assistance was first provided in the 1962 trade act as an additional remedy to be given using escape clause criteria. In 1974, the process by which firms and workers could receive such aid was eased, and the responsibility for administration was removed from the ITC and given to the Commerce and Labor departments. If a recommendation for adjustment assistance is received by the president, he must give aid. Adjustment assistance aid takes the form of weekly trade readjustment allowances, which may supplement a worker's unemployment insurance benefits or be used for retraining, job search or relocation benefits for displaced workers, and guaranteed loans and technical assistance for

firms and communities affected by imports. This type of import relief, severely cut back under the Reagan administration, does not interfere with the U.S. free-trade posture. No further barriers to trade are constructed; rather, the government reimburses those adversely affected by the state's foreign policy goals.

Anti-Dumping Legislation

Enacted in 1921, anti-dumping is intended to prevent unfair foreign competition created by pricing items below their cost of production. A dumping ruling entails a finding of dumping by the Treasury Department (after 1979, by the Commerce Department), and a confirmation of serious injury to an American producer by the ITC. If both agencies report an affirmative finding, a special anti-dumping duty is assessed equal to the amount by which the price is below its foreign market value. There is no presidential discretion, but dumping charges originate and are awarded by the Treasury Department (by the Commerce Department after 1979).

Countervailing Duty Legislation

In existence since the end of the nineteenth century, countervailing duties are assessed whenever a bounty or grant, . . . i.e., a subsidy, is paid in a foreign country "upon the manufacture or production for export of any article or merchandise manufactured or produced in such country. . ." (*U.S. International Trade Commission Annual Report*, 1979, p. 9). A duty is levied equal to the net amount of the subsidy upon importation. After 1974, this provision covered both dutiable and non-dutiable merchandise. (Only non-dutiable determinations went to the ITC for an injury determination.) Under the 1974 trade act, the imposition of duties could be waived under a number of conditions by the secretary of the

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treasury. These conditions related to the effect of such duties on ongoing trade negotiations and on the foreign government's willingness to eliminate its subsidy program. Under the 1979 act, responsibility for countervailing duty investigations was transferred to the Commerce Department. All affirmative findings go to the ITC for a judgment on whether a domestic industry is being materially injured by reason of subsidized imports.

Unfair Import Practices

If foreign producers pursue unfair methods of competition which cause injury to an efficient domestic industry, their product may be denied entry. Under this statute, which has pertained mainly to patent claims, the ITC determines, subject to judicial and presidential review, whether the importer is violating the law.

The Findings

Three alternative approaches to the analysis of trade policy have been suggested. The first, an international structural explanation, predicts that policy outcomes would respond to changes in U.S. interests in the world. Variations should occur over time, mirroring changes in the international configuration of power. In general, American policy should be most liberal in the period from 1958 to 1968, and become increasingly less so in subsequent periods. The second approach, looking to domestic forces as explanation, expects the level of protectionism to increase with sectoral and/or group demands for aid. As issues of protectionism become more salient, Congress is expected to respond by becoming increasingly involved to insure that "hurt" industries obtain aid. The third approach offered here, looking to the role of ideas and institutions in policy making, expects American policy to be guided by a pro-free trade bias. The expectation is either that there is no relationship between time

and aid, or, at minimum, that there is an inverse temporal relationship to that suggested by structuralists.⁴ If aid is given, the expectation is that it will be symbolic, be oriented toward "buying off" potential opposition to further liberalization, or be the most liberal of a range of options facing a politically constrained executive. There should, however, be variation across types of aid. Aid to keep the market "fair" should have a higher success rate than aid which protects an uncompetitive sector.

In examining the five statutes detailed above, an increase in petition activity appears over time. As tariffs decreased and imports increased their market share in the 1960s and 1970s, American producers did react by petitioning the bureaucracy for protection. Also interacting with market pressures in the period examined are changes in the legislated procedures and criteria under which an industry could obtain aid. Along with market pressures, legal changes affect the utility of applying for aid. Although petition activity is self-evident when examining aid receipt, it too is skewed by the easing and tightening of federal laws.

Of the two types of import aid examined, adjustment assistance (AA) petitions increased most dramatically (see Table 2). From an average of 25 per year in the period from 1963 to 1974, they accelerated to a rate of 967 per year from 1975 to 1978, and to over 2,000 per year from 1979 to 1981. Comparatively, escape clause (EC) petitions were highest in the first and third periods examined (see Table 3). Between 1958 and 1962, and 1975 and 1978, they average 11 and 10 per year respectively. There was a sharp decline in interest in escape clause aid under the 1962 act, and again a decline appears to have occurred after passage of the 1979 Trade Act.

Of the cases involving market disruptions, petitions against unfair trade practices (UFT) increased from an average of 1 a year (1958-1962) to 17 a year (1979-

Table 2. Adjustment Assistance Cases: 1963-1981

Year	Number of Petitions		Yearly Average		Acceptance Rate	
	Commerce Department ^a	Labor Department	Commerce Department	Labor Department	Commerce Department	Labor Department
1963-1974	62	233	5	19	.37	.30
1975-1978	338	3529	85	882	.91	.45
1979-1981	1245	6213	623	2071	.81	.28

^aPrescreened at regional offices, Commerce Department data only for 1979 and 1980.

Source: See Appendix.

1982) (see Table 4). Countervailing duty (CD) cases increased from a low of 1 a year to an average of 25 a year in the latest period. CD petitions began a sharp ascent in 1974, reaching their highest level between 1975 and 1978 (see Table 5). Comparatively, anti-dumping cases (AD) remained almost constant in the years between 1958 and 1982 (see Table 6). As with CD cases, the exceptional period followed the passage of the 1974 act. The average number of petitions between 1974 and 1979 almost doubled.

Comparison of the five statutes reveals that all had a dramatic increase in petition activity after 1974. In three (EC, AD, CD), that increase slowed down somewhat in the early 1980s. Petition rate, however, does not closely mirror the

changes in market penetration presented in Table 1. Although interest in aid has increased over time, that increase has not looked the same across aid types. The reasons become more evident when each aid program is examined.

Escape Clause Aid

Three trends are noteworthy. First, the ITC rate approval has increased for escape clause cases over time. Compared with a 27% acceptance rate in the early 1960s, by the late 1970s the ITC was finding over 60% of the petitions they adjudicated worthy of import relief. This reflects both eased standards and increasing pressures on the American market. Technically, increasing numbers of indus-

Table 3. Escape Clause Cases: 1958-1981

Year ^a	Number of Petitions	Average Per Year	Number ITC Approved ^b	Number President Approved ^c	Acceptance Rate ITC	Acceptance Rate of President ^d	Acceptance Rate With ITC Remedy
1958-1962	56	11	15	8	.27	.14	.07
1963-1974	31	3	10	4	.33	.13	.03
1975-1978	40	10	24	9	.60	.23 (.20)	.03
1979-1981	13	4	8	5	.62	.38 (.23)	.08

^aOrganized by legislative periods.

^bIncludes Split Votes.

^cAn award of adjustment assistance alone is not considered aid.

^dNumber in parentheses indicates acceptance rate for industries not already covered by escape clause actions.

Source: See Appendix.

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Table 4. Unfair Trade Cases (Section 337 of 1930 Tariff Act): 1958–1982

Year	Number of Petitions	Petitions Per Year	Number Settled	Number Accepted	Rate of	
					Settlement	Acceptance
1958–1962	6	1	0	0	0	0
1963–1974	53	4	5	7	.09	.13
1975–1978	47	12	13	13	.28	.28
1979–1982	69	17	31	15	.45	.22

Source: See Appendix.

Table 5. Countervailing Duty Cases: 1958–1982

Year	Number of Petitions	Average Per Year	Number of Positive	Acceptance Rate
1958–1962	4	1	4	1.0
1963–1974	16	1	15	.93
1975–1978	149	37	45 ^a	.30 ^b
1979–1982	101	25	30	.30

^aThirty-three cases were waived.

^bEight percent if waived cases counted as negative.

Source: See Appendix.

tries were meeting the escape clause standard after 1974.

Secondly, although technically qualified to receive aid, a majority of these cases never received effective import relief. This finding is sustained by three types of data. First, the overall presidential acceptance rate reduced the effect of ITC positive findings. If only instances of new industries gaining escape clause aid are considered, the president reduced the rate of positive findings to 14% before 1974 and 26% thereafter. The low presidential acceptance rate may be the reason

that petition activity declined after 1979 even though bureaucratic acceptances were increasing. Not only did the president veto the vast majority of cases his bureaucracy deemed in need of aid, but in cases where he accepted a need, he often did not accept the recommended remedy. Although the overall presidential acceptance rate is on the rise, so too is the use of non-ITC remedies. This tactic, which denies Congress an opportunity to overturn a presidential veto, was increasingly used after the mid-1970s. In over 50% of presidential decisions made after 1958, the

Table 6. Anti-dumping Cases: 1958–1982

Year	Number of Petitions	Average Per Year	Number Approved	Acceptance Rate
1958–1962	142	28	5	.04
1963–1974	286	24	62	.22
1975–1978	168	42	26	.15
1979–1982	113	28	28	.25

Source: See Appendix.

Table 7. Effects of Aid on Market Share for Escape Clause Petitioners: 1958-78 (%)

Change in Market Share ^a	Outcome ^b	
	Unsuccessful	Successful
Market growth or market constant	14.9 (28)	1.6 (3)
Market decline	64.9 (112)	18.6 (35)

^aComputed by product categories. Market share is a measure of imports relative to the value of domestic shipments.

^bThe numbers in parentheses indicate the number of cases.

Source: See Appendix.

president accepted the positive ITC vote, but substantially deviated from the ITC's chosen remedy.⁵ Thus, in over half the cases in which a positive escape clause decision was rendered, the president was able to undercut the clout of the ruling by tinkering with the amount of aid awarded. It is in this context that OMAs need to be understood. They are executive-centered remedies which weaken the more protectionist recommendations of the ITC. If these deviations in remedy are incorporated into the figures presented in Table 3, the overall acceptance rate for industries in the postwar period declines to under 10%, with the vast majority of these deviations occurring after the mid-1960s.

Thirdly, following the point above,

when escape clause petitioners do get aid, import pressures seldom recede. Aid is often too little, too late, or too short-lived. When recipients are examined by product groups, they look no better off than those groups who were unsuccessful in garnering aid⁶ (see Table 7). Under 2% of those who received aid can be portrayed as having maintained market share over the postwar period. In short, it appears that aid has had a minimal affect on the industries examined (see Baldwin, 1982). In 42% of the cases in which the president did not agree with the technical remedy suggested by the bureaucracy, industrial market share declined (see Table 8). This is not to argue, however, that the ITC remedy is guaranteed to succeed. In less than half of the instances of

Table 8. Market Share and Presidential Decision: Cases Decided between 1958 and 1978 (%)^a

Market Share	President and ITC Agree	President and ITC Disagree
Decrease	26 (5)	42 (8)
Same or better	11 (2)	21 (4)

Source: See Appendix.

^aThe numbers in parentheses indicate the number of cases.

presidential-bureaucratic consensus did market share increase. It does, however, point to the institutional autonomy of the office of the president, and the limited history of government measures able to thwart import competition.

This review of the escape clause statute reveals three trends. First, although Congress has given much time and attention to appropriate escape clause procedures, changes incorporated into trade legislation have been undermined by presidential actions. Second, the role played by the ITC has been greatly diminished by presidential interference. The staff of trade professionals and their politically appointed commission become a much less interesting source of explanation for escape clause policy, since the decisions of the ITC are regularly overturned. Thirdly, in the aggregate, American policy with respect to the escape clause mechanism has not led to overt protection of industries unable to adjust to changed trade patterns. Using market share in 1963 as a bench mark we find that petitioning industries were not able to procure sufficient aid from the state to retain their market positions. Although in the short term some industries that were granted aid may have improved their market share, these industries continue to be overwhelmed by foreign imports.

These findings speak primarily against an interest-group analysis of protectionism. Although it appeared that the number of positive rulings increased with societal pressure, presidential intervention in the form and amount of aid granted and the inability of these industries to maintain their viability even with federal aid point to a problem with a pluralist analysis. These findings suggest that the government plays a far more autonomous role in granting aid than a society-centered theory would suggest. Petitioning parties may appear to gain a positive response, but this response may be merely symbolic.

Adjustment Assistance

Both in concept and form, adjustment assistance deviates from other aid types examined. Why then include this aid? Adjustment assistance is a central ingredient of the American trade program. Although adjustment assistance decisions are not tied to negotiations or concessions, industrial adjustment to international competition is central to the continuation of a consensus around a liberal trade regime. The American state cannot ignore groups who are adversely affected by national policy. The state can, however, either de-legitimize their claims or symbolically incorporate their needs into state policy. The industries most directly affected by liberalization are those that are unable to retool and produce competitive goods. These industries form a potential source of opposition to U.S. policy. Adjustment assistance is one policy that can mollify these industries. The costs of this institution are born by the state's budget, and are not passed on to the international community through changes in custom barriers. Because of this cost, the chronicle of petition activity decisions and payments can be seen as a measure of state support for liberal trade.

As Table 2 details, significant numbers of petitioners received aid. These numbers dwarf any of the other aid types examined. Although underused before 1974, transferring responsibility for AA cases to the Commerce and Labor Departments greatly enhanced the program's visibility. As opposed to other aid types, a majority of those who applied for aid did receive it. Although AA aid was consistently used by both Democratic and Republican administrations through 1980, the 1981 data reflects the decreased role such transfer payments have played in the Reagan administration. Worker aid receipts declined to 9% in 1981; in the previous two years aid receipt had run at 41% for worker cases.

Two complementary explanations may be suggested for this decline. First, in the 1960s and 1970s adjustment assistance served as a pressure valve. Potential opposition to liberalization was deflected through a system of transfer payments. Adjustment assistance helped keep the U.S. on its liberal path by reducing the need to give industries a protectionist trade barrier. Reagan's policy stance rejected both of these functions. The Reagan administration was committed to a reduced role for the state sector; governments should not use a system of redistribution as compensation for economic policy. Often characterized as an ideologue, Reagan argued for the benefits of trade unfettered by state involvement. Liberal trade did not need the assistance of an adjustment program. Secondly, the groups who had received the bulk of these transfer payments were groups whose political influence gave them a favored position in the Department of Labor. In an administration tied neither to organized labor nor to an aggressive Department of Labor, it is not surprising that such aid would cease.

Unfair Trade

A philosophical difference exists between cases of fair competitive trade that adversely affect an American producer, and cases of "unfair" trade that cause the same result. Belief in the long-term beneficial aspects of the market has led American decision makers to condemn foreign governments and foreign industries who interfere with "natural" market mechanisms, particularly when the goal of that manipulation is predatory. Since it is the relationship between the American state and private industry that is the model used to evaluate actions taken by other nations, no country is allowed to assist overtly home industries, or to interfere with consumer market preferences so as to undersell an American producer.

There are two problematic elements in the administration of laws which attempt to keep the market "fair." First, there is a problem in the discovery and definition of a violation. Aside from an apparent problem with data collection on the production of foreign goods, there is the more complex issue of what criteria are to be used to assess cross-national policies. States vary greatly in their philosophical and historical relationship to producers. The relationship found in the U.S. is not characteristic of that found in other polities. To use the U.S. case as a benchmark establishes criteria on the extreme end of a continuum. Other nations whose postwar recoveries depended on greater amounts of state participation find the American model a useless and impractical archetype. Further, it has been argued that the enforcement of American values on state-industry relationships not only interferes with a set of foreign policy goals that seeks prosperity in non-communist states, but translates into interference in other states' domestic policies. Such interference alienates allies, and in principle deviates from regime norms of sovereignty (Goldstein and Krasner, 1984).

Secondly, and related, a problem exists in the determination of intentionality. Foreign producers may without predatory intent engage in what Americans view as unfair practices. There is a dilemma in how to judge, for example, national pricing policies or subsidized research and development programs. In unfair trade cases, foreign practices were often not created to gain a market advantage, but merely reflected national goals. The difficulty of assessment, however, has not precluded the passage of legislation to protect American industry from such practices. On the contrary, unfair trade laws have a longer history than the other legislation studied. These two problems do serve, however, to undercut the potentially protectionist aspect of unfair trade legislation.

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In examining the legislative histories of unfair trade statutes, certain trends emerge. First, in all three, some administrative flexibility has always existed. In administering AD and CD cases the Treasury Department was never constrained by legislation to enact a particular type of policy. In AD cases, Treasury Department officials, who by the mid-1960s held a pro-liberal bias, had the ability to eliminate cases by ruling no "just cause" for an investigation. Even upon a positive preliminary determination, the ambiguous nature of defining whether sales were "less than fair value" (LTFV) gave much latitude to investigators. Though defined by 1300 lines of text in the *Federal Register*, the nature of the assessment process still contains a large element of arbitrary evaluation (Davis, 1966). And even if LTFV sales were ruled to have occurred, the ITC until 1979 had a wide amount of discretion in its injury determination. In particular, the causes the ITC studied in making its final determination did not necessarily have to apply to findings used in making the injury determination. For instance, the commission could find no injury, based on a re-determination that the foreign producer was not, according to their judgment, dumping, and overturn the Treasury Department's ruling.

This discretionary nature of the injury determination and its potential for becoming an overly protectionist instrument was of central concern to foreign producers in the Tokyo Round of GATT negotiations. The anti-dumping code agreed to in Geneva incorporates the less protectionist notion of "material injury," not just any injury, by reason of less-than-fair value imports.

For CD cases, no delineation existed before 1979 on how an investigation should be conducted, or on the appropriate criteria for duty assessment. Rather, upon completion of an investigation the Secretary of the Treasury decided

whether to impose the duty, and the appropriate "equalizing" amount. The 1974 and 1979 Trade Acts added an injury requirement in order to make U.S. law consistent with the GATT codes. In 1974, items admitted duty free became subject to countervailing duties only if the ITC determined that the practice had "the effect of substantially reducing sales of the competitive U.S. product in the United States" (*U.S. International Trade Commission Annual Report*, 1975, p. 3). The 1979 law extended this consideration to all items. The 1979 law also specifies for the first time procedures and guidelines for the new oversight agency, the Department of Commerce.

Unlike countervailing duties or cases of dumping, the administration of unfair trade did not change under the 1979 law. Although some free trade advocates have criticized the 1979 act for this (Marks, 1980), the organization of Section 337 had already included a number of "safeguards" against its application. In particular, both the judicial review procedure and the existence of a presidential veto have caused the law to be less frequently invoked.

Secondly, not only do all unfair trade laws exhibit administrative flexibility, but there is no clear criterion in the legislative histories of these laws for assessing whether they are getting more or less protectionist. In the countervailing duty and anti-dumping cases, the use of the injury determination in all cases after 1979 makes it more difficult to obtain aid, even with the change of departmental responsibility to Commerce. Injury tests in the past have undermined the use of these kinds of aid.

And finally, although the president holds no legal authority in CD and AD cases, office holders have increased their power over outcomes. Two cases exemplify this point. First, the imposition of the countervailing duty waiver under the 1974 act transferred final authority from

the bureaucracy to the secretary of the treasury. As a presidential appointee and member of the president's cabinet, actions of the secretary may be assumed to be reflective of the president's own position. Secondly, the existence of an extra administrative mechanism such as trigger prices gives to the executive authority in dumping cases denied him by the legislature. In effect, the trigger-price system created an alternative to the Treasury route for anti-dumping petitioners, allowing the president to undercut a more deterministic Treasury finding (see Crandall, 1981).

When data are examined, none of the three types of aid shows a straight increase in acceptance rates over time. Although petition activity increased rather dramatically in the 1970s for all three types of aid, government response has not mirrored the increasing activity. In the case of countervailing duties, the acceptance rate has declined; for anti-dumping petitions, the rate rose after 1962, but has not significantly changed after that. The acceptance rate of Section 337s went up under the 1974 law only to decline after 1978.

In countervailing duty cases, the high rate of acceptance in the early period reinforces the notion that subsidies were never considered legitimate. As the number of cases filed accelerated, however, the high acceptance rate undercut America's commitment to the further liberalization of world commerce. The waiver provision, used for over 90% of the products which had technically qualified for aid, was an executive-centered mechanism for circumventing duty increases. After the right of waiver elapsed, the acceptance rate mirrored what it would have been without executive privilege. After 1979, the inclusion of an injury test in all CD cases assured that the pre-1974 acceptance rates of over 90% would not recur.

Perhaps most striking about the anti-dumping duty data is its long history of

use. The anti-dumping law has been used consistently since the early 1960s as a mechanism to keep the American market "fair." Low approval rates in the late 1950s may be attributed to American economic preponderance—few foreign manufacturers had yet acquired the capacity to endanger American producers at home. With postwar recovery, anti-dumping legislation again became a central element of American trade policy. From an acceptance rate of under 5% in the early 1960s, dumping duties increased to an average 21% after 1963.

Instead of societal pressure as measured by petition activity, changes in legal criteria through congressional activity may be an explanation for changes in the acceptance rate of dumping cases. Petition activity decreased under the 1962 Trade Expansion Act, and increased dramatically under the 1974 Trade Reform bill. The acceptance rate went in a somewhat different direction. The years covered by the 1962 act were years of high acceptance rates for petitions (22%) relative to the periods covered by the 1958 and the 1974 acts. This increase occurred mainly in the post-1971 period when both the number of petitions and their acceptance rates accelerated rather precipitously. After 1979, acceptance rates went back up to that level found in the late 1960s. Between 1979 and 1982, 25% of all petitions were successful. These changes are not directly due to shifts in legal criteria. For instance, although low rates of acceptance are found under the 1974 law, most commentators, as well as America's trading partners, argued that the 1974 law made dumping receipt easier, not harder. However, low rates under the 1974 act may be due to the use of the trigger price mechanism to "pull back" steel cases. After 1979 and the shift of administration to Commerce, rates did increase, but to pin this increase to the change in administration would be premature. The Commerce Department is accepting petitions

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at about the same rate as did Treasury in the 1960s.

The cases of unfair trade or Section 337s complete the troika of cases examined which quell market disruptions. The procedure for obtaining a market exclusion as a result of a claim under Section 337 is the most technical of the types of protectionism examined. Section 337 cases typically involve complex issues relating to patent law and/or antitrust violation. Adjudication is conducted by one or two ITC administrative law judges who conduct hearings and make recommendations to the commission as a whole. The procedure encourages settlement between parties. Overall, in the period studied there was a greater number of settlements (49) than positive findings (35). Further, although there were other reasons why an ITC ruling could be overruled (most often due to action in the courts), the president intervened in almost none of the cases, although so empowered. Two explanations can be offered. First, in most cases a positive finding clearly represents an international predatory practice on the part of a particular foreign producer to "steal" American technology. The laws judged to be violated not only relate to international norms, but to domestic law enforced by the Federal Trade Commission. Patent violations or other forms of industrial espionage are not sanctioned by any international norm. Any nation that finds such violations is expected to react harshly. Secondly, a positive ruling arises from a complex process. It is difficult to argue with the finding of the ITC technicians. Thus it might be said that administrative clout is here contained in the sophistication of the procedures used to adjudicate the cases. In general, both the content of the ruling and the method of its determination may serve to dampen the incidence of a presidential veto. The nature of the cases may explain why settlement is more likely and presidential action less so.

The average rate of acceptance in the period from 1958 to 1982 for Section 337s has been 16%. That rate, however, has varied from none in the late 1950s and early 1960s to 28% in the later 1970s. The number of petitions has increased dramatically since the early period, as America's technological advantage has been undercut by foreign reproduction of American products. But, as these problems have arisen, so too have solutions. High settlement rates have undercut the government's need to exclude many of these products as private parties have been encouraged to arrive at some form of agreement.

Comparatively, of the three types of unfair trade legislation, more anti-dumping petitions were filed than either countervailing duty or Section 337 petitions. In all three, petition activity increased after 1968. Although anti-dumping petitions were more common, countervailing duty petitions had the highest success rate; Section 337s had the lowest. Of the positive rulings, most have occurred since 1968. Although the number of positive rulings have increased, so too have the number of political vetoes.

This examination leads to three general conclusions. First, in the 1960s societal interest in stopping trade penetration generally increased. In the three unfair trade types examined, only anti-dumping petitions were consistently filed throughout the period examined. After 1968, not only did anti-dumping petitions rise in number, but aid requests under countervailing duty and Section 337, less used from 1958 to 1968, became common. Since the data presented in Table 1 indicates that the American trade balance deteriorated quickly after 1968, we can assume that the increase in petition activity basically reflects increasing import pressure on home producers. Secondly, although the majority of affirmative findings occurred after 1968, they did not occur at significantly higher rates than in

the earlier period. It is correct to note an increase in the aggregate number of positive rulings. Success rates, however, are not increasing. Thus, although in raw numbers more industries are getting aid, this cannot be interpreted as an increasing proclivity of the administration to give unfair trade aid. As petitions increased in number, so too did the number of political vetoes and extra-legal settlements. Constancy, not change, better describes the adjudication of these laws.

Thirdly, there is some variation across agencies involved in unfair trade legislation. However, this variation has not changed significantly over time. Acceptance rates by the ITC tend to be higher than those of the Treasury Department or the Commerce Department. This is especially evident in anti-dumping cases where both agencies examine the same case (although under different mandates). Both agencies have increased their acceptance rates slightly over time, although after 1974 the Treasury's acceptance rate declines in both anti-dumping and countervailing duty cases. The ITC's acceptance rate also dropped slightly after 1974 for anti-dumping cases, but accelerated somewhat in that period for Section 337s. Commerce Department behavior in countervailing duty cases is below that of Treasury's tenure, and about the same as Treasury's for anti-dumping cases.

These findings relate both to structural and group theory hypotheses on American protectionist policy. As with the examination of escape clause cases, variation in policy due to either increased societal interest in aid or changes in America's trade position was not found to be a significant determinant of policy. Although aid did increase after 1968, positive rulings were stabilized by the use either of political vetoes or other extra-legal settlements. As in escape clause cases, the president has continually attempted to undercut the bureaucracy,

and has succeeded. Compared to escape clause cases, unfair trade cases demonstrate a higher rate of positive findings. This higher acceptance rate exists throughout the period studied, leading to the conclusion that the type of aid, and not social pressure, is a better predictor of success rates. In short, issues of fair trade receive a better hearing by the state than do issues relating to the loss of competitiveness of an industry.

Conclusion

This essay has examined American institutions created to aid industries adversely affected by import competition. Its central finding is that these institutions have not become increasingly protectionist over time. Rather, protectionism (1) has existed in the case of unfair trade legislation over the entire period covered, and (2) did not dramatically shift in response to domestic competition. As imports have increasingly dominated the market in the U.S., industries have responded by repeatedly asking the government for aid. The state's response, however, has been guided by a desire to adhere to liberal norms to the extent possible given institutional constraints.

Three findings need to be stressed before addressing the more general problem of assigning a weight to ideas in the explanation of American policy. First, in the aggregate, few industries obtain absolute protection from the trend toward lowered trade barriers, and even fewer are able to increase their protection after a concession has been made. It is clear that as the U.S. moved toward increased economic openness, domestic groups and sub-units of the state apparatus have put up resistance. However, the character of that resistance has been more obvious than the extent of its success.⁷ This finding seems to falsify the simple assumption that the U.S. is weak and penetrated. Increased

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pressures did not result in changed government policy, as theories based on a model of a penetrated American state would suggest (Katzenstein, 1977).

There may be a need, however, for a conceptual differentiation between large and small petitioning industries. Large industries are unusual on two grounds. First, they have the capacity to amass a tremendous amount of political pressure. Secondly, if granted aid, large industries represent a substantial deviation from America's liberal position. Analytically, two paths to protectionism may exist (Goldstein, 1983).⁸ This research suggests that the "low" path, the road for the smaller producer, is one in which symbolic receipt of aid has less of a role. Technical merit plays a greater role in the case decision. Conversely, on the "high" path, where there is a greater display of political pressure, there is a greater need for a creative response from government leaders. For example, allowing a positive ruling for the steel industry on its dumping petitions was unacceptable. In its place, some other remedies such as OMAs or VERs were offered. Although clearly not optimal, since these agreements do represent deviations from liberal policy, they are not only much less exclusive than the alternatives facing central decision makers, but are based on cooperative agreements with America's trading partners.

Secondly, certain conclusions may be reached about the policy process associated with commercial policy. In a classic analysis of tariff legislation, Lowi suggested a relationship between issue areas and policy-making processes (Lowi, 1964). Tariffs in the early part of this century were stated to be a public good amenable to pork-barrel distribution; all petitioners received some aid from Congress. In the postwar period, tariffs shifted issue areas, becoming regulatory goods and involving coalitions dictating who would receive protection. However,

the data presented here suggest that tariffs are neither a distributive nor a regulatory good. Even though the constellation of interests changed in the postwar period, there was no associated change in the amount or manner of distribution of protection. Those affected by market penetration were not ignored; rather, they were given some compensation, but not in the form or amount demanded. The state does respond to well-organized social groups, but it develops the selection of goods and the criteria used to distribute benefits. In the state-created myriad of goods to be given to import-sensitive industries, effective protection was excluded. Those industries affected by cheating in the home market, however, fared quite well throughout the postwar period. However, both executive intervention and, after 1979, new GATT codes serve to undercut even these numbers of positive aid receipts. This variability cannot be explained by changing societal forces or by market penetration alone.

Thirdly, in looking at the data on adjustment assistance and escape clause cases, we find support for the position that a free trade bias exists. This bias translates into noninterference into the private sector's ability to market its products; as opposed to acceptance of domestic Keynesian economic policy, the state follows a *laissez-faire* approach to trade policy. Liberal states, however, cannot ignore domestic constituencies. In the U.S., the political marketplace dictates that the state "pay off" groups in some manner. Compensating industries and workers either directly or through loans may be understood as an accepted way for the state to insure internal support. Just as nineteenth and early twentieth century industry received pork-barrel preferential tariffs from Congress, mid-twentieth century industry receives transfer payments from the executive. The difference is that tariff manipulation

is no longer an option with the acceptance of free trade.

The American executive, however, is not omnipotent. Rather, it is constrained by Congress, by the bureaucracy, and even by the tariff structure set in 1929. An examination of the legislative history of the trade agreements program shows that elements of both economic openness and protectionism can be found. Opposition to executive-centered trade liberalization was present from the beginning of the program in 1934, and reappeared each time legislation was required for further negotiation. In each case the president, appealing on national interest grounds, was successful in obtaining negotiating authority. In return, Congress checked presidential discretion by widening the scope of the trade-related bureaucracy. Congress reacted to constituent pleas for protection by easing the legal criteria used by the bureaucracy to redress import injury. Instead of intervention on behalf of a specific industry, Congress would ask the bureaucracy to adjudicate the case.

The position of the executive is the least ambiguous in this period. When confronted by a choice between giving aid or not, the executive gave no aid. When protectionism was mandated by the bureaucracy, the president often chose to give a transfer payment, to give less than recommended or, in the case of countervailing duties, to sanction a tariff waiver. In dumping findings, legislation leaves no recourse but to assess a duty. However, every effort was made to convince the exporter to halt the practice. This perspective suggests that the executive-centered program of a trigger price mechanism for steel was a preemptive attempt to avoid an inevitable rash of dumping findings that the industry could legally gain.

We may now return to the problem posed at the beginning of this essay. In essence, it has been argued that continued support for the liberal economic regime is

a function of the acceptance by the policy-making community of a set of rules and norms. These rules and norms were derived from both historical experience and institutional constraints. They are not "liberal" as articulated in nineteenth century British thought, but are a hybrid of ideas and institutions best characterized as defending "free" and "fair" trade. This hybrid is uniquely American, differentiating U.S. policy from that of our trading partners.⁹

The defense for an independent role in policy analysis for ideas does not rule out the importance of more traditional explanations for American policy. Rather, the argument made here is that an explanation of trade policy without inclusion of this variable will be incomplete. Assigning weight to the role of ideas as compared to other variables is a task still ahead of the social sciences (see work of Odell, 1982, and Blank, 1977). Certainly, in the case presented here, both legislative and executive behavior were as influenced by the association between protectionism and the Great Depression as they were by constituent pressures. At minimum, acceptance of liberal trade policy has meant that state policy has been less protectionist than would otherwise be predicted.

Finally, ideas may be argued to have a life cycle. Confronted by sufficient environmental feedback, in this case from international structural shifts in economic relations, ideas do change. Further, although America's international interest in promoting "free" trade did not conflict with domestic and institutional support for "fair" trade until the late 1960s, this incompatibility leads to conflict which will contribute to the "aging" of liberal ideas. Will the U.S. continue its free trade orientation in the 1980s and 1990s? Probably not, but the sufficient expansion for change will not be that there was a lag between structural shifts in the international community and changes in

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American policy, nor that a major coalition caused the state to change its policy. Rather, policy will change because the economic ramifications of openness will force state central decision makers to question their beliefs. In time the state will need to alter its decision rules so as to form a better fit with reality (see North, 1980). Certainly the institutional requisites for a protectionist America exist today as they have throughout the post-war period. But protectionism becoming a salient issue in the 1980s is not sufficient cause for policy change. Rather, economic decline must be re-connected to a cognitive conception of commercial policy, as was the case after the Great Depression, for the central orientation of tariff policy to again be examined. The type of policy alternative then chosen will be determined by the intellectual debate of the moment. Unless the American economy goes into fundamental decline, however, change will be but an incremental step away from its current liberal stance.

Appendix

Tables 2-8 are based on the following data sources:

Table 2: For data on petitions between 1963 and 1974, see *United States Tariff Commission, Annual Report*, 1963, pp. 18-22; 1964, pp. 6-9; 1965, pp. 3-4; 1966, pp. 1-2; 1968, pp. 3-5; 1970, pp. 8-13; 1972, pp. 7-12; 1973, pp. 6-12; 1974, pp. 6-11. For 1975 data, see *United States International Trade Commission, Annual Report*, 1975, pp. 7-12. Data for the years 1976 to 1981 were obtained from the data banks of the U.S. Department of Labor and the U.S. Department of Commerce.

Table 3: For data on petitions between 1958 and 1974, see *United States Tariff Commission, Annual Report*, 1958, pp. 3-8, 1959, pp. 3-9; 1960, pp. 6-10; 1961, pp. 5-12; 1962, pp. 4-10; 1963, pp. 4-10; 1964, pp. 4-6; 1965, pp. 1-2; 1967, p. 2; 1968, pp. 1-3; 1969, pp. 6-7; 1970, pp. 6-11; 1971, p. 7; 1972, p. 6; 1973, pp. 5-6; 1974, pp. 5-6. *Annual Report of the President of the United States on the Trade*

Agreements Program, Second, p. 7; Third, pp. 10-11; Fourth, pp. 18-19; Fifth, pp. 20-21; Sixth, pp. 12-13; Seventh, pp. 16-17; Eighth, pp. 21-22; Ninth, p. 19; Tenth, pp. 25-26; Eleventh, pp. 31-32; Twelfth, pp. 54-56; Thirteenth, p. 31; Fourteenth, pp. 33-34; Fifteenth, pp. 29-30; Sixteenth, p. 31; Seventeenth, p. 38; Eighteenth, p. 28. For data between 1975 and 1981, see *United States International Trade Commission, Annual Report*, 1975, p. 8; 1976, pp. 4-6; 1977, pp. 5-7; 1978, pp. 5-8; 1979, pp. 5-7; 1980, pp. 16-19; 1981, pp. 1-3. *Annual Report of the President of the United States on Trade Agreements Program*, Twentieth, pp. 38-39; Twenty-First, pp. 42-43; Twenty-Second, pp. 49-53; Twenty-Third, pp. 67-74; Twenty-Fourth, pp. 105-107; Twenty-Fifth, pp. 179-180; Twenty-Sixth, pp. 197-198. For ITC recommendations and presidential proclamations see *Federal Register*, February 2, 1957, p. 713; November 14, 1957, pp. 9043-9045; December 4, 1957, pp. 9687-9689; January 15, 1958, p. 281; February 5, 1958, p. 820; March 11, 1958, p. 1708; March 24, 1958, p. 1976; April 24, 1958, pp. 2721-2723; April 28, 1958, p. 2852; September 26, 1958, pp. 7475-7448; July 29, 1958, p. 6063; October 24, 1959, pp. 8625-27; July 6, 1960, p. 6340; August 29, 1960, p. 8278-79; May 20, 1961, p. 4411; August 8, 1961, p. 7113; March 26, 1962, pp. 2789-2790; March 27, 1962, pp. 2791-92; December 31, 1969, pp. 2045-52; January 3, 1970, p. 131; February 25, 1970, pp. 3645-47; March 3, 1970, pp. 3975-3977; April 26, 1972, p. 8371; February 26, 1972, p. 4125; August 24, 1973, pp. 22833-34; April 1, 1974, pp. 11861-63; January 26, 1976, pp. 3786-87; April 6, 1976, p. 14600; May 4, 1976, pp. 18398-99; June 15, 1976, pp. 24103-05; February 4, 1977, p. 9065; March 24, 1977, pp. 15978-79; March 28, 1977, p. 16489; June 24, 1977, pp. 32430-39; June 27, 1977, pp. 32747-54; November 15, 1977, pp. 59037-40; February 9, 1978, pp. 5592-93; April 11, 1978, pp. 15127-29; September 12, 1978, pp. 40555-56; November 9, 1978, pp. 52294-95; November 17, 1978, pp. 53701-02; December 20, 1978, pp. 59445-47; January 8, 1979, pp. 1698-99; February 26, 1979, pp. 10973-10975; December 7, 1979, pp. 70583-87; January 18, 1980, pp. 3561-63; August 27, 1980, pp. 57221-27; November 3, 1980, pp. 27217-18; October 7, 1981, pp. 49683-87; November 6,

1981, p. 55163; November 17, 1981, pp. 56407-08; December 3, 1981, pp. 62339-41; February 24, 1982, p. 7997. U.S. Congress, House, January 2, 1980. H. Doc. 96-245, pp. 1-2.

Table 4: For data on petitions between 1958 and 1974, see *United States Tariff Commission Annual Report*, 1958, pp. 30-32; 1959, pp. 30-33; 1960, pp. 33-35; 1961, pp. 35-36; 1962, pp. 30-32; 1963, pp. 26-29; 1964, p. 15; 1965, p. 6; 1966, pp. 6-7; 1967, pp. 9-10; 1968, p. 11; 1969, pp. 15-18; 1970, pp. 27-30; 1971, p. 14; 1972, pp. 14-16; 1973, pp. 13-14; 1974, pp. 12-14. For data between 1974 and 1982, see *United States International Trade Commission*, 1975, p. 15; 1976, pp. 7-9; 1977, pp. 9-10; 1978, pp. 11-13; 1979, pp. 10-13; 1980, pp. 24-30; 1981, pp. 10-12; 1982, pp. 11-13.

Tables 5 and 6: Data were obtained for the years 1958 to 1982 from two sources. Petitions were collected from the *Federal Register*, 1958-1982, for both Countervailing Duty and Anti-dumping Cases. Once a positive ruling was made by the Treasury Department, data on USITC (old Tariff Commission) rulings were found for the years 1958 to 1974, see *United States Tariff Commission, Annual Report*, 1958, pp. 32-34; 1959, pp. 33-35; 1960, pp. 35-39; 1961, pp. 36-47; 1962, pp. 32-34; 1963, pp. 29-32; 1964, pp. 15-19; 1965, pp. 6-8; 1966, pp. 10-11; 1967, pp. 11-15; 1968, pp. 11-12; 1969, pp. 19-22; 1970, pp. 30-36; 1971, pp. 16-17; 1972, pp. 16-18; 1973, pp. 16-17; 1974, pp. 16-17. For years 1975 to 1982, see *United States International Trade Commission, Annual Report*, 1975, pp. 15-16; 1976, pp. 6-7 and 10-12; 1977, pp. 11-13; 1978, pp. 10 and 16-19; 1979, pp. 8-10 and 14-17; 1980, pp. 20-23 and 30-37; 1981, pp. 4-7; 1982, pp. 5-9.

Tables 7 and 8: For data on petitions see note about Table 3 above. Data on changes in market share were computed from *U.S. Imports FT 210* (U.S. Department of Commerce), 1963, 1967, 1972, 1977, and the *Census of Manufactures* (U.S. Department of Commerce), 1964, 1967, 1972, 1977. Product categories were those designated in the *U.S. Standard Industrial Classification Manual*.

Notes

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1. These two approaches are not the only ones that could be offered to explain policy. In the more general field of foreign economic policy different authors suggest different typologies for arranging theoretical explanations. For example, Cohen (1981) presents seven modes of economic decision making; Odell (1982) examines five models, and Pastor (1980) analyzes a different five. (The construction of my own analysis was heavily influenced by Odell's "ideas" model and by Pastor's Executive-Legislative Politics model.) There are a myriad of ways to organize competing explanations. The two approaches used here are the two general types most frequently offered to explain change in an existing level of protection.

2. These remarks need two clarifications. First, the president has given periodic import relief throughout the postwar years. My claim is not that the U.S. never aids industries, but rather that these theories over-predict the amount of that aid. Second, OMAs and VERs are examples of trade agreements among nations that may not necessarily constitute protectionism as traditionally defined. As cooperative agreements over market share, OMAs are often the most liberal of a range of possible options facing an American executive.

3. The requisites for escape clause relief have varied with time. Under the 1951 law a domestic industry could petition the Tariff Commission which had one year to determine whether "increased imports had caused or threatened to cause serious injury." If the president rejected Tariff Commission advice, he merely needed to report the reason to Congress. (In 1958, Congress was given the ability to override presidential action by a two-thirds vote.)

The 1962 Trade Reform Act tightened escape clause criteria. Imports no longer merely had to "contribute substantially" to the industry's problem but had to be shown to be a "major factor." Causation had to be established between a trade concession and an absolute, and not relative rise in imports. The 1974 act returned to the pre-1962 standards. Imports again needed to only be a "major part" of industry malaise. The causal connection of increased imports to trade concessions was abolished. Congress could now overrule the president by a majority vote, but the president retained discretion over accepting ITC recommendations. After 1962, the president could award an industry receiving an affirmative finding with adjustment assistance.

4. If liberal ideology is posited to have a gestation period, protectionism would have a higher likeli-

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hood of being found in the later time periods.

5. Presidential remedies were coded into three categories: Agree with ITC; Agree with part of ITC remedy; Agree with none of ITC remedy. Table 3 reflects just the first of these three categories. If both the first and second are counted, the president agreed with the ITC at a rate of 9%, 1950-62; 9%, 1963-74; 8%, 1975-78; 16%, 1979-81.

6. Escape clause cases are here organized by the eight-digit standard industrial classification (SIC) codes covered by the decision. The number of cases and products may not match, since one decision could cover a number of SIC categories.

7. Since 1980 the overvalued dollar has been repeatedly blamed for both America's negative trade balance and the rise in protectionist pressures. However, for most of the post-1958 period, with the exception of 1973-1979, the dollar has been overvalued. Since an American trade deficit existed with and without an overvalued dollar, and the great expansion of protectionist interest occurs in the years of an undervalued dollar, this variable is at best one among many. Although clearly not helping America's trading position, there is no simple relationship between the dollar and official trade policy.

8. This point is clarified in Goldstein (1983). Three findings in that work are relevant. First, no sector was highly successful in all aid categories. Electrical equipment and machinery ranked first and second for escape clause cases; textiles and food and kindred products were first and second for unfair trade cases (primary metal industries were third); and food and kindred products and leather and leather products were first and second for adjustment assistance relief. In short, there was no privileged business sector. Secondly, econometric models of who gets aid in each category showed size to be unrelated to unfair trade cases, positively related to adjustment assistance, and negatively related to escape clause cases. (Size was measured by a number of variables including shipments and employment figures.) Who applies, however, does seem to be affected by this size variable. Thirdly, special solutions to import problems have been rendered to a discrete number of industries. Steel, autos, textiles, color television sets, and footwear are the best known, although world quotas have been set on a range of other items from clothespins to sugar. At the same time, producers of no great political weight have received substantial tariff or quota relief, such as the mushroom, citizens' band (CB) radio and motorcycle industries. The case-by-case data reveal no preference to give the better known industries aid; the aid granted to the CB radio industry was certainly more exclusive than that ever given to steel. The high road industries do have a greater probability of gaining political attention, but because of their weight in international trade will probably get a less exclusive agreement than could a less significant industry.

9. Although the U.S. has a unique set of ideological preferences on trade, we share subsets of these beliefs with others. The U.S. and Germany in the recent period and Britain in the nineteenth century, sharing a belief in free trade, hold a closer position on trade matters than the U.S. currently holds with either France or Japan. Variation between countries with similar ideological preferences is the result of differing historical experiences and thus institutional structures.

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POLITICS,
PRIMORDIALISM,
AND
ORIENTALISM:
MARX, ARISTOTLE,
AND THE
MYTH OF THE
GEMEINSCHAFT

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Nineteenth-century evolutionary historical schemas formulated in *Gemeinschaft-Gesellschaft*, or status to contract terms, which underpin theories of modernization and development in the contemporary social sciences, are now called into serious question. Recent archaeological discoveries show ancient society to have been preeminently contractual; and anthropological studies of family, clan, and tribe reveal them to be more than primordial relics in modernizing systems. A careful reading of the ancient writers—particularly Aristotle, who was read as lending support to the *Gemeinschaft-Gesellschaft* distinction—yields a very different picture, as Marx in fact appreciated. Family, clan, tribe, patron-client, friendship, and other affinal sets were constitutive of the ancient society of the polis, and have shown extraordinary durability in the modern political society of the Mediterranean basin. In the small-scale, urban, entrepreneurial, and commercial society of the Mediterranean polis, ancient and modern, which was characterized by a high degree of face-to-face interaction, people learned participation in the plethora of little-incorporated societies—familial, religious, cultic, and recreational.

Much of Western thinking about politics from the early nineteenth century on has been conducted in terms of a basic three-phase schematization that meets with wide general agreement. The evolution of Western social forms and institutions is seen as a transition from (1) initially primordial society, characterized by rule of family, clan, and tribe, to (2) political society, marked by emergence of the city-state, and then to (3) advanced industrial society based upon the modern nation-state. On reflection,

one notes a quite general acceptance of this schema from the inception of the modern social sciences in the nineteenth century, and virtually across their entire range. The work of nineteenth-century anthropologists in uncovering hitherto undiscovered tribal and primordial societies in Africa and Melanesia encouraged them to draw the lines more firmly between the political and the primordial, and vast numbers of social scientists have since become interested in promoting strategies of "development" to

assist "underdeveloped" societies that fall within the Western sphere of influence in order to make the move to modernization.¹

The canon for theories of modernization was set, as we know, by Max Weber's account of the rationalization of authority, the differentiation of bureaucratic structures and institutions, and the mobilization of resources that define this process. Weber, following Marx, gave the distinction between *homo politicus* and *homo oeconomicus* definition in a body of theory that accounts for the transition from the second to the third stages of the schema, or from political to modern industrial society.² But the theories of Marx and Weber, like those of their long line of predecessors who reflected on the origins of civil society (Hobbes, Locke, Rousseau, Adam Smith, Adam Ferguson, and others) were predicated on certain common assumptions about the transition from primordial to political society, or about the transition between the first two phases of our schema. This transition received considerable refinement in the hands of nineteenth-century thinkers with whom Weber was more directly associated: Lewis Henry Morgan, who in his *Ancient Society* (1877) characterized the transition as a shift from *societas* to *civitas*; Henry Sumner Maine, who in *The Ancient Law* (1861) characterized it as a shift from status to contract; and Weber's associate in the *Verein für Sozialpolitik*, Ferdinand Tönnies, who characterized it as a shift from *Gemeinschaft* to *Gesellschaft*, in the book of that title (1955).

There is, however, much confusion among these thinkers about what transition they are in fact explaining. Are they really describing the shift from pre-political to political society, or are they describing, rather, the emergence of the modern industrial state that has claimed (mistakenly, as I shall try to show) exclusive rights to the mantle of politics in its classical form? This perpetual ambivalence is obvious in Tönnies' distinction between community and association, and it is even evident in Marx. Marx, Tönnies,

and also Morgan, see the community, *Gemeinschaft*, as the ideal form, of which the characteristically modern form of association, *Gesellschaft*—exemplified by bourgeois, and subsequently mass, industrial society—is a corruption. However, they do not see primordial society, where property was held in common among the tribal orders, to have been the perfect form of community. It is rather the ancient commune of the polis, already admitting private property, patronage, and class politics, that they consider to have been the most perfect communal form of the past, from which modern mass society has deviated.

The matter is further confused by the fact that the study of ancient history itself fell under the shadow of Maine, Morgan, and their followers (from which it is only beginning to emerge) so that there were conscious—and less conscious—attempts to force the transition from Homeric to polis society into the status-to-contract mold. Other scholars saw in the tribal institutions of classical Greece and Rome evidence of primordialism eventually sloughed off as the political state consolidated.³

Developments in recent decades have conspired to cast doubt on the tripartite historical schema of primordial to political to modern industrial society. Not least among these have been the dramatic archaeological finds of the past century, which have turned up whole libraries of public and private documents and literary and religious works that give a detailed account of daily life in ancient societies for which, a hundred years ago, our only sources were Herodotus, second-hand reports of other classical writers, and the Old Testament.⁴

A second factor suggesting a reappraisal of the conventional macrohistorical schema is the recent analysis of tribal society, particularly in the Mediterranean basin, suggesting that a dichotomy between tribal/primordial compared with political (i.e., polis-based) society is unfounded. Indeed, in the case of Mediterranean and Near Eastern societies

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one can argue the opposite case: that for the most part there is in fact an identification between tribal and political society that demonstrates a continuity from ancient Greek and Semitic cultures through classical Rome and the modern Middle East. This does not rule out the possibility that a distinction is warranted between the primitive tribal societies of, for instance, Africa and Melanesia, and the "political" (polis-based) societies of the Mediterranean and Near East, but suggests, rather, that tribalism is itself a heterogeneous phenomenon, and that perhaps the more apt distinction is between urban/centralized and rural/nomadic decentralized societies.

A third factor suggesting the inappropriateness of the tripartite historical schema is recent work by comparative anthropologists, sociologists, and political scientists. Their efforts encompass network systems, familism, patron-client relations, and other affective, affinal, and traditional social structures and systems of power which exist alongside modern "rational" institutions, or for which in many cases apparently rational bureaucratic structures are surrogates. The persistence of familial and traditional associational groups—or their modern substitutes in the form of tribal and kinship networks, spiritual godparents, and the peculiar role of religious and sporting clubs and professional syndicates in Greece, Italy, and the Middle East—warrants serious consideration as more than primordial relics in modernizing systems.

The tripartite evolutionary schema that spells out the transition from primordial society to the classical polis, and from the city-state to the modern industrial nation-state as the *cursus honorum* of social development, was first called into question seriously by Weber. He rejected as tactfully as possible the *Gemeinschaft-Gesellschaft* distinction of his associate Ferdinand Tönnies, although he accepted Maine's concept of a shift from status to contract. Generally speaking, Weber was disinclined to accept evolutionary his-

torical schematizations, which put distance between him and Marx—who was a convinced evolutionist even to the point of embracing the theories of Charles Darwin as an historical model. But Weber's rejection of evolutionary models, partly due to a certain ethnocentrism that posed a cultural blinder, was not radical enough. He still tended to see the significant landmarks between different forms of society as watersheds in the history of Germanic culture—witness the emphasis on the *Burgertum* as the vehicle of transition from traditional to modern society (Weber, 1958).

A more radical revision of the conventional historical schema, and the one proposed here, would see the differences between the societies of *homo politicus* and *homo oeconomicus*, that is to say, between "traditional" and modern industrial society, not as chronological or developmental, but as primarily regional differences. The societies of Greece, Italy, and the Near East have been political from time immemorial, by which is meant they are urban, polis-based, and characterized by tribal, family, cultic, religious, and occupational institutions as networks of political power. The *politeia*, or republic, is thus a more or less successful aggregate of the little societies that constitute it which enjoy a life of their own and a fair degree of autonomy. Evidence that we now have for the ancient city from second millennium Babylonian sites of the period of the Hammurabi Code points to a composite entity within which tribal groupings, the court and its bureaucracy, and the rentier class of urban notables—a "closely intermarrying pool of judges, merchants and scribes" (Adams, 1969, p. 189)—had their separate locales. Foreign traders were located outside the city at the harbour, and enjoyed an "autarchic economic" climate (Oppenheim, 1969, p. 6) now considered typical of Near Eastern society. Documentary evidence, in the form of innumerable private contracts witnessed by city and royal officials, as well as by priests, professional men, and

other citizens of high standing listed according to rank, suggests a fair degree of autonomy for the city as a corporate entity, such that it could buy and sell real estate, decide legal cases, petition the court, and be held accountable for its actions. Other features reminiscent of the classical polis of antiquity and its legacy in the Islamic cities of the Mediterranean region are the election of eponymous officials for a one-year term, allowing the rotation of office among a small group of isonomous peers; the cooption of wealthy citizens to privately underwrite costly public functions (reminiscent of the Greek liturgy in later times); and the commercial development of agriculture by a slave-owning rentier class, with control of the hinterland from the city (Oppenheim, 1969, pp. 9-15). These are, indeed, the oldest urban, entrepreneurial, and commercial societies for which we have records, and their social structure has shown a surprising durability over some five millennia. Thus we may fairly characterize them as the habitat of *homo politicus*.

By contrast, Northern Europe, as it emerged from the decentralized rural communities of the Germanic and Celtic tribes, constitutes, along with its colonial empires, the locus of *homo oeconomicus*. Characterized by a separation from earliest times of military, political, and economic functions, this was a form of society which gave no real experience of political power to the majority of the population. Weber, in his comparison of Mediterranean and oriental cities with those of medieval Europe, pointed out that whereas the former enjoyed relatively autonomous existences as fortresses, market centers, and viable social communities, the medieval cities of northern Europe lacked autonomy, or enjoyed what autonomy they had as a dispensation of nobles with a monopoly of coercive force who lived in the countryside (Weber, 1968, vol. 3, ch. 16). Only in the relatively densely populated, heterogeneous, and contiguous community of

the city was competition for power possible, in which family and tribal structures, cultic, recreational, and occupational forms of association played such an important role. In their typical form as scattered and isolated household units, the familial structures of the Germanic and Celtic tribes were unserviceable as political vehicles. The cities of medieval Northern Europe arose not as *poleis*, but as extensions of the essentially feudal society out of which they emerged, in which the only public person was the king. The revival of Roman Law in the Celtic and Germanic cities saw, paradoxically enough, the transfer to the king of the ancient rights of corporate immunity, rights to property, etc. once enjoyed by the cultic *collegia*, the *universitates*, and other forms of popular society. This process reached its apogee with English theories of sovereignty, in which the King is defined as a "corporation sole," enjoying a public right to sue and be sued while possessing private and personal immunity under the principal of "the king can do no wrong" (which remains the basis of municipal immunity in the tort systems of English and American common law). Medieval and early modern theories of sovereignty, which laid the basis for the absolute monarchies of northern Europe, typically applied Roman law—specifically corporation theory, which had been initially developed to define the rights of subordinate groups vis-à-vis the imperial power—in this manner.⁵

The great mystery that has preoccupied macrohistorians since Marx, and Weber in particular—why capitalism, characterized by *homo oeconomicus*, developed in the West and not in the East, despite the ability of the ancient monetarized economies of Sumeria, Babylon, Pharaonic Egypt, and the Greek and Roman Empires to develop wealth on a large scale—may be explained negatively in the following way. The rise of capitalism in the West may be attributed to a number of inter-related factors: the general absence of

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indigenous political structures capable of obstructing or competing for the right to economic accumulation on the part of a few; the role of the monarchies in granting royal patents and commercial concessions, as well as land, on grace and favour terms; the complete lack of the experience of political power on the part of the majority, who merely shifted status from indentured serf to that of wage-labourer; and the absence of indigenous familial, tribal, cultic, and occupational clubs with relative autonomy which—in the urban, entrepreneurial, and political societies of the Mediterranean and Orient—have enjoyed intermittently the experience of power for millennia.

Not least among the factors differentiating the society of *homo politicus* from that of *homo oeconomicus* so that capitalism as we know it was peculiar to the latter, were the religious prohibitions against the lending of capital at interest (specifically among coreligionists, but also more generally) perpetuated from their pagan predecessors by Judaism, Christianity, and Islam. Indicative of the aspirations of the autocephalous communities of ancient and oriental cities to preserve their time-honoured democratic and fraternal spirit against large-scale monopolists, these prohibitions led to the perpetuation of ancient forms of association for business purposes. The economic imperative within a citizenry dominated by merchants to keep one's capital circulating at all times while spreading the risk as widely as possible (Goitein, 1967–1983, vol. 1, pp. 154–5, 203, 263) was pursued through the instrumentality of partnerships of various kinds, and the *commenda*, or limited joint venture. For these purposes, friendship became formalized as an association of trust that permitted economic diversification beyond the traditional economic units of family, clan, and religious confession. Indeed, as the superb collection of medieval documents, sacred, secular, and communal, for the Jewish communities of the Islamic world serves to demonstrate, "friend-

ships," as the foundation of economic activity, and partnerships were as common between confessional communities as within them.

Partnerships happily accommodated the mutual interests of the investor, who supplied capital to the venture in exchange for labour, and the non-property partner, who participated as an equal, thereby avoiding the dreaded employer-employee relation too easily mistaken for that of master and slave (and these were all slave societies). Among the plethora of forms of contractual association, partnerships in the form of the mutual loan (capital and labour), the *commenda*, as the European counterpart of this Islamic form of partnership came to be known (much discussed by Max Weber as of economic significance in the development of the Italian city states), were most nicely tailored to the needs of a society of urban notables who must distance themselves from "work" and tread the fine line between investment of wealth and profits from capital (Weber, 1968, vol. 3, chap. 16, pp. 1216, 1294–95).

An economy based on networks of voluntary cooperation expressed in partnerships, *commendas*, and family trusts, fitted the exigencies of social systems where economic enterprise was not considered the normal business of the state, and where economic intervention was limited to sporadic revenue raising and direct buying (Goitein, 1967–1983, vol. 1, pp. 66, 266–72). The legal and contractual form that these partnerships took followed wider patterns of contractual relations in entrepreneurial communities in which "freedom of contract" was sacrosanct, and where all contracts, including marriage, were individually tailored documents rather than ritualistic symbols. For instance, marriage contracts would enumerate the material goods and chattels brought to the arrangement, as well as the expectations and desires of the future partners (some including stipulations that the husband not remove his wife to the provinces without her express

permission [Goitein, 1969, pp. 83–84]) and provisions for divorce and demise, inheritance, the rights of the wife in the case of subsequent marriages, etc. In the same way commercial partnerships specified in detail the number and status of the partners to the contract; the objects of the contract (partners were not above stating as an express aim the desire to benefit from the greater prestige of one's associate [Goitein, 1967–1983, vol. 1, p. 174]); the nature and extent of the contribution that each partner made (capital, goods, labour, premises, etc.); the partners' share in profit and loss and conditions governing expenditure and disbursement; conflict of interest and whether or not the partners could enter other ventures in the same field; the duration of the partnership—normally for a year unless renewed; the accounting period, and so on (Goitein, 1967–1983, vol. 1, pp. 171–2).

Although apparently individualistic and potentially unstable, these individually contracted arrangements worked well in the relatively small-scale society of the city, where the reference group was finite and members interacted personally. The individual freedom thus afforded and the scope for individual enterprise, long masked by stereotypes of "oriental despotism" and "underdevelopment" have only recently been publicized (Goitein, 1967–1983, vols. 1, 2, 3; Goitein, 1969; Lapidus, 1967, 1969; Lewis, 1937; Morony, 1984). As Paul Veyne (1976) has demonstrated in his superb work on the ancient economy, a high level of regional autonomy was not inconsistent with a strong imperial power, for the ancient and oriental imperiums controlled their subjects at a distance, through the agency of the tax farmer, police, and the smattering of bureaucratic officials, who were usually, if not always, content to allow indigenous communities to conduct their business in peace, so long as they paid their taxes and did their military service. An indication of the low profile maintained by the imperial power is the relative absence of government build-

ings (Goitein, 1969, p. 90). Apart from the Mint and the Exchange, the police station, the prison, and the various offices which dispensed licenses for certain purposes, most of the public buildings and the volume of activity they represented—sugar factories, flour mills, oil presses, abattoirs, granaries, bourses, shops and bath-houses—belonged to the municipality, not the government. An equally impressive range of public buildings were the preserve of confessional communities within the city: synagogues, churches, and mosques, as well as schools, convents and monasteries, universities, law courts, and hospices. In fact, landholding by the religious communities as corporate bodies, through the "community chest" or in the form of benefices and *waqfs*, rivalled the individual and corporate holdings of the big merchants and notables.

The funding and administration of these communal organizations is a story in itself (see Veyne, 1976; Goitein, 1967–1983, vol. 2; Morony, 1984), best summed up in the concept of *évergétisme*, a neologism derived by French scholars from the term used in Greek inscriptions to honour those notables who through their wealth and good works benefited their city (*evergetein ten polin*), and corresponding to the Latin term used by Cicero, *beneficia*, rather unsatisfactorily translated into English as "services." Briefly, *évergétisme* accounts for the system of liturgies prevalent throughout the ancient and oriental worlds (and still present in the Islamic in the form of *waqfs*), whereby a floating class of notables, the political and economic elite of municipalities and their subgroups, undertook to fund public services, benefits, buildings, etc., at their own expense in exchange for public honours, prestige, and the right to rule.

The relative economic and political autonomy that such a system yielded for municipalities under the various empires of the ancient Near East—Hellenistic, Roman, Byzantine, Umayyad, Abbasid,

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Fatimid, Mamluk, and Ottoman—has been nicely described by Veyne in his account of conditions under the Roman Empire:

Independent or simply autonomous in relation to the central power, the city was the ultimate frame of reference for social life just as the Empire was the ultimate framework of political life. For it was the center for decisions in matters concerning everyday life, the reference point for social distance, comparisons being made from city to city. It was in the city that the *évergètes* shone by their munificence; it was their city that they wished to dazzle at the expense of neighbouring cities, raising in it the most beautiful monuments "ad aemulationem alterius civitatis" as [Justinian's] Digest put it [50, 10, 3]. Socially, psychologically, and, not least, administratively, the city was self-sufficient: it was autarchic in Aristotle's sense. When a Roman or a Greek, subjects of the Roman Emperor, spoke of his "country" this word, *patria*, designated his city, never the Empire. There was no such thing as a Roman bourgeoisie, but a Pompeian, Athenian, or Ephesian bourgeoisie. The artisans enlisted in the professional associations were not members of an international or a syndicate: there were only local associations and one was a member of the "college" of carpenters of Lyons or the bakers of Sétif. . . . One's country was visible to the eyes, for one was able physically to gather it together in one public place (in classical Greece it even happened that the whole city literally turned out). At that time it was in the town that "everyone knew one another" as we say of the village. Such were these societies where everyone knew one another, at least by sight or by name, where everyone was engaged with the same pre-occupations—and everywhere around them, thanks to the *évergètes* was an air of grand architecture. (Veyne, 1976, pp. 107-8, my translation.)

Such a romantic picture of civility is not too far from the truth in the great oriental cities where, as they became increasingly large conurbations, the frame of reference shifted from the city as such to the neighbourhood.

Marx on the Ancient and Oriental Communes

The nineteenth-century myth of the *Gemeinschaft* arises, as I have suggested, on the one hand from an ignorance of the highly contractual nature, revealed by subsequent archaeological discoveries, of

the ancient oriental society that long predated Greece and Rome (its continuation and culmination in so many respects), and on the other hand from a misinterpretation of the nature and function of family, clan, and tribal structures in antiquity. A careful reading of the ancient authors—Aristotle in particular—on the relation between the *polis* (city), and the *oikos* (family) might have raised suspicions from the beginning about the efficacy of the distinction. Marx, for instance, was in no doubt that the tribes of classical Greece were inherently political, familial forms constituting integral links in the network of power-sharing groups that made up the participatory structure of the polis. He contrasted the city-state, based on the twin principles of kinship and landed property, with the decentralized, apolitical society of the Germanic tribes that, lacking any on-going communal institutions, and specifically the corporate structures of a city, remained a mere collection of isolated households. However, the idea of progress, a nineteenth-century preoccupation, stood in the way of his admitting the implications of this insight. As it turned out, Morgan later won him over rather easily to the view that classical antiquity was in effect a primordial society in which the *gens* (clan) was the most significant social form. Nevertheless, this represents a change of heart on the part of Marx, whose understanding of the political forms of classical Greece, if not those of the Orient, showed customary perspicacity.

In the *Grundrisse*, in which Marx submits communal forms, and especially those of antiquity, to their closest examination, a theory is produced with striking parallels to Aristotle's.⁶ Like Aristotle, Marx has no anti-urban bias, and he refrains from glorifying the prepolitical, tribal society of the Mycenaean, or even of the Homeric age as *gemeinschaftlich* in comparison to the polis society of the classical period. Quite to the contrary, he considers the polis, with its rudimentary separation of communal and private property, and the incipient duality of

individualism and communism to which it gives rise—and which is the eventual source of its destruction—to be the authentic, and perhaps the highest, form of commune among historical forms of the past. Indeed, it is a curiosity of Marx's thought that the standpoint from which he judged Western European society of his day was that of the polis and its virtues. One could even dare to suggest that the phenomenon of "alienation," which sums up that judgment, is more a product of Marx's propensity to hold *homo oeconomicus* to account by the standards of *homo politicus* than it is a reflection of fundamental dislocations within the capitalist system. The agonistic virtues or warrior excellences of the polis, and the rough democracy of the all-male *gymnasia* and political clubs which constituted it, were perhaps mirrored for Marx in the struggles between the revolutionary parties and the fraternal socialist societies and syndicates of his own political world, social forms otherwise lost in the bland and depersonalized structures of the modern state.

Marx analyzes the communal forms of the pre-modern period into three groups: (1) the oriental commune, (2) the polis type, and (3) the community of the Germanic tribes. He betrays a general ignorance of oriental society, which we now know to have included city-states of the most highly developed type, by characterizing it as exclusively nomadic. In oriental society he sees the primordial type of spontaneous clan organization that he imagines to be the archetype for all forms of primitive society, and over which the city-state structures of the polis were seen to represent a significant advance. He maintains that in oriental society, communality (*Gemeinschaftlichkeit*) is accidental, a mere function of common land ownership and ethnicity. The polis community marks a watershed in the evolution of communal forms because of the very fact of the coexistence of communal and private property.⁷ Marx speculated that the classical polis evolved

gradually from the clan-dominated tribal commune, for which he believed the oriental city was the model: an "initial naturally arisen spontaneous community (*naturwüchsiges Gemeinwesen*)," constituted of "the family, the family extended as a clan (*Stamm*), or through intermarriage between families, or combinations of clans" (Marx, 1953, p. 375; 1973, p. 472). Once it became settled in the form of the polis "this original community (*ursprüngliche Gemeinschaft*)" was modified by "removal of the clan from its original seat," the "occupation of alien ground," and "new conditions of labour" which permitted the individual to "become a *private proprietor* of land and soil—of a particular plot—whose particular cultivation falls to him and his family" (Marx, 1953, pp. 378-9; 1973, p. 475). Thus two factors distinguish the second type of commune, the polis, from the first type, the oriental: (1) the separation of communal and private property in the more highly developed form; and (2) the urban nature of the polis as opposed to the characteristically pastoral oriental commune. "The history of classical antiquity is the history of cities," Marx maintains, "but of cities founded on landed property and agriculture" (1953, p. 382; 1973, p. 479).

Here, of course, he was quite wrong. Far from being antithetical, all the evidence suggests a direct parallel between the ancient oriental city and the classical polis. Babylonian and Assyrian cities from the second millennium B.C. were not pastoral communities, but farmed their fields (whether attached to the municipality or outlying), as commercial ventures underwritten by rentiers, either individually or in partnerships, using risk capital, mechanization, and employing professional managers and slaves. The produce was stored in state granaries under the supervision of municipal officials, and the proceeds disbursed under an elaborate accounting system, once taxes had been collected (Oppenheim, 1969, pp. 13-15). Control of the

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countryside by the city has continued to be a feature of Middle Eastern society, as has the mix of public and private ownership along these lines. Goitein points out that in the medieval period, while orchards were privately owned, fields and vineyards—which were classed as fields—were legally the property of the state and its officials (Goitein, 1967–1983, vol. 1, p. 123). Even under Nasserite socialism the same division prevailed, the private ownership of orchards was often in the form of partnerships where blocks of trees were apportioned according to shares, while fields were farmed as state collectives and worked by the *fellahin*.

The classical commune, the second form in Marx's series, is distinguished from the commune of the Germanic tribes, the third, by the same characteristics that distinguished it from the oriental commune: by virtue of the separation of private and communal property and by virtue of being a city—factors which are mutually related. It is "with its coming-together in the city" that "the commune (*die Gemeinde*) possesses an economic existence as such," Marx argues: "the city's mere *presence*, as such, distinguished it from a mere multiplicity of independent houses." In other words "the whole, here, consists not merely of its parts; it is a kind of independent organism" (1953, p. 382; 1973, p. 483).

It is clear that Marx considers Germanic tribal society as such as not properly corporate in the way that the city-state was. He refers to the tribal chiefs, scattered through the Germanic forests in their patriarchal households, and their periodic assemblies as a "coming-together (*Vereinigung*) . . . not a being-together (*Verein*); as a unification made up of independent subjects, landed proprietors, and not as a unity" (1953, p. 383; 1973, p. 483). More specifically, "the commune (*die Gemeinde*) therefore does not exist as a state or political body," as in classical antiquity, because it does not "exist as a city." This means that "for the commune to come into real existence, the free

landed proprietors have to hold a meeting, whereas e.g. in Rome it exists even apart from these assemblies in the existence of the city itself and of the officials presiding over it. . . ." (1953, p. 383; 1973, p. 483). Tribal society, like the oriental commune, is superficially communal due to gentilitian ties and familial forms in which the unity of the clan system "is represented in a chief of the clan family, or as the relation of the patriarchs (*Familienväter*) among one another" (1953, p. 377; 1973, p. 473). Only with the polis, however, does the commune possess "an economic existence as such" (1953, p. 383; 1973, p. 483). The difference between the classical commune and that of the Germanic tribes is for all the world like that between polis and Homeric *oikos* society made up of independent, self-sufficient households (*oikoi*):

In the world of antiquity, the city with its territory is the economic totality; in the Germanic world, the totality is the individual residence, which itself appears as only a small dot on the land belonging to it, and which is not a concentration of many proprietors, but the family as independent unit. . . . In the Germanic form, the agriculturalist [is] not [a] citizen of a state; i.e. not an inhabitant of a city; [the] basis [is] rather the isolated, independent family residence, guaranteed by the bond with other such family residences of the same tribe (*Stamm*), and by their occasional coming-together (*Zusammenkommen*) to pledge each others' allegiance in war, religion, adjudication, etc. (Marx, 1953, pp. 383–4; 1973, p. 484)

Marx believes that in the case of the Germanic tribes, like the oriental commune, initially social forms of appropriation represented by common "language, blood," and land are not translated into an incorporated community: "the commune (*die Gemeinde*) exists only in the interrelations among . . . individual landed proprietors as such; communal property (*das Gemeindegut*) as such appears only as a communal accessory (*gemeinschaftliches Zubehör*) to the individual tribal seats (*Stammsitzen*) and the

land they appropriate (*Bodenaneignungen*)" (1953, p. 384; 1973, p. 484).

The significance of landed property in classical antiquity under the polis form is that, although mediated by the commune, it can be owned privately, thus allowing individual self-determination within the bounds of communal interests. Reaping the advantages of a qualified individualism and greater development of human powers and capacities through private property, "the commune (*Gemeinde*)—as state [i.e. as polis]—is, on the one side, the relation of these free and equal private proprietors to one another, their bond against the outside, and is at the same time their safeguard" (Marx, 1953, pp. 378-9; 1973, p. 475).

Marx characterizes the economic activity of the classical commune in Aristotelian terms: it is properly directed toward the self-sufficiency (*autarkia*) of the community as such, and not the production of wealth. For this reason the polis as an affinal entity transcending family, clan, and tribe, which characterized oriental and Germanic societies, is superior to the spontaneous primordial forms of association that become its sub-units: "it is not cooperation in *wealth-producing* labour by means of which the commune member reproduces himself, but rather cooperation in labour for communal interests (*gemeinschaftlichen Interessen*) (imaginary and real) for the upholding of the association (*des Verbandes*) inwardly and outwardly" (1953, p. 380; 1973, p. 476).

Such a highly romanticized view of public-spiritedness in the polis is hardly consistent with the facts. One is prompted to wonder why, indeed, Marx ever referred to the classical polis—already showing the signs of the tension between private and communal property that would ultimately be the source of its destruction—as the most highly developed form of commune in historical experience. But one might have posed the same question to Aristotle, aristocratic in his predilections, and in many respects

still wedded to the Homeric code. Why, since he hoped to ban retail trade and curtail commerce, making no provisions for the new rich to participate in power, did he not, therefore, recommend the *oikos* society of the Homeric period—as a collection of households bound by primordial ties—over the polis, potentially divided by class and threatened by individualism and acquisitiveness?

Aristotle's response is similar to Marx's. "It is manifest," the former maintains, "that a state is not merely the sharing of a common locality for the purpose of preventing mutual injury and exchanging goods." "These are necessary preconditions of a state's existence," he notes, but "even if all these conditions are present, that does not therefore make a state (*polis*)." The city is "a partnership (*koinonia*) of families and of clans" for the purpose of "living well, and its object is a full and independent (*autarkous*) life." For this to be realized, members of the community must "inhabit one and the same locality and practice intermarriage." This, Aristotle argues, is "the reason why family relationships have arisen throughout the states, and brotherhoods and clubs for sacrificial rites and social recreation." The root of such forms of association is "the feeling of friendship (*philia*)," or love; for "friendship is the motive of social life," and "while the object of a state is the good life, these things are means to that end." In sum: "a state is the partnership of clans and villages in a full and independent life, which in our view constitutes a happy and noble life; the political fellowship (*politikon koinonian*) must therefore be deemed to exist for the sake of noble actions, not merely for living in common" (*Politics*, 3.5.13-14.1280b30-1281a15, Loeb ed., pp. 217-219).⁸

Ironically, no description could better fit the oriental city, a federation of autocephalous communities, each resident in its separate locality and governed by kinship, confessional and fraternal organizations, and by forms of mutual

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association through which politics and business were transacted. In ignorance of the documentary evidence now available, and due in no small part, one suspects, to a certain ethnocentrism, nineteenth-century European scholars consistently misinterpreted the oriental city and its relation to the polis. For instance, Max Weber (1968, vol. 3, chap. 16, pp. 1285-93) concedes that the Mediterranean *poleis* (which would surely feature the oriental cities) were spontaneous synoecisms of ethnically diverse communities that contracted to live peacefully together, and that maintained a rough equality among themselves, developing an increasingly democratic politics. The Northern European medieval cities, on the other hand, lived on the grace and favour of a landed nobility whose interests were served by the development of market and manufacturing towns only so long as the political aspirations of the townsmen were confined to policing their occupations through the guilds. At the same time, Weber quite arbitrarily attributes to the European medieval city a corporate identity and charter of rights and immunities absent in the oriental city (1968, vol. 3, chap. 16, p. 1220). Not only is this far from being the case—in Sippar of the second millennium B.C. a parallel structure of municipal officials vis-à-vis the court defended just such corporate rights—but it reflects the typically statist assumptions of most nineteenth-century scholars—and plenty of twentieth-century ones too. Marx himself was among them. So preoccupied are they with the centralized authority of the modern state that the past is read back systematically from the present. Thus the struggle of the Northern European *popolo* against despotic sovereign authorities is considered an historical advance over the decentralized, pluralistic, Mediterranean, and oriental communities that had already won their concessions, or perhaps never needed to. In Marx's case this view sits uneasily with his general glorification of the polis as a social form.

The extraordinarily speculative and undocumented view of oriental society which Marx and Weber represent, and which we now know to be false, has nevertheless been quite pervasive in its influence. It found its possible source in the mythology of Islam, which saw itself as a purifying force that purged the decadent "sedentary" civilizations of the Levant with the values of conquering Bedouin tribes from the hinterland. However, the self-perpetuated mythology of Islam as a tribal religious culture in its desert fastnesses was not even true in the time of Ibn Khaldun (1958), who bears a major responsibility for propagating it. Mohammed was the product of a sophisticated merchant city, and the early literature and the legal, educational, and philosophical traditions of Islam bear the stamp of a merchant class that played a formative role in its development (Cook, 1974, pp. 226-7). Orientalists have argued for some time now that the Islamic conquest gave rise to cities in which the newcomers were soon assimilated to the ways of the old, and where tribalism rapidly became a notional entity—a collective badge and a link with the past that tied together the Islamic urban elite, which displaced an older class of notables at the municipal level, and at the same time provided neighbourhoods an identity reinforced by the ties of family, clientage, common village origin, and the full range of fraternal organizations—Sufi orders, youth gangs, etc.—so reminiscent of the Athenian cults, fraternities (*phratries*), and often thuggish political clubs (*hetaireiai*) (Calhoun, 1913; Lapidus, 1969, pp. 49ff).

One can see time and again the confusion that Marx's (and Weber's) thesis about the unincorporated nature of oriental society still produces. For instance, Nadav Safran, addressing Weber's thesis directly, maintains that the reason why "the Muslim city did not develop a corporate political entity" or "strong interest groups," like those of medieval Western burgher-groups, is because cities in the oriental world, unlike those of medieval

Europe, were seats of government, and "the government as the dominant urban interest did not allow other interests to develop" (Safran, 1969, pp. 76-77). Therefore, "no independent society emerged," witnessed by the fact that "there were no autonomous laws and regulations in Muslim cities, and hence there were no corporate interest groups challenging the caliphate and forcing the development of constitutional law, as took place in the West" (Safran, 1969, pp. 76-77).

This is a veiled thesis of oriental despotism (Wittfogel, 1957). Moreover, it is implausible on its face. The struggle for a single corporate identity enshrined in a centralized body of law was unnecessary (1) because the state was only one of the salient administrative elements in these societies; (2) because government was indeed located in the city and accommodated the interests of its various communities; and (3) because these communities enjoyed severally a level of rights and immunities, economic independence, and intercommunal toleration, that is remarkable even by today's standards. Goitein and others emphasize the last point over and over for Egypt (Goitein, 1967-1983, vol. 1, pp. 66ff., 266-72; Goitein, 1969, pp. 93-95; Goitein, 1967-1983, vol. 2; Lewis, 1937; Lapidus, 1967, 1969); and it was true even for Iraq, whose cities were the seats of those successive empires of increasing magnitude and ever more tightly controlled bureaucratic apparatuses: the Babylonian, Assyrian, Achaemenid, Parthian, and Sassanian (Morony, 1984).

In a parallel kind of argument, informal forms of association, craft, and profession, as well as confessional, familial, and fraternal forms, are frequently forced into the mold of the European guild—or deemed inferior if they do not fit. However, as Goitein categorically states, at least until the fourteenth century there was no such institution, because there was no need of it: "the classic Islamic society was a free enterprise society, the very opposite of a community organized in rigid guilds and tight professional cor-

porations" (Goitein, 1969, p. 94). Moslem corporations with characteristic rights and immunities abound, frequently adopting the ritualistic practices of the Sufi orders and brotherhoods. However, supervision of the work of artisans was not typically one of their functions, but was a duty placed "in the hands of the state police, which availed itself of the services of trustworthy and expert assistants" (Goitein, 1969, p. 94).⁹

Aristotle on the Contractual Nature of the Polis

Aristotle maintained, and Marx affirmed, that the polis better realizes the human *telos*, for man is by nature a political animal (*zoon politikon*), and this means simply that he is a city (*polis*) dweller. Only the proximity and multifaceted communal ties produced by contiguous existence permit the objectification of communal forms in everyday life, and, at the same time, the full flowering of the many-sided individual. Aristotle suggested, and recent scholars affirm, that the forms of association to which he refers—family (*oikia*), clans (*gene*), tribes (*phyle*), brotherhoods (*phratritai*), clubs (*hetaireiai*), and cultic organizations (*thysiai*)—were always political, and not primordial, as far back as the historical record goes.¹⁰ Although in the *Laws* (680D-E) Plato gives a genetic account of the emergence of political society out of the primordial—a collection of scattered households and patriarchal clans of which Marx's Germanic tribes are reminiscent—it is difficult to separate fact from myth. Aristotle's account lends support to the idea that, although political society was organized along apparently familial lines, the link of consanguinity between the clans and tribes, into which the city was organized for administrative purposes, was more fictional than real. Moreover, quite apart from the havoc to blood lines wrought by migration, on which both Marx and Aristotle comment, the deliberate policy of reformers like Solon and Cleisthenes had been to "mix it all up" by

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encouraging immigration, by the enfranchisement of aliens, by realigning the class structure to reward the upwardly mobile, and by reconstituting the tribal structure on a regional basis to destroy the power base of some families and strengthen that of others, in particular the urban tribes (Aristotle, *Politics* 6.2.11.1319b20-5, Loeb trans., p. 507).

Thus any simple association of *Gemeinschaft*, as the ideal type of organic community, with primordial society, where property was exclusively communal, and of *Gesellschaft* with urban, contractual, exchange-based society falls wide of the mark. Marx, following Aristotle, located the most highly developed form of the community in the *polis*, which was geared not merely to subsistence, but to "living well." His theory of exchange borrows explicitly from Aristotle's, and it is only in his last works, the so-called *Ethnological Notebooks*, that Marx, under the influence of Morgan, changes his mind, leaning toward a primitivist account of the *Gemeinschaft* taken up by Thomson, Godelier, Terray, and others today.

In Tönnies' version of the *Gemeinschaft-Gesellschaft* theory the use-value/exchange-value distinction was a crucial factor in distinguishing between the two forms. Invoking Hegelian, and subsequently Marxist, concepts of labour as a medium for the objectification of the human subject, he saw in the *Gemeinschaft* the appropriate social setting for this working out of human powers in the external world of objects. "Where such activity [labour] serves to maintain, further, or give joy to a *Gemeinschaft*, as is the case in natural and original relationships, it can be conceived of as a function of this *Gemeinschaft*," he declares (Tönnies, 1955, p. 92). By contrast, the *Gesellschaft* is geared to commerce, and "commerce, as the skill to make profits, is the opposite of all such art" (Tönnies, 1955, p. 92).

Much has been made by economic historians and classicists of Aristotle's distinction between *oikonomia* and

chrematistike as forerunners of the distinctions between use-value and exchange-value in Marx, and some have even given these concepts in Aristotle a reading that would suggest an early form of the *Gemeinschaft-Gesellschaft* distinction. We may summarize Aristotle's arguments briefly in order to make two different points: (1) that Aristotle's recommendations concerning economic activity have been misinterpreted as statements of fact, leading to a serious underestimate of the significance of commerce and trade in antiquity; and (2) that the application of the *Gemeinschaft-Gesellschaft* distinction to the *polis* fails on all counts. The tendency to characterize the *polis* as the *Gemeinschaft* takes no account of Aristotle's explicit argument that all communities rest on exchange—"if there were no exchange there would be no association (*koinonia*)" (*Nicomachean Ethics* 5.5.14.1133b17-18, Loeb ed., p. 287)—or of the highly contractual nature of social relationships in the ancient world, where membership in subordinate kinship and status groups (tribes, phratries, *gene*, *orgeone*)—not to speak of marriage, friendship, citizenship, adoption, and even tribal affiliation—was a closely circumscribed and explicitly regulated contractual arrangement sealed by ceremony and ritual. However, those theorists, including Marx in his last writings when he came under Morgan's influence, who see pre-*polis*, primordial society as the *Gemeinschaft* miss a different mark. Their application of the distinction takes no account of the degree to which so-called primordial forms were in fact political, assuming as organic, forms of social relationship that in recorded history have borne a tenuous relation to real bloodlines or systems of consanguinity with a genetic basis.

The highly contractual nature of relations in the *polis* could not be better demonstrated than by the fine distinctions Aristotle makes between the different forms of transaction and the principles of justice (equality or proportionality, as

they are translated into legal/juridical terms) appropriate to them. They include the private contracts between parties termed "voluntary transactions," but also include relationships between "friends" in general, friendship being understood as a contractual relationship with finely graduated scales of benefits and duties, depending on the degree of the relationship. There is, indeed, an analogue between the exchange relationship and that between private citizens that emerges when disputes arise. When disagreements arise between friends, or when parties turn to a judge for rectification of damages that arise from a breach of contract, essentially the same process is involved. What has to be achieved is agreement on compensation for gain or loss that is acceptable to both parties who construe their needs from diametrically opposed positions. The result, as in the act of exchange itself, is to achieve the impossible: an essentially asymmetrical relationship is converted into a symmetrical one by an exchange in value in which the gain and loss are equalized. Aristotle in fact admits that "the terms 'loss' and 'gain' in these cases are borrowed from the operations of voluntary [economic] exchange . . . as for instance in buying and selling, and all other transactions sanctioned by law" (*Nicomachean Ethics* 5.4.13.1132b10-15, p. 279).

Aristotle on Friendships and Partnerships

Not only is Aristotle's concept of justice specifically tailored to the need for fair contracts and equity in the distribution of goods and services that an exchange-based economy produces, but even his concept of "friendship" presupposes contractual obligations that an economy based on informal association demanded. Indeed, the point of Aristotle's typology of forms of social relationship in the

Politics—husband/wife, father/child, master/slave, householder/household—is not so much to emphasize their inequality (a fact taken for granted in the ancient world) as it is to show that the *polis*, as "the larger community," is an aggregate of subordinate forms of association, qualitatively different and irreducible, but each of which describes a schedule of goods and benefits shared and received.

In the two chapters that he devotes to friendship in the *Nicomachean Ethics* (a more extensive treatment even than that given justice), he deals systematically with the affinal groups out of which the city is constituted. "All friendship involves community or partnership (*koinonia*)," he says (*Nicomachean Ethics* 8.9.1.1159b25-30, and 8.2.1.1161b 10-15), but some relationships are definitely more contractual than others. Relationships between the members of a family, and of comradeships (*hetaireiai*) and brotherhoods (*phratriciai*), he says, are "less in the nature of partnerships than are the relationships between fellow citizens (*politikai*), fellow tribesmen (*phyletikai*), shipmates, and the like; since these seem to be founded as it were on a definite compact" (*Nicomachean Ethics* 8.12.2.1161b 10-15, pp. 497-99).

All forms of friendship are partnerships or associations for the exchange of benefits, Aristotle maintains, referring under the umbrella of friendship to the whole array of diverse associations in antiquity: tribes (*phyletai*), demes (*demotai*), cultic associations (*gennetai*), religious guilds (*thiasotai*), dining clubs (*synousias*) (*Nicomachean Ethics* 8.9.5. 1160a15-25). Since it is bound to be the case in some of these associations that one or other of the parties feels it has not gotten what it contracted for, the problem then arises of rectifying the damages in a case where gain and loss are assessed from the different points of view of the two parties. Clearly Aristotle does not mean by friendship a primarily emotional rela-

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tionship. These are forms of association entered on explicitly contractual terms with procedures for arbitrating disputes in which one or other party feels that the conditions of the friendship were not met.

Modern classicists such as Adkins (1960, 1963), Long (1970), Finley (1954), Ehrenberg (1943), and others have emphasized the extent to which concepts of friendship in antiquity describe quasi-juridical relations between individuals, and only secondarily as a consequence of mutual interaction, bonds of affection. They point out that in the Homeric period, and to some extent in the classical, human beings as such had only those rights that derive from relations of birth, marriage, *philotes*, etc. Outside kinship relationships all relations of mutual reliance fell under the category of "friendship."

In the absence of a strong state to guarantee personal security, such networks were essential to the autonomy and self-sufficiency of the family (*oikos*) and its members. Adkins (1963) points out, for instance, that despite an apparent distinction between friendships based on utility and those based on virtue, all three forms of friendship described by Aristotle—the hedonic, the utilitarian, and the virtuous—are in fact pragmatic. Friendship for another *agathon* (noble), on account of virtue (*arete*), and friendship for a comrade (*hetairos*) or equal, were as necessary as any other form of friendship for self-sufficiency and survival. Thus the conventional definition of friendship that we find in Plato of "helping one's friends and harming one's enemies" (*Crito* 45c5ff., *Meno* 41e2ff., *Republic* 332D) takes on significance as a formula for one's duty, or protecting one's interests and preserving the in-group against the out-group (Adkins, 1960, pp. 228ff.).

Philologically the verb *philein*, to love, the noun *philotes*, friend, and the adjective *philos*, are interesting: *philos* being translated as "dear," "beloved," in

the emotive sense, or "one's own," in the quasi-juridical sense. Thus, as Adkins points out, "the Homeric *agathos* (warrior-chieftain in charge of his own *oikos*) uses *philos* of his various faculties, possessions, and fellowmen, beginning as near as possible to the man himself and working steadily outwards" (Adkins, 1963, p. 30). So intimately is the term related to concepts of properties, possessions, and rights that it is used in Homer's *Iliad* and *Odyssey*, for instance, to refer to one's own (*philos*) limbs or one's own (*phila*) clothes or bed, respectively, where to translate the term *philos* in an emotive sense with "dear" or "beloved" would be inappropriate (Adkins, 1963, p. 30).

It has been shown, indeed, that the history of concepts of friendship and affinity in the ancient world took the form of the gradual extension of concepts of kinship (*oikeiotes*) to ever larger affinal groups (Baldry, 1965, pp. 42, 142-3, 178). From *oikos*, kinship construed in its narrowest terms, was derived more abstract concepts of likeness (*oikeiosis*) that, by virtue of a Stoic preoccupation with a common human nature transcending natural boundaries, gave rise eventually to the conception of the inhabited world as the *oikoumene*, the family of man with its natural divisions of family, clan, and tribe.

The intimate connections between kinship, ownership, and love in antiquity are very likely the basis for Aristotle's bracketing of the twin sentiments "this is my own" (property) and "this I love" (affinity) in his critique of Plato's communism in the *Republic* (*Politics* 2.1.8-12.1261b15-1262a25). Aristotle points to the extreme unity of the *Republic*, which ignores the plethora of relationships based on affinity, consanguinity, and marriage that hold the city together. What would it mean under communism to call a boy "my son" when he may be shared by any number of people from two to ten thousand? Better he were "one's private

nephew than one's son in the way described" (*Politics* 2.1.12.1262a10-15). Parenthood creates a special "belonging," and Aristotle leaves no doubt that his major objection to Plato's reconstitution of social and political forms in the *Republic* is Plato's substitution of an abstract and arithmetical relation of equality and solidarity under communism for the specific and historically developed forms that have a real life in the everyday Greek world. Creating community (*koinonia*) does not come from watering down subordinate groups, but rather from creating "a partnership out of a multitude through education (*paideia* *koine*)" (*Politics* 2.2.10.1263b35-40, p. 91). "The state is a partnership of families, living well in their clans, brotherhoods, clubs for worship and recreation" (*Politics* 3.5.14.1280b35-1281a5). And the reason why all these subordinate social forms cannot be collapsed to a unity in communism is that community is built on social solidarity or "friendliness," as Aristotle put it simply, and the experience of social solidarity, like most virtues, can only be learned by doing it (*Nicomachean Ethics* 3.5.21.1114b25-30, Loeb trans., p. 153). Participation in the plethora of small scale social groups, familial, local, cultic, and religious, that made up everyday life in ancient Greece (as they continue to do in Mediterranean society today) provided that experience. Political society is not antithetical to the clan-based, cultic community, but is a continuation of it.

Aristotle's dissertation on friendship in books eight and nine of the *Nicomachean Ethics*, treating the various forms of friendship, the expectations attached to them, their relative durability, and how to arbitrate in cases of breakdown where expectations have not been fulfilled, is testimony to the degree to which the *polis* as a community is based on a network of little societies whose bonds are affective but whose status is quasi-judicial. The

highly contractual nature of friendship and the texture of Aristotle's arguments have been missed by those commentators from the analytic school of philosophy whose ear is not attuned to historical and cultural nuance. They have also been overlooked by proponents of the *Gemeinschaft-Gesellschaft* distinction who have seen in primordial forms of kinship and friendship, relations based on "love" antithetical to the contractual relations of modern society. In general, the relation that Aristotle's analysis of friendship and the bonding virtues of love, magnanimity, honour, and justice in the *Nicomachean Ethics* (bks. 4-5, 8-9) bears to social groups in their legally constituted form in the *Politics* (bks. 1-3) has been underestimated.

Homo Politicus and Homo Oeconomicus

The *Gemeinschaft-Gesellschaft* distinction needs at the very least to be revised. The more appropriate distinction, hinted at by Marx, is between properly political society—small-scale, urban, highly participatory and entrepreneurial, confined in Europe now, as in ancient times, to the Mediterranean basin—and northern European society, whose ancestry is feudal, rural, and decentralized. In these latter systems (excluding northern Italian city states and the free cities of the Hansa League) participation in free and equal institutions is only a recent part of history, and absolutism, where the only public person was the king, was once the dominant mode.

The political community of antiquity was an urban phenomenon based on a coastal entrepreneurial economy generating considerable wealth, whose life-blood was an elaborate network of forms of association—familial, religious, regional, occupational, recreational, and class. The success of fifth-century democracy was in many respects a function of the habit of

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associating together in a multitude of little societies. Polis society is characterized by the fact that kinship and tribe have become modified constructs tailored to the religious calendar and rotation of offices of the city (Veyne, 1976; Connor, 1971; Davies, 1981). Religious cults and the monopoly of exchange for public purposes through a system of liturgies by which private citizens underwrote the expenses of financing the fleet, the army, choral and dramatic performances, games, athletic contests, and festivals, put a brake on private accumulation in the polis. Concepts of friendship, community, obligation, and justice have a high degree of specificity and describe a way of life (*politeia*), and not abstract values or duty for duty's sake. However, as against Tönnies, the relationships between members of these fellowships (*koinonia*) are not simply organic, based on love, but are properly contractual—sealed by ritual compacts, blood covenants, etc. For this reason it is possible to distinguish different degrees of relationship which, although based on the model of different degrees of consanguinity, are not necessarily based on blood relationships at all.

The mix of public and private in these relationships parallels that of family and friend. Thus demes, phratries, and so-called clans (*gene*) were semi-official organizations, but under the control of private resources. The highly influential male clubs—*hetaireiai* (political), *symposia* (drinking), and *gymnasias* (wrestling)—had equality, if not precedence, over relations through marriage that would unduly elevate the status of women, and were in many respects public in function in a society based more on "fraternal" than patriarchal social relations.¹¹ Only the *oikos* itself was defined as the exclusively private sphere. Aristotle's account of the social constitution of the polis (*Politics* bks. 1–2) is consistent with this analysis. For Aristotle saw the

city as the aggregate forms of partnership or association (*koinonia*), some of which were based on real or fictional concepts of consanguinity, others being based on shared religious, ethnic, or cultic background. But the *koinonia politike* is a whole greater than the sum of its parts, so that to rule over "a large household and a small city" is not the same thing, but involves a difference in kind (*Politics* 1.1.2.1252a10–15, p. 3). "The partnership that comes about in the course of nature for everyday purposes, is the *oikos*," Aristotle observes, and as the extended rather than small family it includes "food-sharers," "hearthsharers," and "sharers of the mother's milk" (*homogalaktes*), none of which according to modern authorities are necessarily blood relations. "The primary partnership made up of several households for the satisfaction of . . . needs is the village" (*Politics* 1.1.6–7. 1252b10–20, p. 7), while "that partnership which is composed of several villages is the city-state," which has "at last attained the virtual self-sufficiency" which fits it for the good life (*Politics* 1.1.8.1252b10–35, pp. 7–9).

Aristotle refuses any distinction between natural, primordial forms of association and the city as a product of art or convention: "Hence every city-state exists by nature, inasmuch as the first partnerships so exist," he maintains, "for the city-state is the end of the other partnerships, and nature is an end." It is from this point of view that "the city-state is a natural growth and that man is by nature a political animal." As a consequence, "a man that is by nature and not merely by fortune citiless (*apolis*), is either low in the scale of humanity or above it (like the 'clanless [*aphretor*], lawless [*athemistos*], hearthless [*anestios*] man reviled by Homer)," for ". . . man is a political animal (*politikon ho anthropos zoon*) in a greater manner than any bee or gregarious animal" (*Politics* 1.1.8–9. 1252b30–1253a10, pp. 9–11). (We are

reminded of Marx's several asides on the Aristotelian definition *zoon politikon* properly referring to man as a city or polis dweller, as well as his famous distinction between the architect and the bee, the former a social being whose production represents the needs and purposes of a living community, the latter a creature whose gregariousness and productivity are instinctively driven.) Aristotle's various references to subordinate forms of association make it plain that he conceives these now to be units of urban administration, whatever their original function may have been. So when he speaks of voting "by tribes (*phylas*), or demes (*demous*), or brotherhoods (*phratrias*)" (*Politics* 4.12.11.1309a10-15, p. 363), these are clearly alternative juridical units. When he recommends the auditing of public funds be carried out by "brotherhoods (*phratrias*), companies (*lochous*) and tribes (*phylas*)" (*Politics* 5.7.11.1309a10-15, p. 429), he is referring to the administrative function of these forms of association.

Certain fertile remarks in the *Grundrisse* and *Capital* suggest in fact that Marx might have formed a comprehensive theory of ancient society much closer to Aristotle's—and one in which the *Gemeinschaft-Gesellschaft* distinction would not have figured—had they been developed. For instance, Marx notes both that "the history of classical antiquity is the history of cities," and that "there was in antiquity no more general institution than that of kin groups" (1973, pp. 478-9). His references to *zoon politikon* emphasize not only that man is a town dweller by nature, but also that politics as an urban phenomenon is, as Aristotle insisted, characteristically "rule of the free." By contrast, the society of the Germanic tribes, typifying Northern European society from which the capitalist nation-states ultimately developed, is rural, decentralized, and tribal in a different sense:

The Middle Ages (Germanic period) begins with the land as the seat of history, whose further development then moves forward in the contradiction between town and countryside; the modern age is the urbanization of the countryside, not the ruralization of the city as in antiquity. (Marx, 1973, p. 479)

Marx is hinting, perhaps, that the primordial/political distinction is a regional rather than a chronological one, and that while "political" societies (literally *poleis*) of the Mediterranean basin have never changed essentially in character, northern European society is an extension of primordialism which grew out of Franco-Germanic tribal society and its feudal legacy.

The *Gemeinschaft-Gesellschaft* distinction postulates an original primordial community ruled by family, clan, and tribe, out of which political society emerges. This proposition has been seriously called into question by modern classical scholars, who show that concepts of *genos* and clan are reconstructions of nineteenth-century scholars drawing on contemporary anthropological conceptions of tribalism in Africa, Melanesia, and Australia that are inappropriate to urban Mediterranean society in antiquity.¹² Marx fell under the influence of these nineteenth-century scholars—Morgan, Maine, Fustel de Coulanges, etc.—in his last years. So while his account of the polis in relation to other communal forms in the *Grundrisse* is much closer to the appraisal of Aristotle and those modern scholars who have turned back to his account, Marx's position in the *Ethnological Notebooks* represents an unfortunate break with this tradition. We have in this, his last work, evidence that he became an adherent of the *Gemeinschaft-Gesellschaft* myth, believing with Morgan that the ancient *gentes* (clans) provided the original model of the democratic and fraternal *Gemeinschaft* that constituted the polis (Krader, 1974, p. 3).¹³

Political society, as we have used that

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term—i.e., polis based, urban, entrepreneurial, highly participatory, and in some cases even democratic—was typified by contractual relations of the most highly specified sort. It is significant, for instance, that the earliest Greek papyrus that we possess is a marriage contract of 311 B.C. from Assuan in Upper Egypt. We also have Aristotle's authority for the homology between social relations of the polis and partners to a business transaction. Even forms of friendship were conceived of contractually, concerned with the mutual exchange of goods and services, as Aristotle attests. The city-state in antiquity was itself more in the order of a voluntary association, and not coextensive with the inhabitants of a territory. One contracted in and out of the polis insofar as qualification for citizenship and formal admission by ratification of the phratry permitted; departure was marked by ostracism, voluntary exile, or death. As such, the polis constituted an aggregate of subordinate forms of association entered into by way of initiation rites, pacts, seals of blood covenant, etc., which enjoyed secret rites, private festivities, and legal immunity. As orientalist and classicists now concur, kinship or tribal organizations were, in the last analysis, voluntary forms of association rather than primordial networks. Recent work on tribalism in both classical antiquity and the modern Middle East suggests that tribe and clan are socially constructed entities that bear a tenuous relation to real blood lines, while trading off the moral resources of family and other forms of in-group loyalties.¹⁴ Thus the designation of the tribe, named after a heroized ancestor, a mythical historical homeland, or an ancient totemistic sign, usually represents at the same time territorial claims, ethnic identification, and status, financial, or other aspirations on the part of the living association. In a relatively densely populated, commercially oriented city-state (as most Mediter-

anean and Middle Eastern societies have been from time immemorial) economic and social viability depend on a considerable volume of successful transactions of all kinds that is less significant in a decentralized agrarian society lacking the features of contiguous and mutually dependent existence. Oaths, pledges, promises, pacts, covenants, and contracts in a plethora of formulations and with all sorts of sanctions seem to have provided the guarantees on which such transactions depended, as witnessed by such documentary evidence as the Oxyrhynchus Papyri, oriental inscriptions, the Hammurabi code, etc. (Springborg, 1985).

The conclusion of this argument, then, is that *homo politicus* and *homo oeconomicus* represent not two phases in a process of social evolution, but rather alternative and contemporaneous ways of life with distinctive structures and institutions, differentiated regionally rather than chronologically. Moreover, these two regions have shown an amazing degree of continuity in their historical form. For instance, the sectarian warfare, institutionalized in the cultic, clan, and ethnic forms of association of modern Lebanon is reminiscent of the ancient skirmishing of Phoenicia, its historical parent state, of the agonistic struggles of ancient Greece, and of the civil wars of Rome, all of which Lebanon experienced historically more or less directly (Khuri, 1976). The ancient class, clan, and cultic forms of organization, the noble clubs, phratries, hetairies, and gymnasia, have their modern counterparts in the brotherhoods, professional syndicates, clans, and the religious and sporting clubs of modern Mediterranean society.¹⁵ Where family and clan networks have proved to be ineffective they have been replaced by functional substitutes—for instance, reconstructed village associations that hark back to an original homeland, several generations and sometimes even countries removed; and associations formed by membership

in the same graduating class of a high school, university, or military college (Gellner and Waterbury, 1977; Springborg, 1975, 1979, 1982; Khuri, 1975, 1980).

The renowned features of Mediterranean society as a shame culture hark back to a code of honour and protection of the in-group based on affinal networks against the out-group that have their counterpart in antiquity (Péristiany, ed., 1965, 1968, 1976). There too the *agon*, or contest for honour, was a constitutive feature. Performing the same function was an elaborate set of distinctions defining relations of social proximity, from immediate to extended family relations, friendship, and guest friendship, to the stranger, and backed up by private militias and blood feuds.

It has been said of ancient society that whereas modern life may be analyzed in terms of its public versus private dimensions, antiquity requires a tripartite division: public, private, and cultic. The elements which, when assembled, constituted the foundations of political society—family, clan, tribe, and village forms of subordinate association—were all religiously defined. This means that they were each associated with cultic rites and practices which created a differentiated set of common bonds for different purposes.

The substantive content of these cults was, curiously enough, provided largely from the Near East. There is a continuity of mystery and miracle cults, magic practices, forms of table worship, omen-taking, astrological lore, cultic incantations, and initiation rites that go back in some cases to Pharaonic and Babylonian practices of the third millennium B.C., and which were taken over and faithfully incorporated into their own religious practices by successive Semitic, Greek, and Roman invaders (Hooke, 1953). We have in the twentieth century seen investigations of some of the relics of these

cults and practices, including the Coptic Gnostic gospels (Robinson, 1977; Pagels, 1979), the rituals of the Gnostic anti-Christian Mandaeans of the marshlands of modern Iraq (Rudolf, 1978), and the ancient secret rites of the Druzes, influenced as they were by neo-Pythagorean practices, as a direct link between ancient pagan oriental cults and their Hellenistic transmission (Makarem, 1974).

Because of the way in which the cultic dimension of social life influenced political and economic developments, this religious continuity in the Graeco-oriental world is of much more general significance. One of the distinctive features of ancient capitalism, as Weber observed, was the role of the notable in underwriting public works through liturgies (the funding of the army, the navy, and games and contests in ancient Greece), and the foundation of religious institutions (temples and mosques, funeral associations, and shrines for saints or martyrs, or *Waqfs*, as they are known in the Islamic world) (Veyne, 1976, pp. 232–79; Khuri, 1980, ch. 8). Because the urban, commercial, entrepreneurial society of the Mediterranean basin supported a wealthy class of notables more or less throughout its history who served local kings and emperors and foreign occupiers as municipal functionaries, and who underwrote public works at their own expense as the *quid pro quo* for social prestige, there was no need, and no room, for modern northern European post-Industrial Revolution capitalism to take root. The relatively small-scale enterprises of a relatively large class of notables shut it out. For this and other historical reasons, “modernization” and “development,” where they have occurred, could only be undertaken by the state or the revolutionary party, because only they had the political and military muscle to override the indigenous commercial establishment. Even so, indigenous social forms have shown a characteristic ingenu-

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ity and durability in being able to honeycomb so-called "modern" imported institutions and structures with traditional affinal networks (Khuri, 1975; Moore, 1977; Springborg, 1975, 1979, 1982). It is the very durability of these ancient political forms, in the Middle East in general and in Lebanon in particular, that has presented to Western powers of a somewhat different historical experience some of their greatest challenges in the field of foreign policy.

Notes

I wish to thank Carole Pateman, Henry Mayer, Michael Jackson, Gian Poggi, John Burnheim, Gyorgy Markus and Maria Shevtsova, my colleagues, and Robert Springborg, my husband, for their comments on this paper and for discussions from which I have learned much. I owe a special debt of gratitude to John Pocock, whose comments ran to a small essay, Leszek Kolakowski, Preston King, Philip Rieff and Clement Henry, whose generosity in reading papers and offering scholarly advice on this and other occasions is greatly appreciated. Thanks are also due to Gabriel Almond, David Kopf, Philip Pettit, and Oliver Williams. My interest in Mesopotamian culture was rekindled during my time at the University of Pennsylvania, which financed many of the great excavations, including those of William Flinders Petrie, and which houses a magnificent collection of cuneiform texts, including copies of the Epic of Gilgamesh.

1. The same is true, of course, for Soviet policy makers assisting third world countries that fall within their orbit to make the transition to socialism as the third phase in the Marxist version of the ubiquitous three-phase macrohistorical schema. Marx was as convinced an historical evolutionist as any of his nineteenth-century contemporary social scientists, and orthodox communism has promoted "modernization" by way of industrialization as ardently as the West.

2. Max Weber's account of the distinctiveness of modern industrial society, the locus of *homo oeconomicus*, compared with the society of *homo politicus*, the ancient polis, is still the most convincing and is remarkably unbiased by nineteenth-century romanticism which characterizes the distinctions of Morgan, Marx, and Tönnies. Observing that "whereas in Antiquity the hoplite army and its training, and thus military interests, increasingly came to constitute the pivot of all urban organization," Weber notes that "in the Middle Ages most burgher privileges began with the limitation of the burgher's military duties to garrison service." As a

consequence, "the economic interests of the medieval townsmen lay in peaceful gain through commerce and the trades, and this was most pronouncedly so for the lower strata of the urban citizenry." Thus, he concludes, "The political situation of the medieval townsman determined his path which was that of a *homo oeconomicus*, whereas in Antiquity the polis preserved during its heyday its character as the technically most advanced military association: the ancient townsman was a *homo politicus*" (Weber, 1968, vol. 3, chap. 16, pp. 1353-4). Weber's theory, and indeed the bulk of his earlier writings, constitute an interesting commentary on Marx's epigrammatic observation that "the mode in which they gained a livelihood" explains why "for Athens and Rome . . . politics reigned supreme," but "not for the Middle Ages," in which "Catholicism played the chief part" (Marx, n.d., vol. 1, p. 86n).

3. Henry Sumner Maine (1861), dates the status to contract shift to the transition from tribal to agricultural society; Tönnies (1955, p. 274) to the transition from Graeco-Roman to Northern European cultural dominance; Marx, Weber, and modern sociologists to the transition from feudal to nation state in Europe (on the distinctions in Marx, Morgan, and Maine see Krader [1974, pp. 21, 36, 49ff.]). The classicist Sir Moses Finley (1955, p. 194), however, dates it to the transition from Homeric to classical polis society in Greece, seeing in the shifting marriage patterns of the Homeric world "merely one element in a much more general transition from an oikos-kinship world of status relations to the polis world of transactions consummated under the rule of law."

4. On the library of Ashurbanipal in Nineveh see Hooke (1953), Pritchard (1950), and Thompson (1928); on the Tell el Amarna letters, Petrie (1894), Mercer (1939), and Campbell (1964); on the Oxyrhynchus Papyri, see Grenfell and Hunt (1848-1967), and the two volumes of selected papyri in the Loeb Classical Library (Hunt and Edgar, 1932-34) which contain legal and other non-literary texts for the Hellenistic period; the first volume comprising private, the second, public documents. The Hammurabi Code—which is in fact less in the nature of a code than a set of amendments to third millennium Babylonian common law which the king promulgated in his own name—has now received definitive translation and legal commentary by Driver and Miles (1952-55), the latter of whom is a jurist. On the gnostic gospels see Robinson (1977) and Pagels (1979); on the Geniza documents see Goitein (1967-1983); on the Arabic texts of otherwise lost classical Greek and Hellenistic works of philosophy, science, medicine, etc., see Walzer (1962) and King (1978, 1980); on Sippar see Oppenheim (1969); finally, on the Ebla tablets, see Bermant and Weitzman (1979), and Matthiae (1980).

5. This remarkable chapter in the history of juris-

prudence is documented in the neglected study of Gierke, *Das Deutsche Genossenschaftsrecht* (Gierke, 1900, 1934). Arguing that "on the whole, the theorists of Natural Law were driven by the tendency of their time to deny that local communities and corporate bodies [i.e., the *collegia* and *universitates*] had a social existence of their own" as autonomous voluntary societies, Gierke concludes that "so far as it moved in this direction, the natural-law theory of associations was far less favourable to such bodies, on the fundamental issue at stake, than was the positive-law theory of Corporations" expounded by theorists of Roman civil law. Gierke saw in the social contract theories of Hobbes, Locke, and Bodin the logical outcome of this process by virtue of which "the sovereign State was directly based on the conclusion of a [fictitious] contract between individuals. . . . The Family and the State were therefore regarded as the only societies which possessed a basis in Natural Law," the family—*societas privata*, or *domestica*—being subdivided into the "three societies of husband and wife, parents and children, and master and slaves," over which these theorists "hastened to set . . . at once the *societas politica* or *civilis*, a community armed with sovereign power. . . . [Thus] local communities and corporate bodies, as distinct from the Family, were regarded as only arising after the constitution of a system of political order" (Gierke, 1934, pp. 62–3). Gierke argues that this interpretation of the relation of subordinate forms of association to the state was based in part on a reading of the development of the classical *polis*, and probably Aristotle thereon, as a "progressive widening of the original family-community, first into the local community, then into the city, and finally into the greater kingdom . . . which made the local community merely a preliminary stage of the civic community, and treated the civic community as the perfect realisation of the idea of State" (Gierke, 1934, p. 63). A reading that it is my purpose in this paper to correct. On Hobbes and corporation theory see Springborg (1976). On the cultic *collegia* and *universitates* see Cumont (1956), Nock (1935, 1936, 1944), and Nilsson (1972). On the king as "corporation sole" see Maitland (1936, 1900), the latter comprising excellent essays on corporation theory and its ramifications in early modern thought, and also Kantorowicz (1957). On some peculiarities of social contract theory as it developed along these lines, see Gauthier (1977) and Pateman (1984a).

6. References to Marx's *Grundrisse* list first the page numbers of the German edition (1953) and second of the English translation (1973) unless otherwise noted. On the formative influence of the classics on Marx see Ste. Croix (1983, pp. 19–85) and Springborg (1984a, 1984b, 1984c).

7. Max Weber also considered the corporate nature of communal life to be the defining characteristic of the city as such. But he believed this com-

munity to be a feature of Western rather than Eastern cities. "Not every 'city' in the economic sense, nor every garrison whose inhabitants had a special status in the political-administrative sense, has in the past constituted a 'commune' (*Gemeinde*)," he maintained. "The city-commune in the full meaning of the word appeared as a mass phenomenon only in the Occident; the Near East (Syria, Phoenicia, and perhaps Mesopotamia) also knew it, but only as a temporary structure" (Weber, 1968, vol. 3, chap. 16, p. 1227). Weber goes on to argue, rather arbitrarily, that in the oriental city—in antiquity, and in particular under the great territorial kingdoms—"the associational character of the city and the concept of a *burgher* (as contrasted to the man from the countryside) never developed at all or existed only in rudiments." The "special status of the town dweller as a 'citizen,' in the ancient medieval sense, did not exist, and a corporate character of the city was unknown" (Weber, 1968, vol. 3, pp. 1227–8). He appears to make exceptions of those cities of Mesopotamia, Syria, and Phoenicia that grew up along the caravan route, which saw the "rising power of the patrician families in the 'city hall,'" for which he cites the Tell el Amarna tablets (Weber, 1968, vol. 3, p. 1230). But, because of its clan character, he completely misunderstood the character of Mecca, one of the most economically diverse and sophisticated oriental cities, observing that "all safely founded information about Asian and Oriental settlements which had the economic characteristics of 'cities' seems to indicate that normally only the clan associations, and sometimes also the occupational associations, were the vehicles of organized action (*Verbandshandeln*), but never the collective or urban citizens as such" (Weber, 1968, vol. 3, p. 1233).

8. Citations from classical authors, Plato, Aristotle and Herodotus, follow the standard form with page references to translations where they are not my own.

9. In this respect Islamic cities far exceeded the levels of economic development achieved by Greece and Rome. Goitein observes that the Cairo Geniza documents list "about 265 manual occupations . . . 90 types of persons engaged in banking and commerce and approximately the same number of professionals, officials, religious functionaries and educators" (Goitein, 1967–1983, vol. 1, p. 99). This total of around 450 professions for medieval Cairo, may be compared with the 150 professional corporations documented for ancient Rome, the 278 occupational corporations listed for Cairo in 1801, and the lexicon of some 435 recognized occupations compiled by the Qasimis, father and son, for Damascus beginning in 1890 and published only in 1960 (Goitein, 1967–1983, vol. 1, p. 99). It should not be forgotten that the Middle Eastern city has always been an urban conglomerate on a scale that the Greek *poleis* or the European medieval *metropoleis*

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could hardly match, with extensive needs, therefore, for food and consumer durables. For instance, Babylon in the first millennium B.C. covered some 2,500 acres; Nineveh some 1,850; Uruk, 1,100; Calah, 800; while Athens at her height under Themistocles covered only 550 acres (Oppenheim, 1969, p. 5). The population of the Hellenistic cities, Alexandria and Constantinople, was in their prime around 200,000 souls, while that of Antioch is put at 100,000. For the Islamic period, the population of tenth century Baghdad is put at anywhere between 500,000 and 1,500,000, while Cairo in the fourteenth century was reported by a French traveller to be "twice as large as Paris [with] four times the population": now reckoned to have been 300,000 as against Paris's 90,000. In the modern period around 1800 the Syrian cities of Aleppo and Damascus had populations of around 200,000 and 100,000, respectively; Hama, Homs, Jerusalem, and Tripoli, 10,000 or more; while the total population of Syria was below 1,500,000. At the same time Baghdad was a city of around 100,000, the other Iraqi cities Mosul, Hilla, and Basra were all around 50,000; in a total population about the size of Syria's: Turkey in 1800, with a total population of over 10,000,000, boasted Istanbul at around 1,000,000, and Isfahan around 300,000. For a comparison the population of London in 1800 was 400,000 (Issawi, 1969, pp. 102-105).

10. Roussel (1976) and Bourriot (1976) have been hailed by classicists as having achieved a breakthrough in the study of ancient social structures, calling into question standard accounts of the transition from tribal to political society (see Gauthier, 1978; Fisher, 1979, 1981). English language scholars who anticipated the work of Bourriot and Roussel include Ferguson (1910, p. 283), who argued that the so-called clans (*gene*) were "ideal, not real, family groups" of the privileged, designed to look after the common interests of their members. In his specific study of the *genos* Salaminioi (1938, p. 42ff.), Ferguson showed this famous "family" to be a fictitious clan created to lay territorial claim to Salamis. Ferguson (1936, 1944) and Nock (1944), further emphasize the ceremonial and religious function of "familial" organizations.

Other significant works on family clan and tribe in antiquity are Wade-Gery (1931, pp. 131ff.), who argues that the phratry was an "ecclesiastical parish" based on religious cult and descent, the deme a "secular parish" based on locality and domicile; Costello (1938), who defends Aristotle on tribe, phratry, and *gene*, although seeing totemic origins for the clan; Pusey (1940); Andrewes (1961a, 1961b); Forrest (1960); Hammond (1961); and Davies (1977-78), on the phratry as a mechanism to ratify citizenship.

11. See Arthur (1973) and Saxonhouse (1980), both of whom conclude that women were better off under oikos-centered Homeric society than they were under the male polis. For a very interesting

treatment of fraternal political relations see Pateman (1984b). Humphreys in particular observes:

The subordinate segments of Athenian society, demes and phratries, though they borrowed all the forms of public procedure in the assembly, lacked its revenues and remained often under the sway of private resources and private interests. Private loyalties and, on occasion, conflicts were bound to be sharp in small patrilineally recruited groups. . . . Appeals to ties of kinship through marriage had no special status distinguishing them from appeals to ties of friendship. . . . For political alliance inside Athens it was not necessary to penetrate into the inmost private sector of the *oikos*, the women's quarters; the companionship of gymnasium and symposium were enough. . . . Within Athens, personal ties could be forged in the semi-private, semi-public masculine world of the symposium and gymnasium without recourse to marriage. The culture and traditions of the *hetaireiai* are more important than dynastic marriages. (1977-1978, pp. 98-101)

Costello (1938, p. 176) noted long ago that "the *hegairaia*, not the *genos*, was the characteristic organization of the aristocrats," whose fraternal relations were celebrated by Aristotle. Calhoun's work (1913), remains the best treatment of the *hetaireiai* and their manifold social functions.

12. An argument made explicitly by Roussel (1976, pp. 9ff.) with reference to Marx and Engels. Morgan, Maine, and Tönnies influenced a whole line of ancient historians and classicists, their legacy in the interpretation of the ancient *genos* having long outlived the credibility of the anthropological theories on which it is based. The elements of so-called tribal society which they isolate—divisions of the people into tribes and phratries, etc.—always in fact signified the constitution of a city (*polis*), as Max Weber himself observed (cited by Roussel, 1976, p. 5). Furthermore, the division of the citizens into tribes and phratries in Greece was almost always the work of a founder or reformer of the city.

13. That Marx underwent a change of heart between the *Grundrisse* and the *Ethnological Notebooks* is suggested by Hyndman, an English socialist with whom Marx had contact during 1880-1881, who noted in his autobiography: "Thus when Lewis Morgan proved to Marx's satisfaction that the gens and not the family was the social unit of the old tribal system and ancient society generally, Marx at once abandoned his previous opinions based upon Niebuhr and others and accepted Morgan's view" (quoted by Krader, 1974, p. 87, who makes nothing of this change of heart).

We have further evidence in the *Ethnological Notebooks* found in Marx's excerpts from Morgan on the Grecian gens, quoting George Grote (Marx, 1974, p. 201) to the effect that the *oikos*, extended

family, was the foundation of Greek society, and that members of the same *gens* were not commonly related at all. For although the natural families which constituted the *gens* traced their origins to an eponymous common ancestor, this was a "fabulous genealogy" and the *gens* was properly a cultic or religious form of association rather than a kinship unit. This view is supported by recent scholarship as well as by Niebuhr and Mommsen, Marx's recognized authorities in the *Grundrisse*, whom he now rejected on the authority of Morgan. In general, Marx finds little to disagree with in Morgan's account, making only one significant departure: he postulates antagonism within the *gens* due to the emergence of private property as a consequence of which "land, cattle and money capital had become of decisive importance with the development of slavery" (Marx, 1974, p. 213; Krader, 1974, p. 21). The contradiction between private and communal property, we know from the *Grundrisse*, was seen by Marx as the source of the decline of the ancient commune. Morgan also singled out the accumulation of private property and territory as the vehicle of transition from *societas* to *civitas*, but without postulating the internalization of conflict within the *gens*. Although Marx is considerably more skeptical of the work of Maine, he implicitly accepts the notion of the development of society as a transition from status to contract in the *Ethnological Notebooks*.

14. This is emphasized over and over again in Péristiany (1976). Emrys Peters (1976, p. 4) notes in his study of affinity in a Lebanese Maronite village that "affinity" is the "power to create groups"; Khuri, in his study of family associations in two suburbs of Beirut, notes the fictitious nature of eponymous clan organizations, "whose main function was to act internally, as a welfare and protection group and, externally, as a political entity, a receiver and dispenser of privileges":

A family association is created by the founder registering the association and seeking to receive into it the largest possible number of those bearing the same family name while validating their claim to common descent by the fabrication of spurious genealogies asserting, in one instance, common descent from Abraham. (1976, p. 5)

15. On the role of wrestling clubs in Iran (descendants of the ancient *gymnasia*), see Bill (1973); on the Turkish *hammam* or baths, frequently a gift of the notables to the city as a continuation of the tradition of the *gymnasium* under the control of the *gymnasiarch*, see Veyne (1976, p. 365, n. 313). Several of the *Oxyrhynchus Papyri* (especially nos. 1416 and 1418) deal with the *gymnasiarch* in Hellenistic Egypt (Veyne, 1976, p. 365, nn. 310, 314). The role of the Moslem Brotherhood, the Sufi orders, and the processional and funeral clubs as associational groups with dual religious and political functions

in the Middle East (the latter in many respects the descendants of the famous Hellenistic funerary foundations), is too well known to require documentation.

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Forthcoming in June

The following articles and research notes have been scheduled tentatively for publication in the June 1986 issue:

Richard Born. "Strategic Politicians and Unresponsive Voters"

Steven Forde. "Thucydides on the Cause of Athenian Imperialism"

Milton Lodge and Ruth Hamill. "A Partisan Schema for Political Information Processing"

Rasma Karklins. "Soviet Elections Revisited: Voter Abstention in Non-competitive Balloting"

Robert E. Lane. "Market Justice, Political Justice"

Arthur H. Miller, Martin P. Wattemberg, and Oksana Malanchuk. "Schematic Assessments of Presidential Candidates"

Edward N. Muller and Karl-Dieter Opp. "Rational Choice and Rebellious

Collective Action"

Charles W. Ostrom and Brian L. Job. "The President and the Political Use of Force"

Arlene W. Saxonhouse. "From Tragedy to Hierarchy and Back Again: Women in Greek Political Thought"

Brian D. Silver, Barbara A. Anderson, and Paul R. Abramson. "Who Overreports Voting?"

Rein Taagepera. "Reformulating the Cube Law for Proportional Representation Elections"

S. Sidney Ulmer. "Are Social Background Models Time-Bound?"

Stephen K. White. "Foucault's Challenge to Critical Theory"

Gerald C. Wright, Jr. and Michael B. Berkman. "Candidates and Policy in United States Senate Elections"

CONTROVERSIES

THE EFFECTS OF LEADERSHIP SUCCESSION IN THE SOVIET UNION

What happens when there is a change in leadership in the Soviet Union? Does the period of leadership succession present unusual opportunities for domestic policy concerns to be vented? Or, are major issues on the Soviet domestic policy agenda addressed mainly after a succession of leadership is completed? Answers to such questions have a bearing on how the Soviet elite is replenished, and more generally help to improve understanding of Soviet politics. Two specialists on Soviet and East European politics, Valerie Bunce and Philip Roeder, have been drawn to different conclusions about these issues.

There is an ongoing debate among specialists in Soviet and East European politics about the impact of leadership succession on domestic policy priorities (see, for example, Breslauer, 1982a; Brown, 1982a; Bunce, 1980, 1981; Roeder, 1985). In my own work, I argue that the concerns of the public receive special attention during succession periods throughout the Soviet bloc (Bunce, 1980, 1981).¹ By contrast, Philip Roeder (1985) argues, for the Soviet case only, that such concerns rise on the elite agenda after succession has been resolved.

The purpose of this comment is to assess which argument best describes the Soviet case. This is not a trivial question. The precise relationship between "high politics" and "high economics" in the Soviet Union, as elsewhere, tells us a great deal not only about leadership succession, but also about the very nature of Soviet politics.

The logical place to begin the assess-

ment is with the assumptions we make about Soviet elite politics within the Soviet Union. Roeder makes two dubious assumptions.² The first is that the first secretary always champions "mass" interests, whereas the rest of the Politburo always champions elite interests. This assumption, however, has little empirical support. Khrushchev's major rival, Malenkov, promoted light industry, a position resonant with mass concerns. Moreover, Brezhnev's policy stances over the course of his rule could hardly be said to support "mass" interests (see Breslauer, 1982b). All this would seem to suggest that it would be better to assume, as I did, that first secretaries vary in their policy orientations, and that decisions during and after succession are the outcome not of first secretaries "against all," but rather of more complex interactions among principal players driven by concerns with both power and policy.

Roeder's second problematic assumption is that the policy process during suc-

cession periods is essentially an elite "free-for-all." In my own view, new first secretaries, while assuredly constrained, are nonetheless *primes inter pares*. The evidence for my assumption is much stronger. If we read in between the lines of *Pravda*, we find clear indications that new first secretaries, while constrained, do dominate (see Breslauer, 1982a; and for the current succession, Brown, 1985; Gorbachev, 1985; Zagladin, 1985). Even more to the point, the power of new first secretaries is evident in their behavior—a far more concrete indicator of elite politics than veiled rhetoric. If intra-elite conflict is so pronounced, and if first secretaries are fighting for their political lives, as Roeder assumes, then why do new first secretaries always win?³ Moreover, if conflict reigns and new first secretaries are impotent, then why do we see, during succession periods, so many innovations (which hardly speaks to a policy process immobilized by conflict), so many controversial decisions (which hardly speaks to the dominance of "elite" interests), and, finally, so many dramatic decisions that are clearly the work of the purportedly embattled first secretary? Just a few of the examples one can point to within the period from 1953 to 1957, the period Roeder identifies as the most conflictual of the post-Stalinist era, are the innovations Khrushchev introduced at the 1953–1954 Agricultural Plena, Soviet rapprochement with Tito, the substantial reforms in housing and welfare introduced from 1955 to 1957, and, of course, de-Stalinization. All this seems to indicate that a better assumption, and the one which guided my analysis, is that new first secretaries, while operating with a limited mandate and finite political and economic resources, have strong incentives and some opportunities for innovation during succession periods. To assume a "leaderless" process, therefore, is to ignore the concrete facts at our disposal.

The second area of disagreement

between Roeder and myself is methodology.⁴ The "improvements" Roeder offers have, in fact, the opposite effect. Let us turn first to his independent variable: the division of the period from 1953 to 1974 into six regimes (see his Table 3). The first component of this classification is shifts in the power of the first secretary. While I agree with Roeder that my dichotomy between succession and postsuccession periods is more "blunt" than the ever-changing world of high politics, I would counter that further temporal distinctions seem to uphold my original results (Breslauer, 1982a), and that such distinctions can only be examined through contextual, and not qualitative analysis (see Bunce, 1981, chs. 1–2, 6).

The second point is crucial. While most specialists can agree on when succession begins and ends in the Soviet Union, and this legitimates drawing a clear dichotomy between succession and postsuccession periods, the same can hardly be said for the additional gradations (by month!) Roeder provides. I do not share his belief that Khrushchev's power in April 1960 can be ranked numerically against his power one month later, or that Khrushchev's power in 1958 can be ranked against Brezhnev's in 1969. The data we have about the inner workings of the Politburo are necessarily subject to varying interpretations (compare, for instance, Breslauer, 1982a; Linden, 1966; and Rush, 1968).

Indeed, students of the presidency, privy to much more information, would shudder if asked to make similar "close calls" about the month-by-month changes in the power of Carter or, more generally, Carter's power in 1979 versus Ford's in 1975. Of course, Roeder evidences some discomfort as well. He overlaps years from one regime to the next, and the years he actually uses change both within the article in question (1960–1964 becomes 1962–1964) and from one article to the next (Roeder, 1984, 1985).

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The problem of forcing rigor on bad data is also evident in his second measure of regime change: variations in levels of intra-elite conflict, as measured by elite survival rates. This is a poor measure. First, any small shift in (the controversial) timepoints would produce very different results. Second, it can be argued that survival rates may reflect governing style, not conflict—for example, Khrushchev's "distrust of cadres" versus Brezhnev's "trust in cadres" (see Breslauer, 1982a; Bunce, 1983), and may tap dynamics the opposite of those assumed by Roeder. For example, it can be argued that, rather than signifying the opposite, high turnover requires elite consensus.

Finally, even if we "pretend" that his regime boundaries and measures of power and conflict are all valid, we are still left with the serious problem that the results reported in Table 3—that is, the "final" regime rankings he employs in his analysis—do not add up. For example, according to his figures, column 9 ("mass interests") should register three ties, not the ranking from one to six he reports. Indeed, the most "mass-oriented" period in that column should not be 1957–1960, but rather 1953–1957 (the Khrushchev succession period, or what I originally argue!) and 1967–1970. Similar problems exist in column 10, where the most elite-oriented period, according to his numbers, should be 1953–1957, not the one he designates: 1960–1964.⁵ And if we work with the correct results, we are left with the curious conclusion that one period, 1953–1957, is, despite a world of trade-offs, the most responsive to both elites and masses.

Roeder's methodological problems, moreover, do not stop with his independent variable; they are also evident in his measures of public policy and his assessment of policy change. At the bare minimum, an assessment of elite responsiveness to mass concerns requires (1) internally consistent data, (2) examina-

tion of a wide range of redistributive policies that can be traced to elite action, and (3) controls for temporal lags and trade-offs.⁶ Roeder's analysis fails to meet any, let alone all of these preconditions, and this failure (not "ascendancy periods") explains his results.

The first problem is that Roeder relies heavily on budgetary and investment outlays, yet these data are not reliable for the timepoints he examines. If he interpolated the data, coded them as missing (which would still not reconcile differences over time or changes in budgetary scope), or subdivided them into smaller time frames, he should indicate that.

Second, Roeder confines his analysis to measures of policy that are easily quantified. This leads him, first, to mistakenly examine policies that are distributive, not redistributive—for example, growth in national income, sociocultural allocations in the budget (which includes distributive decisions on education, health, and science [see *Narkhoz*, 1976, p. 652]), and overall wage increases (instead of wage increases by class [see Bunce, 1980]). Moreover, it leads him to ignore programmatic changes. This is a serious deficiency. In contrast to shifts in funding and production, programmatic change has the decisive advantage of dealing with elite action and with redistributive issues. Moreover, such decisions occur on a precise date, and can serve as a check on patterns in less direct measures of decision making.

Indeed, if we examine changes over time in mass-oriented programs, it is clear that the "succession connection," and not Roeder's interpretation of postsuccession politics, is upheld. One can point, for example, to major expansions of the welfare state in 1955–1956; major increases in the minimum wage from 1953 to 1956, and in 1967–1968; significant boosts in rural living standards as a result of improvements in wages, pensions, social infrastructure, and state payments for

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Table 1. Corrected Specification of Years: Annual Growth Rates, 1953–1961

Expenditure Category	Succession 1953–1958 (%)	Roeder Ascendancy 1957–1960 (%)	Correct Ascendancy 1959–1961 (%)
Welfare Outlays (Total Budget)	11.2 (4.9)	3.1 (5.1)	5.8 (5.8)
Light Industry (Total Industry) ^a	10.2 (9.5)	15.5 (10.6)	1.1 (6.3)
Housing (Total Investment) ^a	21.8 (9.5)	8.1 (10.6)	1.4 (6.3)

Sources: Data sources for the budget are Pryor (1968, p. 375), and, for capital investment, *Narkhoz* (1959, pp. 548–51; 1962, p. 435).

Note: Roeder does not allow for lags or for changes in overall funding. When corrected, my conclusions are supported.

^aCapital investment begins in 1956.

delivery of agricultural produce following the 1953–1954 and 1965 Agricultural Plenas; the Decree on Housing in 1957; and the impact of all these decisions on income distribution and the quality of life—improvements which seem to have stalled significantly after 1968 (see Bronson and Krueger, 1971; Bunce, 1980, Tables 1 and 2; Bunce, 1981, chs. 5–6; Colton, 1984, pp. 8, 70; DiMaio, 1974, p. 18; Hough, 1976; Maier, 1974; McAuley, 1978, pp. 344, 307–309, 316; Volkov, 1973). In fact, the “succession connection” is even evident in the Gorbachev succession. In May 1985, the Central Committee announced sharp increases in the number of children eligible for state

aid, in the size of minimum pensions for *kolkhozniks* (about a 39% increase), and in state aid to the unemployable, single mothers, large families, and the “single” poor (see B Ts. K. KPSS, 1985).

The succession-related pattern in programmatic innovations is matched, moreover, by similar patterns in the quantifiable policies Roeder examines. He fails to pick these up and uncovers instead a post-succession cycle because he fails to control for lags and trade-offs. More specifically, Roeder allows overall funding increases to distort his results, and sets up his “postsuccession” periods in a way that “captures” decisions made during succession periods.⁷ Tables 1 and 2 below high-

Table 2. Annual Growth Rates, with Funding and Succession Periods Controlled

Expenditure Category	Succession (%)	Non-Succession (%)
Light Industrial Investment (Total Capital Investment)	7.6 (5.0)	3.5 (4.3)
Welfare Outlays (Total Budget)	7.9 (3.2) 7.0 ^a (2.4) ^a	3.8 (3.8) 5.6 ^a (9.5) ^a
Monthly Wages of Collective Farmers (Wages of Workers/Employees)	10.1 (5.2)	4.6 (3.2)
Housing Investment (Total Capital Investment)	7.7 (5.0)	2.7 (4.3)

Sources: The data are taken from Bunce (1980, Table 2); *Narkhoz* (1959, pp. 548–51; 1962, p. 435); and *U.N. Statistical Yearbooks* (1971, 1978, pp. 702, 881). Light industry was defined as the final six categories in industrial investment—that is, “glass through food preparation”—in SEV for 1965–75.

Note: The years are 1956–1977, though they sometimes deviate, given data problems. Succession periods are 1953–1958 and 1965–1968 (succession periods, proper, plus one-year lags for policy effects). Nonsuccession periods are the remainder.

^aThese data are from Pryor (1968, p. 375).

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light the importance of these considerations. When proper controls and time points are used, succession periods again stand out as times when mass concerns receive special attention.

I must conclude, therefore, that my original argument stands. There is a "succession connection" in the Soviet Union and, for that matter, in Eastern Europe.⁸

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In the article under discussion (Roeder, 1985), I argue that the alternation found by Bunce (1980) between "mass-oriented" and "elite-oriented" policies in the Soviet Union can be explained by the conjoint effect of a general secretary's capacity to innovate and his incentive structure. These last two variables are, in turn, the consequence of shifts in the distribution of power and competition for power within the Soviet leadership. This argument revises Bunce's original "succession connection" thesis by finding a different temporal pattern in the alternation of elite- and mass-oriented policies, and by arguing that the general secretary's incentive alone is not enough to explain this pattern. Bunce has now responded with a pointed critique of that article. This exchange, by drawing out the points of disagreement between Bunce and myself, contributes to growth in our understanding of the Soviet system. Therefore, after a brief response to the major points in Bunce's critique, I will venture suggestions for further research arising from this debate.

Bunce's first criticism points up a central conceptual difference in our approaches: while she writes of general secretaries varying in their personal orientations, my own analysis stresses changes in the constraints on decision making. In the latter, one need not make the dubious assumption that all general secretaries

undergo identical changes in personal orientation toward policies. Rather, recurring patterns of policy outputs and outcomes during successions are attributed to recurring patterns in succession politics that tend to induce general secretaries to make similar shifts in policy, even if their personal policy orientations remain constant.⁹

Closely related to this is Bunce's second point—our diverging conceptions of Soviet leadership politics. Bunce overdraws this difference. Although Russian history since at least the time of Svyatopolk the Accursed shows that successions in politics lacking strongly institutionalized procedures for the transfer of power can all too easily become "free-for-alls," the assumption of the inevitability of this is not part of my analysis. In fact, the analyses Bunce and I presented agree in key assumptions—specifically, that the general secretary has been the most important figure in the Soviet leadership throughout the period we examine, but that he does not rule alone; hence, the term "*first among equals*."

We disagree, however, in our diametrically opposite answers to the question whether one can ignore the impact of intra-leadership competition on policy outputs during successions. It is curious that Bunce should discount the importance of this, for the parallel between Soviet successions and democratic elections that she repeatedly draws suggests that competition among elites is central among the explanations for policy shifts. My own analysis rests on the claims (1) that intra-leadership conflict and the distribution of power within the leadership are variables; (2) that their variation is closely tied to successions; and (3) that changes in these affect both the general secretary's power to influence policy and his incentive structure. The evidence of elite contest within the Soviet leadership and its close association with successions is far more substantial than veiled dis-

putes in *Pravda*. The removal and execution of Beria, the exile of Molotov to Outer Mongolia, and the ouster of Khrushchev, to name but a few, are strong evidence of such conflict. Contrary to Bunce's claims, general secretaries do not always win and front runners do not always come out on top, as the early removal of Khrushchev and the failure of Malenkov attest. The original "succession connection" thesis ignores changes in the level of elite conflict and in the structure of power within the Soviet leadership. It ignores changes in the power of the general secretary and examines only his incentive structure. Thus, the original "succession connection" thesis must implicitly posit either (1) that the level of intra-elite competition is constant or (2) that the changes in level of elite competition do not affect policy making and policy. Neither is a tenable assumption.

The third disagreement concerns methodology. Contrary to Bunce's attempt at caricature, my method does not fall prey to the many evils that she conjures. For example, it does rely to an important extent upon contextual analysis. The numbers confirm and reinforce this analysis; they do not stand by themselves. The rankings of regimes do not require close calls between individual years (or months!); the analysis is closely crafted so as to avoid exactly this.¹⁰ Sovietologists have, indeed, made judgments about the power of the general secretary, and there is a surprising consensus in these judgments (e.g., Bialer, 1976; Breslauer, 1976, 1982a; Brown, 1978; Daniels, 1971; Hammer, 1974; Linden, 1966; Slusser, 1967).¹¹ Also contrary to Bunce's claims, Americanists have made similar—as well as more arcane—judgments about presidents (e.g., Barber, 1972; Chamberlain, 1946; Goldsmith, 1974; Spitzer, 1982). It is preposterous to fault the fairly conventional indicator of elite contest that I use (see Hough, 1976) by claiming that Politburo members in the process of removing or

attempting to remove one another constitute a unified leadership. The alternative, of course, is the view that the Soviets themselves have attempted to present, yet few American political scientists would take this seriously. The numbers in Table 3 do not add up because the numbers are not supposed to be added—the relationship is multiplicative!¹²

Our disagreement on method results from Bunce's rejection of methodological rigor. Analysis of a causal relationship—here between successions and policy—requires that one use consistent data and control for exogenous factors that might yield spurious results. Bunce's use of proportions with changing denominators (e.g., budgetary shares) and her failure to control for demographic changes or secular trends make her results suspect. A slovenly methodology only compounds the problems of a deficient data set. A more rigorous methodology is, indeed, an improvement.

Last, it is curious that Bunce attacks the data I employ. In fact, most of the limitations of this data set are the consequence of an attempt to duplicate (for the purpose of a more rigorous retest) the indicators in Bunce's (1980) article. (I provide no explanation for recoding of data or subdivision of data "into smaller time frames" since neither of these procedures is used in my analysis.) Bunce's critique of my use of these data repeats the methodological failings of her original analysis, leaving one again to suspect that the results of her analysis are simply spurious. Moreover, in this critique, the dependent variable ("policy") becomes an even more imprecise concept than in the original statement of the "succession connection" thesis. It is now even less clear whether "policy" in this thesis refers to statements of intent, budgetary outlays, financial investments, or the outcomes of policy. And in this critique the relationship between succession and policy output becomes even more tenuous. For

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example, Bunce defends her original thesis by arguing that increases in spending occur only after the succession is over (that is, in the "period of ascendancy"), due to "lags." If "pumping up" consumer policies is a principal way in which a general secretary builds authority during a succession, in what way does spending two years later (after the succession is over) affect the outcome of a succession? If we accept Bunce's most recent revision of the original "succession connection" thesis, we end up with such silly arguments as that Khrushchev secured his tenuous position in 1956 by pumping up mass-oriented policies in 1958. (Of course, as my original analysis, but not Bunce's, makes clear, one must distinguish between the immediate response in policy outputs and delayed responses in policy outcomes.)

Central to the differences in our supporting data are our diverging standards of evidence. Both Bunce and I seek to identify short-term changes or shifts in policy priorities (mass vs. elite orientation) corresponding to succession and post-succession periods. Bunce seems content to search the pages of Soviet history and statistical annuals for illustrative evidence—e.g., isolated decrees—without any systematic attempt to compare the significance or impact of these with similar policies in other periods. My own analysis employs data that permit temporal comparisons with periods that precede and follow the ones in question. Only by such longitudinal comparisons can we observe policy changes. My analysis also employs data that provide precise definitions of policy outputs and outcomes, for only when we have defined a policy precisely can we observe change in it. Quantifiable data are, of course, not the only evidence that meet these standards; nor, as Bunce's use of statistics illustrates, do all quantitative analyses necessarily meet these standards. Yet with statistical sophistication, quantifiable

data permit one to meet these standards in a parsimonious argument.

Rather than retreat behind defenses of our respective analyses, however, we might more profitably turn to the agenda of research suggested by this debate over Soviet successions and policy. Three items in particular have implications for the larger field of Sovietology: (1) Can we develop a more extensive set of measures of Soviet politics and policy that is consistent over time and permits more precise distinctions among policies (e.g., distributive vs. redistributive)?¹³ Soviet budgetary expenditures and investments in particular are an important source of information about Soviet politics and priorities. Bunce's (1980, 1981) work is significant in that it begins the search for such indicators. My own work (Roeder, 1985) seeks to give greater precision to these.¹⁴ (2) Can we expand nomothetic analysis within Sovietology? Contextual analysis, and particularly historical reconstruction, have been and probably will remain the mainstays of Sovietology. Yet Bunce's use of succession and nonsuccession periods as dichotomous variables that can explain policy priorities (independent of *Zeitgeist* or personality) is an important first step in the direction of theory building. I have sought to elaborate this theory. (3) Can we, through expanded comparative analysis, bring Sovietology closer to the theoretical developments in comparative politics? Bunce's work draws parallels between the "succession connection" of Leninist regimes and the "electoral connection" of polyarchies. My own work argues that parallels exist, but are more complex than those proposed in the original "succession connection" thesis. In short, this debate can be turned to a more constructive purpose.

The long-term outcome of this debate will be of interest to more than just Sovietologists. Our predictions of Soviet behavior—e.g., with regard to elite vs. mass orientation, immobilism vs. activ-

ism—have relevance for those who must formulate policies to deal with the Soviet regime. Our efforts to define more precisely what distinguishes a Leninist regime from a Western polyarchy (and in what ways they are alike) address concerns of comparativists, whether Sovietologists or not. Finally, our search for analytic rigor in both qualitative and quantitative analysis is a path already taken by many political scientists.

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Notes

1. I must note that Roeder's rendition of my work is incorrect in five regards. First, I do not characterize policy change during succession as either dramatic or as an iron law governing elite behavior (see Bunce, 1980, pp. 975–76; Bunce, 1981, chs. 5–7). Second, I do not equate campaigns in the East with those in the West—they differ in the composition and role of the electorate, the role of the issues, the sequencing of winning office versus mobilizing support, and, finally, in the nature of the mandate (Bunce, 1980; Bunce, 1981, chs. 1, 3–7). Third, I do not overlook constraints on the first secretary—I discuss at length the impact of conflict, policy specialization, and resource constraints (Bunce, 1980, 1981; and, for more recent assessments, Bunce, 1983). Fourth, I do not attribute my results to new leaders; instead, as a structuralist, I emphasize the interaction among (1) infrequent successions (which “locks in” interests and resistance to new ideas); (2) building ideological and political pressures to provide butter as well as factories, and (3) the role of the succession process in highlighting problems and legitimating some new policy alternatives. Finally, my point about mass unrest is that past experience is a powerful teacher—there was unrest, for example, in the Soviet Union, Czechoslovakia, and the GDR in 1953, and in Hungary and Poland in 1956. While elites differ on policy, they do not differ in their commitment to continued elite control or in their awareness that a prudent policy during succession periods is one which assumes unrest. To assume otherwise and be wrong would be

far more costly than taking anticipatory measures by wooing the public.

2. Unless otherwise indicated, all references to Roeder concern his article in the December 1985 issue of this *Review*, “Do New Leaders Really Make a Difference? Rethinking the ‘Succession Connection.’”

3. Brezhnev, for example, went through five American presidents.

4. I agree with Roeder that regression analysis would be preferable and that changes in the scope of the budget and plan present problems. However, Roeder hardly deals with the latter, since raw outlays are just affected as shares! Moreover, the “scope problem,” along with problems in internal consistency, forces one to use abbreviated time-points. This, in turn, allows for distortions in regression outcomes—for instance, the exaggerated impact of one unusual year. For this reason, I left the data in a raw form which allows the reader, not the technique, to render a decision, and I examined programmatic change, which Roeder ignores.

5. Also, growth in heavy industrial investment (the primary indicator of elite interests) from 1953 to 1957 does not uphold his argument (see *Narkhoz*, 1959, pp. 548–51).

6. Roeder seems to assume that domestic policy is like foreign policy, in that changes in elite politics produce discrete decisions and carry immediate policy consequences (see Roeder, 1984). Such is not the case. The real world of budgets, for example, indicates that (1) decisions are made on the basis of total available funding and trade-offs among interests, and (2) there are long lags between changes in elite politics and shifts in funding, since budgets take a full year to be constructed and are enacted in December of the preceding year (Garbuzov, 1971; Yevdokimov, 1974). To assume that political changes in mid-1957, for instance, affect allocations in 1957 (retroactively?) or that increases in light industrial outlays can be isolated from increases in total industrial investment, therefore, is to assume away the real world of winners, losers, and time lags.

7. Roeder claims to examine elite interests. However, he examines elite interests for a three-year period only (1962–1964), fails to compare elite versus mass policies, and only looks at two “elite” policies: one rather removed from direct elite control (heavy industrial production), and one which is estimated by the West and exists separately from the overall decisional context (military outlays).

8. Roeder to the contrary, this includes agricultural decision making (see Bunce, 1981, ch. 6; Volkov, 1973). That policies of lesser significance have been introduced at times other than succession periods does not challenge the fact that the major

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innovations in postwar Soviet agriculture occurred in 1953-1954 and 1965—that is, in succession periods.

9. In Bunce's analysis the key explanatory variable is a new general secretary, while in my own it is changes in decision-making processes that are affected by, among other things, successions. Failing to understand this conceptual difference, Bunce criticizes my assumptions about the policy preferences of general secretaries (for example, "the first secretary always champions 'mass' interests"), when in fact I make no such assumptions. The preferability of the latter approach is illustrated by the recent rapid succession of general secretaries from Andropov to Gorbachev. The logic of Bunce's analysis would have predicted a veritable explosion in mass-oriented policies as each new general secretary sought to secure his position. This, of course, did not happen. My own analysis would predict the very limited policy innovation throughout the protracted succession period beginning in 1982 that in fact has occurred.

10. The comparisons in my analysis are between periods of longer term. This "should not obscure the changes that take place within each period over time. In particular, toward the beginning and end of each period, decision making will exhibit many of the features of transition with a previous or new regime" (Roeder, 1984, p. 182). Of course, Sovietologists can, and frequently do, identify the month in which a succession begins or ends: for example, the death or removal of the former general secretary can be a good indicator of the former. In my analysis (Roeder, 1985), while the measure of elite competition is defined by month, the indicators of policy outputs and outcomes derived from Soviet statistical sources do not permit this; the latter are annual figures.

11. It is doubtful, however, that with the evidence available in the Soviet case one can separate, as Bunce attempts to do, "governing style" such as Brezhnev's "trust for cadres" from such constraints on that style as the extent of the general secretary's consolidation of power and the level of intra-leadership conflict.

12. Bunce's point here rests on an unfortunate typographical error in the footnote to Table 3. Columns 9 and 10 are derived by multiplying Column 6, not 5, by Columns 7 and 8, respectively. All columns show the rank orders of the products.

13. It is curious that Bunce's critique should emphasize the distinction between distributive and redistributive policies, for I introduce this distinction as an anomaly in Bunce's indicators that she completely ignores.

14. Many of Bunce's criticisms of my indicators—which are also her indicators—imply that "better" and therefore different data are needed. While this would be inappropriate in an initial retest of the original "succession connection" thesis, it is undeni-

ably a long-term need in the study of Soviet policy.

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EXPLAINING AFRICAN COUPS D'ETAT

Comparative political analysts seek empirical generalizations which will hold water across systems and over a period of time. Yet, modeling important political phenomena over more than a handful of countries is still rather unusual. One focus for substantial comparative research has been the coup d'état—an irregular change of governmental leadership by force—in African countries. Scholars who have engaged in this research find they have various conceptual and methodological differences of opinion. In this Controversy, Robert Jackman and Rosemary O'Kane raise the issues in dispute. Their contentions are answered by Thomas Johnson, Pat McGowan, and Robert Slater. The exchange highlights important research issues without necessarily resolving them.

In a recent paper, "Explaining African Military Coups d'État 1960–82," Johnson, Slater, and McGowan (1984) replicate and attempt to extend my earlier work on coups in sub-Saharan Africa (Jackman, 1978; see also Jackman and Boyd, 1979). They present an alternative model that seeks to account for the total volume of coups that occurred in Africa in the years from 1960 to 1982. Evidently, Johnson et al. are well satisfied with their extended model. It forms the basis of attempts (a) to examine African coups from a more "historical" perspective (McGowan and Johnson, 1984); (b) to forecast more recent coups (McGowan and Johnson, 1985a); and (c) to simulate the incidence of coups (Johnson and McGowan, 1985). Their paper in this *Review* makes two general points. First, Johnson et al. imply that I was arbitrarily selective in defining the appropriate set of countries for my analysis, and they report that when "six new African states" (1984, pp. 630–31) are added to the analysis, my model breaks

down completely. Secondly, Johnson et al. offer a new model they claim accounts "in a theoretically meaningful fashion for 91% of the variation in military coups" (1984, p. 621).

My purpose is to show that Johnson et al. have made a series of fundamental mistakes.* Their discussion of case selection is at odds with the basic canons of comparative analysis. Their new model suffers from crippling problems of measurement validity and of specification error. As a result, their analysis contributes nothing to our understanding of the forces that underlie coups d'état.

Selection of Cases

The analysis in Jackman (1978) sought to account for the total volume of coups experienced by the new states of sub-Saharan Africa in the years from 1960 to 1975. Given the substantive concerns and the period involved, what set of states does this include? I restricted my attention to members of the Organization of

African Unity located in sub-Saharan continental Africa. This is consistent with standard treatments (see e.g., Collier, 1982; Morrison, Mitchell, Paden, and Stevenson, 1972), and indeed, Johnson et al. also exclude the North African countries of Algeria, Egypt, Libya, Morocco, and Tunisia on the grounds that these countries belong to "the Middle Eastern, Arab-Islamic culture complex" (1984, p. 636). However, unlike Johnson et al., I also excluded Madagascar, on the grounds that its political history is distinct from that of the continental black African countries in much the same way as the political history of North Africa is distinct from that of sub-Saharan Africa. This doubtless accounts for the exclusion of Madagascar in analyses by other researchers as well. In addition, Ethiopia was excluded from the main part of the analysis because of its lack of political parties circa 1960. Johnson et al. "find this reasoning acceptable" (1984, n. 5), but then unaccountably include Ethiopia in their own analyses. They report that the inclusion of Ethiopia had no major effect on the estimates of my model (see the third column of their Table 5), though the lack of political parties in Ethiopia, circa 1960, makes one wonder how they generated a party dominance score for this country. Inclusion of Ethiopia can hardly be justified in the context of "replicating" my model.

This leaves four countries excluded by me but included by Johnson et al.: Botswana, Lesotho, Mauritius, and Swaziland. According to Johnson et al., I excluded the first two of these "without offering any justification" (1984, p. 629). But, the original paper referred to the data collected "for the 30 countries listed in the Appendix (*all of these countries were independent by 1965*)" (Jackman, 1978, p. 1264, emphasis added). All four countries listed above became independent after 1965, and were therefore excluded. Why was 1965 selected as a cutoff date?

In answering this question, it is useful to remember that a coup d'état is, literally, a strike against the state. This implies that a coup can only take place within a state. It should be evident that colonies are not states—hence, for example, the literature in the early 1960s on the rise of the "new states" (see, e.g., Emerson, 1962; Geertz, 1963). Colonies cannot experience a coup d'état.

This last point is important, because before we can make comparisons across units (in this case, nation states), we have to be sure that each unit is potentially capable of exhibiting the attribute of concern (see, e.g., DeFelice, 1980; Jackman, 1985). Despite statements to the contrary (e.g., Sartori, 1970), the units need not have actually exhibited the attribute (in this case, coups) to be compared, but they do have to be potentially capable of it. This criterion for the inclusion of cases directly parallels the demographic and actuarial notion of a "population at risk" (see, e.g., Barclay, 1958). Suppose we wish to calculate an infant mortality rate, and that we have, for a given period and unit, the number of infants born who died before age one. To calculate the infant mortality rate, the relevant denominator is the total number of births in the same period and unit (the population at risk). When examining coups d'état, colonies are not part of the population at risk.

In light of this, and given my concern with the years from 1960 to 1975, the focus clearly needed to center on states in sub-Saharan Africa. At the same time, it is desirable for a variety of reasons to maximize the sample size. In the tradeoff between maximizing sample size and restricting the focus of analysis to states, my compromise was to exclude states that were not independent by 1965, so that all cases would be potentially capable of experiencing a coup for at least two-thirds of the period under consideration. Thus, the data covered the various forms of coups for the years "from independence

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or 1960 (whichever came later) through 1975, inclusive" (Jackman, 1978, p. 1264). Any compromise may be subject to criticism. But the most reasonable criticism of this decision would be that it leaned too much in the direction of maximizing sample size, so that 1965 was too late a cutoff.

The criticism offered by Johnson et al., however, comes from the other side of the field—that 1970 (or 1968 in practice) is the defensible cutoff. (Indeed, they seem to imply that my definition of the set of appropriate countries was intentionally selective.) They further report that when the 1970 cutoff is employed, and Ethiopia and Madagascar are also included, my model suffers a "nearly complete breakdown" (1984, p. 629). I agree that under these circumstances my model fails from a statistical perspective, but what meaning should we attach to this fact?

The only reasonable inference is that my model does not apply to those cases to which it was never meant to apply—namely, Ethiopia, Madagascar, and four other states that became independent after 1965 (two in late 1966, and the other two in 1968).¹ I have already discussed the rationale for excluding Ethiopia and Madagascar. The remaining cases were not considered because they were not potentially capable of experiencing a coup for up to one-half of the period under consideration. Since they do not belong in the analysis, the failure of the model brought on by their inclusion is an inappropriate test of validity of that model.

The New Extended Model

Throughout their paper, Johnson et al. refer to their new model as a "theoretically informed" model. Presumably, this means that general arguments and concrete justifications are offered for the specific empirical model that is estimated. What is the broad substantive rationale for the model? What are the critical variables? What is the appropriate struc-

ture of the model to be estimated? How, and for what dates, are the critical variables measured? Do the particular measures chosen offer a reasonable representation of the variables of theoretical interest? Johnson et al. devote rather little attention to the specification of their empirical model. Instead, they report examining "a number of alternative sub-models," the details of which are not supplied. Then, they report: "Combining the results of our sub-model analyses permitted us to specify and to estimate the regressions reported in Table 7" (1984, pp. 633–34). This paragraph is the only discussion of how the empirical model was derived. What implicit theory informed the model? Certainly there is no explicit theory.

According to the model, the total volume of coups (1960–82) is said to be a function of (a) characteristics of the military, (b) political development or lack thereof, (c) social mobilization, (d) national political economy, and (e) international economic dependence. At a very general level, this appears to be reasonable. However, when one examines the exact measures used and the variety of dates to which they refer, the basis for the specified model is not at all clear. Indeed, the specific measures chosen are nowhere justified. They are simply listed (all 10 of them) in footnote 12 on page 634.

Additionally, while the dependent variable covers the period from 1960 to 1982, the "explanatory" variables refer to a variety of dates: e.g., 1950–60, 1975, and 1960–78. No explanation of this variety is offered. Even constraints of data availability cannot account for all of this temporal variation. As I show below, this introduces major potential problems of simultaneity bias.

The specification offered by Johnson et al. is indefensible, and most of the explanatory variables do not belong as part of an "explanation" of coups in the period. The only way to demonstrate this is to

consider each of the explanatory variables in turn, so as to get a sense of what this model is trying to tell us. This approach may seem a little data-centric, but the model appears to have been developed in a very data-centric manner. The lesson to be learned is that the statistical superstructure proposed by Johnson et al. rests on a weak conceptual foundation.

Characteristics of the Military Itself

Johnson et al. represent the military in terms of *army cohesion* and *army political centrality* (1984, Table 7). *Army cohesion* refers to "militaries that are large and ethnically homogeneous" (1984, p. 634, emphasis added). How is it measured? By taking the product of the size of the largest ethnic group (as a percentage of the population in the country as a whole) and the size of the military (total military manpower in thousands). Does a significant coefficient for this measure tell us anything about whether militaries are large and ethnically homogeneous? No, it does not. A high score could reflect this for a given country, but it could just as easily reflect a case where a smaller ethnic group is politically dominant and constitutes most of the army. The second outcome is not uncommon in African politics (remember Idi Amin?). What we have here is one version of the "ecological fallacy" (to use the old term). Johnson et al. to the contrary, their measure simply reflects (1) the raw size of the military, and (2) the ethnic diversity of the population as a whole. It tells us nothing about the ethnic composition of the military.

Because it is the product of military size and ethnic diversity, army cohesion actually represents an interaction between these two variables, with the "main" (additive) effects of military size and ethnic diversity assumed to be zero. In other words, Johnson et al. are proposing that neither of these two components is important in itself, but countries where the military is large and the largest ethnic

group is numerically dominant are especially prone to coups. The rationale for any such expectation is neither provided nor self-evident.²

One other point: Army cohesion is measured for circa 1970. There is a non-trivial problem in using this to "explain" coups starting in 1960, given that one can plausibly argue that after coups, the size of the military increases. The treatment of this variable by Johnson et al. runs a considerable risk of confusing cause with effect. As I show below, this is also an issue with some of their other "explanatory" variables. Minimally, one would have expected them to entertain the possibility of a reciprocal relation between coups and army cohesion. As it stands, their use of this variable (measured as it is for around 1970) in a recursive format raises a problem of simultaneity bias.

Army political centrality is said to reflect the degree to which the military is "relatively central because of its role in repression and because of its claim on state revenue" (1984, p. 634). The actual measure is again a product, this time of (1) government sanctions (1960-67) and (2) defense expenditures as a fraction of total government revenue (1965).³ Those who have worked with the sanctions data (Taylor and Hudson, 1972) know that they are a potpourri that include military actions as well as the activities of other political actors.

The broader point again concerns probability simultaneity bias. On the average, military governments are probably more repressive than non-military ones. Indeed, when one codes military coups from primary sources, one tends to notice that they are typically followed by martial law, arrests, banning of political parties, etc. There is systematic evidence to support the alternative specification that I am suggesting (e.g., Hibbs, 1973).

A similar argument can be made for the size of military budgets. Including both components in a multiplicative term

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makes the resulting measure difficult to interpret, and certainly does little to alleviate the wider potential problem of simultaneity. Labelling the resultant measure "army political centrality" is simply misleading.

Political Development or Lack Thereof

The claim that their model examines the effects of political development is questionable in view of the fact that development is never actually measured and incorporated into the model. Instead, we are offered measures of turnout (*turnout*) and of degree of multipartyism (*political pluralism*). I recognize that there is a good deal of debate over how development is best measured, but a reading of Huntington (1968) and the subsequent literature suggests these may not be optimal measures.

Turnout refers to pre-independence electoral turnout, and was also employed by Jackman (1978). It identifies the amount of participation in the last election supervised by the relevant metropole. One can readily justify the inclusion of this variable, but the construction that Johnson et al. put on it is dubious: "African states . . . which have *maintained or restored* some degree of political participation. . ." (1984, p. 635, my emphasis). The measure that is actually employed has nothing to do with the restoration of political participation after decolonization. Instead, it identifies part of the colonial legacy.

Political Pluralism: The inclusion of this variable, based on a measure of party fractionalization, is extraordinarily hard to justify, since it identifies fractionalization as of 1975.⁴ Note that the t-ratio for the estimate of political pluralism in Johnson et al. (1984) is about 8.5, which is larger than any other t-ratio reported for the model (see Table 7). But the estimate is impossible to interpret. How can one logically claim that party fractionaliza-

tion as of 1975 caused coups starting 15 years earlier?

Remember that in the African case, party fractionalization since independence has exhibited considerable short-term volatility, a good deal of which is caused by coups. After the typical coup, one of the first orders of business is to ban all political parties, or at least everyone else's. This tends to generate a negative correlation between the number of parties (1975) and the number of coups (1960-82), since 1975 is closer to the end of the period 1960-82 than to its beginning. This particular causal inference by Johnson et al. is ludicrous.⁵

Social Mobilization

Johnson et al. (1984) represent social mobilization as *percentage in agriculture* and *percentage in the capital city*. *Percentage in agriculture* is said to refer to the percentage of the population in agriculture (1960). Actually, it refers to the percentage of the labor force employed in the agricultural sector, and there is a non-trivial difference between the two denominators. In any event, Johnson et al. appear to have used the correct variable, but mislabelled it.

Percentage in the capital city refers to the percentage increase in the population of the capital city (1950-60). There are two problems here. While Deutsch (1961) and others do include urbanization as a component of mobilization, they do not restrict their attention to capital cities. There is no good reason for such a restriction, even though coups take place in capital cities for obvious reasons. Specifically, consider what social mobilization is supposed to involve—the erosion of older, more "traditional" attachments and the substitution (if not necessarily full acceptance) of newer attachments among the public. In fact, urbanization is often a dislocating experience. But what is so distinctive about capital cities? The measure used here im-

plies that urbanization only "counts" when the capital city is involved, but why should this be the case? For example, does the meaning people attach to the under- or unemployment that often accompanies mobilization vary according to whether it is experienced in the capital as opposed to some other city? Secondly, why is change from 1950 to 1960 taken as the relevant quantity to influence coups from 1960 to 1982? No justification is offered, and none is readily apparent.⁶

National Political Economy

Growth in GNP, *growth in industrial jobs*, and *exports-imports/GNP* are the variables used by Johnson et al. (1984) to represent the national political economy. *Growth in industrial jobs* refers to growth in the percentage of industrial jobs from 1960 to 1978, and is taken from World Bank sources. *Growth in GNP* refers to growth in GNP from 1965 to 1970. There are two problems here.

First, if 1960-78 is the relevant period for growth in industrial jobs, why is it not also the relevant period for growth in GNP? The source for the growth in GNP measure provides data covering the years up to 1975. Why were these data not used? Even better (for the reasons set forth in Jackman, 1980), why not use the available World Bank estimates based on continuous series for growth that would take us up to 1978 or 1980?

Exports-imports/GNP refers to the ratio of exports-imports to GNP (1965). Although no rationale is offered for this particular measure of economic performance, one can at least think of one for it. However, why is 1965 the relevant date, given the periods to which the other two measures of economic performance refer? The present treatment of the dates is inelegant, to say the least, especially considering that the dependent variable covers the period from 1960 to 1982.

Secondly, and much more importantly, there is again a potential simultaneity bias

here. While military regimes may not have a distinctive impact on economic growth, there is good reason to expect political instability to have a deleterious effect on growth, and the "dependent" variable in this analysis measures political instability, not degree of military influence in politics.

International Economic Dependence

Change in commodity concentration is the term used by Johnson et al. (1984) to refer to change in export commodity concentration from 1960 to 1965. This is a reasonable measure of one aspect of dependence, but it is not clear why 1960-65 is the relevant period. Moreover, it is not clear why this is the relevant dimension of dependence for the problem at hand—why, for example, is this measure more germane than the degree of economic penetration by multi-national corporations?

A final note on the measures. At the end of footnote 12 (Johnson et al., 1984, p. 634) we are told that "in a few [unspecified] cases the *mean value of the variable was used to estimate otherwise missing data*" (my emphasis). While this procedure can sometimes be justified, it strikes me as quite unwarranted in the present context, especially given that the total *N* is 35.⁷

To summarize, the model proposed by Johnson et al. is misspecified. I would like to underscore the point that substantive justification is needed for the inclusion of each measured variable in the model. Unfortunately, such justification is lacking in the analysis by Johnson et al. Several of the critical measures are invalid representations of the concepts they are said to reflect, and the inclusion of only one bad variable can distort the remaining coefficient estimates in the model. The dates to which many of these measures refer are highly variable, for no apparent reason. Coups (1960-82) are "explained" by variables referring to circa 1965, circa

1960, 1975, 1960, 1950-60, 1960-78, 1965-70, 1965, and 1960-65. Finally, there is a problem of major proportions having to do with simultaneity bias that affects many of the critical variables. This means that no clear inferences can be drawn from the model as specified.

What Is To Be Done?

Where does this leave the analysis of coups? Let us start by returning to my original model, the purpose of which was to estimate the effects of initial social and political conditions on the subsequent volume of coups for countries in sub-Saharan Africa.

Why is this an interesting question? The new states of sub-Saharan Africa share a number of characteristics deriving primarily from the colonial experience and the process of decolonization around 1960. Because these countries experienced decolonization at approximately the same time, they form an important setting in which to evaluate arguments about political development. First, these countries form part of the periphery of the so-called world economy (cf. Kick, 1984; Snyder and Kick, 1979), and a good deal of attention has focused in recent years on the prospects for development and growth in the periphery. Secondly, that they are new states means that they are relatively weak on at least one aspect of institutional capacity (cf. Huntington, 1968). Thirdly, the colonial experience was by no means uniform, as becomes clear when one contrasts, say, Kenya with Zaire.

Examining the effects of initial social and political conditions on subsequent levels of political instability is important, in part, because those initial conditions reflect aspects of the colonial legacy itself. While the metropolises did not have total control over the last election held before independence, their activities certainly had considerable bearing on those elections, as reflected by the level of par-

ticipation and the forms of the party systems in those elections. Similarly, the nineteenth-century "scramble" for Africa has obvious implications for contemporary variations in degree of cultural pluralism across the African states. The variety of colonial experiences also has a clear bearing on the variation across these states in the levels of social mobilization they exhibited around the time of independence. My concern was to evaluate the significance of these differences for subsequent levels of instability.

The estimates reported in Jackman (1978) indicate that these initial structural conditions had a pronounced effect on instability from circa 1960 to 1975, which is a relatively long period given the potential temporal volatility of events like coups d'état.⁸ The estimates reported by Johnson et al. (1984, Table 6, col. 3) indicate that most of these effects persisted when the period was extended through 1982, albeit at a weaker level.⁹ I did not anticipate that the effects would persist forever. That they held up through 1975 is noteworthy in itself, but, like all empirical political research, my analysis is clearly time-bound. However, there is nothing in the Johnson et al. analysis that modifies my original conclusions about the structural sources of political instability in sub-Saharan Africa from 1960 to 1975. Nor does their effort advance our understanding of coups in more recent years.

I have emphasized my concern with the effects of initial conditions on the volume of subsequent events. This concern was underscored at several points in the original paper. Before one can draw reasonable causal inferences, problems of temporal sequencing need to be addressed. If they are not, the potential for simultaneity bias that plagues the model offered by Johnson et al. is unavoidable.

Moreover, my purpose was to examine the structural forces that generated a volume of instability in the set of coun-

tries and period at hand. Johnson et al. opine that since coups are inherently "dynamic" events (1984, p. 637), we ultimately need to engage in time-series analyses of these events if we are to develop good models. This is misleading, because dynamic properties inhere not in events or attributes, but in how we study them.

It is important to remember the distinction between the long-term structural forces and the short-run precipitants that underlie coups d'état. The former may help to determine which countries are more prone to coups, but obviously do not address the issue of when a coup is likely to occur in a given country. While cross-sectional analysis is a useful mode of analysis for isolating relevant structural forces (especially those like cultural pluralism that exhibit considerable temporal stability), it is less useful in understanding the precipitants of coups (cf. Jackman, 1985). Examining the latter issue requires both more of a temporal focus and (perhaps) a different set of explanatory variables. The two forms of analysis address distinct questions; the answers to which should complement each other.¹⁰

Finally, Johnson et al. complain that my model is "rather mainstream" (1984, p. 632), in the sense that it ignores characteristics of the military and economic dependency. On the first count, I believe that earlier analyses have overrated the significance of military organization for coups d'état—certainly, Johnson et al. provide no firm evidence to the contrary. Instead, I think that coups are fundamentally political in nature. And if one contemplates events like the coup of 1963 in Togo, it is not at all clear why organizational characteristics of the military should loom so large in explanations of coups. On the second count, it needs repeating that a focus on sub-Saharan African states immediately centers one's attention on the periphery of the world

economy. To attempt an explanation of coups in the African states involves ipso facto a consideration of the prospects for political development in the periphery. If this is mainstream thinking, the field of comparative politics has a brighter future than I had realized.

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In principle, Johnson, Slater, and McGowan's (1984) "Explaining African Military Coups d'État, 1960-1982" advances our understanding of important political events in true Popperian manner. It takes a well-respected hypothesis about African coups (Jackman, 1978), replicates it, adjusts it, and improves on it, taking the condition of our knowledge and ability to explain coups d'état a significant step forward. In practice, unfortunately, it does none of these things, and seems likely to succeed only in diminishing the perceived value of statistical analysis for the already skeptical African specialists. In their article, Johnson et al. take a hypothesis about coups in tropical black Africa, apply that hypothesis only to military coups and to countries that are not all tropical black African countries, and produce results different from Jackman's. Claims are then made for the importance of these results beyond their limitations. In so doing Johnson et al. demonstrate a lack of appreciation of the part played in coups by the military; an ignorance of some African and some European countries; a failure to appreciate the implications of the assumption of *ceteris paribus*; and last but not least, an insensitivity to the dangers of tautology. These considerations point clearly to the value of statistical analyses of models generated by theories as opposed to a collection of "best fit" hypotheses. Furthermore, they reawaken Zolberg's (1968) approach, which both Jackman and John-

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son et al. have sought to undermine. On balance they also support rather than disprove Jackman.

To begin, then: whether all coups or only military coups should be studied will depend on the nature of the hypothesis to be tested: Jackman's model was concerned with coups, not the narrower category of military coups. Separating military from nonmilitary coups is a highly dubious exercise. An accepted part of the definition of a coup is that it is a strategy for government overthrow which involves the threat or use of violence.¹¹ That the military should play such an important part in coups is not surprising. The definition of a coup crucially suggests that no particular significance should be attached to the military playing an active part in it. Where violence needs only to be threatened rather than used, the military may appear not to have been involved when its support has in fact been critical.¹² However, if it is the military's involvement in Africa, rather than coups, which seems interesting, then a different problem arises. Military personnel can be given government posts, and often are placed over affairs of defense especially. The threat or even supposed threat of intervention could in any case be enough to ensure that a civilian government ruled in the interests of the military. It should not be forgotten, too, that in Africa the emphasis on the importance of the military in threatening violence may be entirely misplaced, for not all African countries have had a standing army for the entire period of their independence, and some still have not (*Europa Year Book*, 1982). The police, for no other more significant reasons, may therefore be the important force in these African countries.

That the causes of African coups differ from the causes of coups in other areas of the world is a reasonable proposition; it is also a testable one. African countries generally share a colonial heritage, recent

independence, and tribalism, and suffer from conditions of economic backwardness (low GNP, export dependency on primary goods, and the like). There are also important differences. Johnson et al. emphasize the difference between the northern Arab states and the black African countries on which they concentrate (that is, those independent before 1970 and not under minority rule, such as is the case in Namibia and South Africa). It is a theme which seems to follow Jackman. Johnson et al., unlike Jackman, do not seem to appreciate the theoretical support for the separation out of a particular set of countries. Zolberg (1968, p. 71) makes a strong case for the existence of important shared conditions amongst the "newly independent countries of Tropical Africa": "extremely weak national centers, a periphery which consists of societies until recently self-contained, and levels of economic and social development approaching the lowest limits of international statistical distributions." In using data from only tropical African countries, Jackman performs an experiment to discover the difference between coup and noncoup countries under the assumption of *ceteris paribus* (other things being equal). He therefore correctly removes Ethiopia from his final predictive model for not being "newly independent." Ethiopia, unlike all the other tropical African countries, was never colonized, only being occupied by Italian troops for the short period from 1935 to 1941. Johnson et al. do not accept the importance of this *ceteris paribus* assumption, and are shocked by Jackman's failure to include Madagascar, Mauritius, Swaziland, Botswana, Lesotho, and Ethiopia. The reason these countries were excluded is, of course, because none of them share Zolberg's similar backgrounds in being tropical African states. Two of them are not even black African states.

Ethiopia's lack of a colonial history has already been mentioned. The southern

African countries (Botswana, Lesotho, and Swaziland) differ in crucial respects. Their economies, both import and export, are heavily dependent on South Africa; they are also dependent on South Africa for external defense. They have weak central governments in the sense of being dominated by South Africa, but not "weak national centres" in Zolberg's sense. They are not intensely split by tribes. Mauritius is not even a black African country, with two-thirds of the population being of Indian descent and the remainder being either European (mainly French) or a mixture of Indian and European. It is also hard to view Madagascar as a black African state when the majority of its population is of Malaya-Polynesian stock and its homogeneous Indonesian/African culture is unique (Kurian, 1982). Both, of course, are in the Indian Ocean and not on the mainland. The only thing which Mauritius seems to have in common with the other new countries in Johnson et al.'s data is that since independence it, much like Lesotho, Swaziland, and Botswana, has had an armed police force rather than an army. Mauritius still has no standing defense force, only a special police mobile unit. In Lesotho a plan for converting the paramilitary force into an army was only announced in 1979. Botswana did not establish an army until 1977, while in Swaziland this was done in 1973. Johnson et al. (1984, n. 10) note: "Four of these states—Botswana, Lesotho, Mauritius and Swaziland—are among the most stable in Africa and have never recorded a single military intervention event." While Botswana has an impressive record of stability, this is not so true of the other three countries. Mauritius has been described as being "plagued by political, industrial and racial unrest" (*Europa Year Book*, 1982, vol. 2, p. 928). In both Swaziland and Lesotho, in 1973 and 1970 respectively, the constitution was suspended and opposition leaders were

imprisoned. Those events in Lesotho are often described as a coup; in Swaziland it took until 1978 before proper elections were held. The failure of attempted coups were reported in Lesotho in 1974 and Swaziland in 1984, and coup plots were alleged in both countries at the end of 1983.

Whereas Jackman has performed a *ceteris paribus* exercise, Johnson et al. have performed a test on a collection of countries that neither share the same backgrounds nor are randomly sampled. Indeed, they are a very odd bunch of extra countries. While not sharing Johnson et al.'s view (1984, p. 637) that the poor quality of data for countries with populations of under one million would normally justify their being excluded from cross-national research (small countries with open societies can collect highly accurate statistics, and Equatorial Guinea is an especially closed society), it is interesting that three of these six extra countries had populations of under one million in 1982: Mauritius, Swaziland, and Botswana. (Gabon and Gambia also have populations of less than a million, and Congo had until 1970 [United Nations, 1984].) Furthermore, it is clear that if the North African states were included on the seemingly important criterion of their being members of the Organization of African Unity (OAU), then far more coup countries would have been added. Coups occurred in Egypt in 1952, Algeria in 1965, and Libya in 1969 (*Keesing's Contemporary Archives*). The emphasis on the North African countries being crucially different from countries included in the study, on the grounds that they belong to the "Middle Eastern, Arab-Islamic culture complex whose military patterns differ from those of Black Africa" (Johnson et al., 1984, p. 636), is not very convincing anyway. If "military patterns" implies having military coups or not, these examples suggest that they cannot be so dissimilar. The northern regions of the black

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African countries bordering the North African states are populated by Arabs. The importance of the Muslim religion in black Africa is also not lost on the Ugandans who suffered under Amin, nor is it underestimated in the reinforcing cleavages of tribe and religion which divide Nigeria, nor especially perhaps in Sudan, in the light of the recent coup.

In short, it is not surprising that Johnson et al.'s and Jackman's results should differ. Nor is it surprising that (as demonstrated in their Table 6) Johnson et al. should find the addition of the six countries to Jackman's data to be relatively more important in reducing the significance of Jackman's model ($R^2 = 50\%$) than the addition of the seven years ($R^2 = 59\%$). Contrary to Johnson et al.'s condemnation of Jackman's model, however, these results suggest that his model continued to perform well for seven years. Crucially for Johnson et al.'s model, the implication of this arbitrary sample is that though it may achieve a better "fit" for the cases which are included, it is extremely doubtful that they will have generated any theoretical implications for either coups in Africa or coups in general.

The proposition that the causes of coups are different in Africa (or a part of Africa) from elsewhere is not only a testable hypothesis, but one which cannot be conclusively demonstrated on purely African data, even where the levels of significance achieved by a model are very high. The opposed hypothesis, that the causes of coups are essentially similar wherever they occur, needs to be rejected. Coups are, after all, world-wide phenomena. Johnson et al.'s justification for concentrating on Africa—that "the states of black Africa share a continuing record of coup activity into the 1980s" (1984, p. 636)—is peculiarly weak. Coup activity continues elsewhere too, as even cursory reading of political events clearly demonstrates. What then is needed is an unbiased sample of countries which both

have and have not had coups, both inside and outside Africa. The notion that the sample should only include third world countries because the universe of countries would include "many states of the first and second worlds where coups are not a theoretical or practical problem" is quite extraordinary. It is one thing not to acknowledge that Mauritius and Madagascar are not black African countries; it is quite another to ignore the successful coups in Greece (1967 and 1973); Portugal (1974); and Turkey (1960 and 1980); of the unsuccessful coup in Cyprus (1974); of the attempted coup in Spain (1981), and reported plots in 1980, 1981, and 1983; of the report of an attempted coup in Romania (1983); and last but not least, of General Jaruzelski's military intervention in Poland in 1981 (*Keesing's Contemporary Archives*). These coups and events may not have occurred in such large numbers as in Africa or Latin America, but being general phenomena they clearly invite general explanation. Applying Occam's Razor, if a general explanation can be found for all coups, it must be preferred to specific area explanations. The effects on significance scores of restricted variance in independent variables has been demonstrated elsewhere (O'Kane, 1983); in particular, the importance of economic variables such as GNP per capita are likely to be obscured when their possible full range is excluded.

Jackman and Johnson et al. alike, while recognizing Zolberg's (1968) contribution in stressing the similarities between the tropical African societies, scorn his theory for being unable to explain why coups occur in some countries and not others. This is to miss entirely a most valuable insight into the explanation for coups. Zolberg argues that though these countries in theory share conditions conducive to coups, in practice other conditions may even so restrict coups—recent independence (mentioned by Johnson et al. but used in a different context)—or promote

them—coups engender other coups within one country and can also be contagious outside a country.¹³ Clearly the value of the notion of coups encouraging other coups cannot be employed when the dependent variable is a composite measure of all coup events, for it would become pure tautology.

The general suggestion that there may be conditions that do not cause coups but may hinder them in spite of their necessary preconditions is, though, a view held elsewhere (Luttwak, 1969; O'Kane, 1981). Looking again at the six countries which Johnson et al. add, it seems reasonable to suppose that such dependent relationships with South Africa might hinder coups in spite of the existence of necessary underlying conditions, and that the army (or police force where there is no army) might also present an obstacle to coups where the officers are foreign (as was the case in 1970 in Botswana, Mauritius, Cameroon, Gambia, Malawi, Zambia, and Gabon [Dupuy and Blanchard, 1972]). The presence of foreign armoured divisions within a state might also have a similar hindering effect on coups (O'Kane, 1981). Furthermore, these are problems which occur outside as well as inside Africa. It is also clear that the model produced by Johnson et al. does not reflect specifically African phenomena.¹⁴

The variables included in Johnson et al.'s model themselves require further consideration than they are given. Particularly worrying is variable 3, "political pluralism, 1975." This is clearly a variable which is not independent of the dependent variable. Indeed, it seems to be tautological: countries which have military intervention scores for either 1960-82 or 1970-82 are likely not to have "political pluralism" in 1975, and indeed, those countries with high military intervention scores are more or less bound not to have political pluralism in 1975. It is surely this circularity which accounts for the very high level of significance which this

variable achieves. It would be interesting to know what levels of explained variance variable 3 achieves on its own, and also what the regression minus variable 3 achieves. Jackman's argument in respect of pluralism does not seem to have been disproved, as Johnson et al. claim, and even if it had, it is surprising that Johnson et al. fail to appreciate the implication of variable 4, which clearly supports Jackman. The attraction of the value of this "pre-independence" variable clearly lies in its being a condition which predates military interventions.

The same concern about the direction of cause and effect also applies to the two army variables: military intervention may itself lead to increased expenditure on the military, increased use of the military to suppress opposition, and to changes in the relative predominance of a particular ethnic group in the army. Rather than offering an explanation for military intervention, the fact that variable 2, "army political centrality, c. 1965," is not significant in the 1970-1982 model suggests that it is military intervention that causes both increased expenditure on the military and increased use of the military to sanction opposition. Variable 1, "army cohesion, c. 1950," remains significant for both models, but it is not convincing from the explanation of this variable that it is measuring what it claims. Logically, it should be a measure for the proportion of the army composed of a single ethnic group. It seems more likely that it is a measure of the proportions within the population as a whole multiplied by the absolute size of the army, a variable closer to Jackman's view of the importance of ethnicity, and therefore at least as likely to be as supportive of his views as of Johnson et al.'s. (I wonder still how they have dealt with those countries having only an armed police force and no army; if they have been scored as zero, then it is certainly neither surprising nor of any importance to find that coun-

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tries without armies do not have armies intervening.)

The remaining variables also cause worries about the theoretical importance of the findings. It is still possible that military intervention has had an effect on these socioeconomic variables. Sometimes the military has a good record of encouraging mobilization (e.g., Brazil, 1964). This could then account for the rather large decline in the R^2 from 87% to 68%, though the current view seems to be that the military's performance in government does not differ greatly from civilian performance (Jackman, 1976; McKinley and Cohan, 1975). Certainly the persuasiveness and interesting mixture of findings would have been heightened by the comparison of effect within the same period of time for all the variables. Given the large number of variables in the equation (10 + constant) and the smallness of the data set ($N = 35$), the possibility remains that some of these variables are significant only because they apply to a particular case, rather than indicate a general model as claimed. This is especially a concern given that none of the countries with populations of under one million have had a successful coup, and none of them are scored higher than 4 on the Total Military Involvement Score (TMIS). It would be interesting to see how Johnson et al.'s model performs on data from Jackman's 29 countries only. My expectation is that it would perform either little different from or less well than Jackman's model performed at 50% for the 35 countries. In short, Johnson et al. have not improved on Jackman. They have unintentionally directed attention away from the restricted analysis of African coups and towards the analysis of coups d'état as general phenomena.

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Professors O'Kane and Jackman direct their criticisms at our 1984 article in this *Review*. Given that the article in question is part of an ongoing research program involving a technical report (Orkand, 1983), several conference papers (Johnson and McGowan, 1985; Johnson and Slater, 1983; McGowan and Johnson, 1985a), and two other articles (McGowan and Johnson, 1984, 1985b), we must refer our critics and others interested in this exchange to these publications and available writings for answers to many of Jackman's and O'Kane's critical comments.¹⁵ Restrictions of space force us to limit our response here to their more substantial criticisms.

Case Selection

Jackman and O'Kane are both troubled by the cases upon which we built our analysis of African military coups d'état. For Jackman, the issue of our case selection is crucial to his defense of his 1978 model: he claims that when we added 6 countries to his original set of 29, this resulted in "an inappropriate test of the validity of that model." O'Kane, in turn, states that we do not understand the assumption of *ceteris paribus* because we "have performed a test on a collection of countries that neither share the same backgrounds nor are randomly sampled." We find these arguments to be unfounded.

We took care to spell out explicitly the justification for each of the 35 countries selected for analysis (Johnson, Slater, and McGowan, 1984, pp. 636-37). We note that neither Jackman (1978; see also Jackman and Boyd, 1979) nor O'Kane (1981, 1983) provided explanations for the cases studied in their research on coups d'état. We now learn that Jackman selected only those black African states independent as of 1965, and thus capable of experiencing a coup for "at least two-thirds of the period under consideration," and that four independent states were excluded

because "they were not potentially capable of experiencing a coup for up to one-half of the period under consideration." Now that we know Jackman's decision rule, we agree with it; we used similar reasoning for selecting 1970 as our cutting point, so that all of the 35 states we studied were independent for at least 61% of our 1960-1982 time period (Johnson et al., 1984, p. 637).

Even within the limits of Jackman's shorter time frame (1960-1975), Botswana and Lesotho were independent for just over nine years, or somewhat more than 56% of the identified period; Swaziland and Mauritius were independent for over seven years, or more than 44% of Jackman's sixteen-year period. We do not understand, then, why Jackman excluded Botswana and Lesotho on the grounds that they were not independent for one-half his time frame, and would suggest, in regard to Swaziland and Mauritius, that 44% is sufficiently close to 50% that he should have studied these countries as well.¹⁶

When Jackman writes that we conducted an inappropriate test of his model because we included 6 states that "do not belong in the analysis," he forgets to mention that we did estimate his model with the Total Military Involvement Score (TMIS) for the period 1960-1982 as the dependent variable for his 29 states, and found it did not fit well (Johnson et al., 1984, p. 631, Table 6). Therefore, we cannot accept the conclusion that we conducted an inappropriate test. We evaluated his model both for his 29 cases and for our 35 cases, and in each instance it did not work, yet all cases met the test of Jackman's own decision rule: that they be capable of experiencing the phenomenon in question, coups d'état, for more than one-half the time period considered.

Jackman understands the issues involved here, for he has recently written that "Even a statistical generalization based on 50 cases can be very much

affected by just one or two cases, and it is essential to be sure that such 'influential data points' are not responsible for the empirical results" (Jackman, 1985, p. 167). What we found and reported in 1984 is that the excellent empirical results he obtained in 1978 were highly sensitive to the addition of 6 new cases (thereby increasing *N* by 20%) and seven additional years of behavior. Moreover, we demonstrated that it was more the addition of countries than the increased time period that was responsible for the problems we identified (Johnson et al., 1984, pp. 630-31). We would suggest that we followed rather closely Jackman's then unpublished methodological advice concerning how generalizations in cross-national research can be affected by the addition or deletion of but a few cases.

O'Kane claims we do not understand the assumption of *ceteris paribus* because we "have performed a test on a collection of countries, neither sharing the same backgrounds nor randomly sampled." She objects to the inclusion of Ethiopia, Madagascar, Mauritius, Swaziland, Botswana, and Lesotho in the analysis, and defends Jackman's exclusion of these states. O'Kane argues that Jackman's 29 cases represent a homogeneous set with a theoretical justification, whereas our 35 African states represent an inappropriate mix of tropical, black African and "other" Third World states.

The logic of comparison does not mean that all cases should possess highly similar attributes—comparison inherently connotes differences as well as similarities (DeFelice, 1980). If O'Kane believes that only homogeneous subpopulations may appropriately be substituted for random samples of states, then she should consider Jackman's arguments that "to say that two items are comparable also may be taken to mean that they can be compared for similarities and differences . . . comparative analysis does *not* require that items being compared be of the same

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class. Such a restriction, in fact, unnecessarily reduces the range of variability that we seek to understand" (Jackman, 1985, p. 168).

If, as O'Kane claims, it is wrong for us to include Madagascar and Mauritius because they are manifestly not tropical or black African states, why is it appropriate to include Mauritania and the Sudan, as both Jackman and we did, given that these two countries are dominated by Moorish and Arab cultures quite as different from the black African cultural mainstream as are the predominantly Indian and Afro-Indonesian cultures of Mauritius and Madagascar? Why is it wrong for us to include the ethnically homogeneous states of Botswana (99%), Lesotho (99%), and Swaziland and not also wrong to include ethnically homogeneous Burundi (90%), Rwanda (90%), and Somalia (97%), as both we and Jackman did?¹⁷ Jackman's 29 cases are, in fact, quite heterogeneous in terms of historical, cultural, and ethnic patterns, and no more meet O'Kane's conception of a *ceteris paribus* exercise than do our 35 states.

The presumption, apparently shared by Jackman and O'Kane, that we should study only those countries "sharing the same backgrounds" is self-defeating. While the member states of the Organization of African Unity (OAU) share many similarities, such as poverty, underdevelopment, dependence, and, with but two exceptions, a history of colonialism and recent decolonization, they also clearly manifest numerous and increasing differences (Tordoff, 1984, pp. 1-4). Any sizable subset of these states will contain striking and, no doubt, theoretically important differences. *Ceteris paribus* in the study of Africa is not an absolute, but rather a matter of degree; we remain satisfied that common membership in the African regional subsystem and in its major interstate organizations, such as the OAU and the African Development Bank, renders our set of 35 states meaningfully comparable.

O'Kane defends Jackman's (1978, p. 1268) exclusion of Ethiopia and criticizes us for including it because "Ethiopia, unlike all the other tropical African countries, was never colonized." We know that Ethiopia was never colonized by a European power, but only briefly occupied by the Italians. Liberia also was never a colony. No rule of logic supports excluding Ethiopia for lack of a colonial background while not also excluding Liberia for the same reason.¹⁸ Yet both Jackman (1978, p. 1274) and we (Johnson et al., 1984, p. 627) have included Liberia, and O'Kane does not object.

Regarding the case of Ethiopia, we were puzzled to read Jackman's statement that "... the lack of political parties in Ethiopia, circa 1960, makes one wonder how they generated a party dominance score for this country." The clear implication is that we made up a score that did not exist. Our party dominance score for Ethiopia (15%) comes from the same source originally used by Jackman: Morrison, Mitchell, Paden, and Stevenson (1972, p. 103, Table 8.8).

Model Specification

Jackman attacks our model specification, claiming that we gave "little attention" to this critical theoretical exercise. But our 1984 *Review* article is but one part of a research effort that has been ongoing since October 1982. A technical report (Orkand, 1983) and one of our conference papers (Johnson and Slater, 1983) provided abundant explanation for our final, ten-variable model. Space constraints placed upon our *Review* article precluded a full discussion of model specification procedures beyond the literature review provided (Johnson et al., 1984, pp. 632-33), which did justify the model's theoretical underpinnings.

The technical report and conference paper (Orkand, 1983; Johnson and Slater, 1983) feature an extensive review of the literature on military intervention. They

derive and specify from this literature five competing theoretical models of the causes of military involvement in African politics that deal with theories of political development, national economic performance, international economic performance, social mobilization and change, and military motivations (Orkand, 1983, pp. IV1-IV31; Johnson and Slater, 1983, pp. 6-35). Once specified, each model was evaluated against appropriate data for our 35 states (Orkand, 1983, pp. VI1-VI20; Johnson and Slater, 1983, pp. 43-55) in a fashion similar to O'Kane's (1983) strategy. The results of this stage in our research led us to conclude:

This effort arrived at deductively specifying and estimating five models of military intervention in sub-Saharan Africa and it provides powerful evidence that a concept as elusive as military intervention can be statistically explained. . . . Clearly, it is plausible that a model which builds on the results of these five models might provide an even more powerful explanation. Theoretically, such a model is defensible because the individual models should not be viewed as mutually exclusive. Moreover, our statistical results suggest that none of the models can be completely ignored. Thus, a next research step is the combination of key variables into a more general model of military intervention. Such a model is the subject of subsequent research. (Johnson and Slater, 1983, pp. 55-56)

This subsequent research resulted in our published model, but only after we had thoroughly evaluated Jackman's (1978) model for its validity when it was applied to our research problem and expanded data base. While our *Review* article may give an impression of being "data-centric," our total research effort has been powerfully informed by theoretical reasoning.

Operationalization

Regarding our dependent variable, Professor O'Kane is critical of our focus on military coups, which she regards as "a highly dubious exercise." Her own

research (O'Kane, 1981, pp. 297-98; 1983, p. 42) purports to examine "all" coups, whatever the nature of the participants, in "all" states ($N = 125$), during the period from 1950 to February 1971. She argues that our focus on African military coups caused us to overlook those instances where "the police may . . . be the important force in these African countries" or in which there was no overt use of force by the military.

We cannot have stated more explicitly (Johnson et al., 1984, p. 625) that we collected all coup events where "elements of the state's military, security, or police forces played a role in the event" (our emphasis). As previously noted (Orkand, 1983) and cited (McGowan and Johnson, 1984, p. 637), our conceptual and operational definitions of military coups d'état included the recognition that "physical injury resulting from a coup may be negligible or pronounced" (Orkand, 1983, p. A2). Both police actions and threats of violence are included in our conception of military coups.

In criticizing our research O'Kane does not sufficiently recognize that, at least in Africa, as Tordoff (1984, p. 153) has recently written, ". . . coups are typically army coups." Yet, in her own research she does recognize the significance of the military in such research: "the variable representing the presence of a well-organized army . . . was both significant and correlated in the expected direction, suggesting military characteristics to be important for the event of a coup" (O'Kane, 1983, p. 33, our emphasis). Having compared our coup data with her list of all countries where coups occurred between 1950 and 1971 (O'Kane, 1981, p. 298; 1983, p. 42), we find the same 14 African states as in our own list. Her more inclusive conception of coups d'état does not generate, in the African instance, more coup observations than our allegedly "narrower" definition.

Indeed, O'Kane (1981, p. 298, n. 37)

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claims that of these 14 states, only 4—Benin, Sierra Leone, Sudan, and Togo—experienced more than one coup in the years 1950–1970. We agree in these four instances, but we have recorded multiple coups in another 5 states—Burundi (2), Congo (3), Nigeria (2), Uganda (2), and Zaire (2). Our definition of coups d'état led us to observe 27 coups in these 14 countries, whereas O'Kane counted only a maximum of 21 coups, and perhaps less.¹⁹

Finally, of the 28 coups d'état that occurred in sub-Saharan Africa in the period 1950–1971, the only one not part of our data set is that which took place in Lesotho in January 1970 (Morrison et al., 1972, p. 280), as it does not meet our stated criteria for a military coup. We note that while O'Kane states that "those events in Lesotho are often described as a coup" she did not count this event as a coup d'état in her own research (O'Kane, 1981, p. 298; 1983, p. 42). O'Kane's argument that not all coups are military coups may have some merit outside sub-Saharan Africa, but it is a distinction without an empirical difference as far as Africa is concerned.

Both Jackman and O'Kane criticize one of our key explanatory variables, political pluralism measured in 1975. Because our dependent variable, TMIS, is measured over the period from 1960 to 1982, Jackman believes that our operationalization leads to a simultaneity bias, while O'Kane finds this variable tautological and circular. Our critics appear to be saying the same thing with different words. Because they believe that political pluralism is conceptually and operationally very close to being the exact opposite of military intervention and coups, they believe the 1960–1975 TMIS may have generated the 1975 political pluralism scores, that in 1975 pluralism and TMIS are the same thing with different names, and that any impact 1975 pluralism has on the 1976–1982 TMIS is not causal but rather a func-

tion of circular reasoning and tautological measurement procedures.

First, it should be noted that our TMIS index includes attempted coups and reported coup plots as well as successful coups. Together they comprise 270 points, or 51% of the 530 points in our TMIS index for 1960–1982. Logically as well as empirically, African states can accumulate a considerable amount of our dependent variable while maintaining a civilian, pluralistic political regime.

Second, political pluralism and military coups are not the same thing either conceptually or operationally. Conceptually, pluralism refers to the degree to which individuals and groups can express and act upon their political beliefs and preferences. In Africa this is most widespread in multiparty civilian regimes and most restricted in entirely military regimes that rule by decree. One-party and one-party dominant regimes in which the military may or may not share power with civilians are intermediate. We operationalized this concept by using an index which gives the highest pluralism scores to regimes in which many political parties of equal size sit in a legislature, and the lowest value to regimes in which there is no legislative body at all. We are tautological only if successful military coups always lead to long-term African military regimes that rule by decree without permitting the existence of legislatures and political parties—but such is not the case.

Third, contrary to what Jackman asserts, even when African militaries seize power in successful coups d'état, they do not always completely destroy political pluralism. Jackman has stated elsewhere that "the bulk of the cross-national evidence . . . indicates that the military/civilian government distinction does not help account for patterns of economic growth, social change or even the size of military budgets" (Jackman, 1985, p. 170). He cites his own previous work (Jackman, 1976) in suggesting the lack of

impact these types of regimes have on subsequent economic, societal, and military dynamics.

Through the end of 1984 there have been 56 coups in 26 of the 45 sub-Saharan African states (McGowan and Johnson, 1984, p. 638; 1985a, Tables 2 and 3). Only in 35 instances in 23 states did the military form a purely military regime which lasted any period of time. In 21 instances it quickly turned power over to a new set of civilian politicians, or formed what Anene (in progress) calls a "fused regime," in which power was shared with civilian politicians and technocrats. Even in the 35 cases where the military has ruled directly for a period of time, in only 11 countries did it continue to rule by the end of 1984. In the 24 other instances of military regimes since the first military coup in the Sudan in 1958, the military has turned power over to elected civilian regimes nine times and has formed fused civilian-military regimes in the remaining 15 cases (Anene, in progress). Clearly, as we argued (Johnson et al., 1984, p. 635), some degree of political pluralism, even if it is only of the one-party or one-party dominant type of regime, has been maintained by numerous African states (of the 19 regimes that have never had a coup, 18 are multi-party or one-party dominant and Swaziland is a monarchy with no parties) or has been reintroduced in the 15 states noted above with what Collier (1982, pp. 147-48) calls "one-party plebiscitary" regimes. Our argument is that, all other things being equal, serious military interventions are becoming less likely where such regimes are in place.

Fourth, we can report new evidence relevant to this point. Nine out of 45 sub-Saharan African states never witnessed an attempted or successful coup during the period 1958-1984. Three of these states have maintained multiparty regimes—Botswana, Lesotho, and Mauritius—while five have single-party civilian governments; Swaziland is a monarchy

without parties (Young, 1984, pp. 468-69). Next, we can partition our data into two time periods, 1958-1975 and 1976-1984. If we then ask, for each time period, what type of political regime was in place in each of the remaining 36 states at the time of one of the 56 successful coups or one of the 62 unsuccessful coup attempts, two highly interesting patterns are evident. First, even though the initial time period covers 18 years and the second only 9 years, the 1976-1984 period includes 42% of all coups and coup attempts. Serious military interventions are increasing with time in sub-Saharan Africa (McGowan and Johnson, 1984, pp. 641-44). Second, and most importantly for this discussion, there has been a pronounced shift with time in the kind of regime experiencing serious interventions. In the period 1958-1975, fully 73% of serious military interventions happened to civilian regimes in which some or a great deal of political pluralism existed, thereby indicating a positive relationship between pluralism and interventions, as Jackman found in 1978 (p. 1271). Beginning in 1976, however, the pattern is reversed, with 64% of the interventions happening to regimes led by the military, thereby indicating a negative relationship between coups and pluralism, as we found in the context of a multiple regression analysis (Johnson et al., 1984, pp. 634-35). Furthermore, the explanation of this is not that there are no civilian regimes left in Africa against which the military can act. As of the end of 1984, there were 21 fully civilian regimes, 13 fused regimes, and only 11 entirely military regimes, a clear preponderance in the civilian, pluralist direction.

Fifth, if Jackman or O'Kane had tested their claim of "tautology," they would have found that the bivariate correlation between our dependent variable, TMIS 1960-1982, and our measure of political pluralism in 1975 is only $-.42$. This correlation is hardly of the magnitude to

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suggest tautological measurement. Moreover, four other bivariate correlations between our dependent variable and independent variables have similar or greater correlations: army cohesion, .49; pre-independence voter turnout, -.41; percentage of industrial jobs, .40; and army political centrality, .36.

Lastly, one cannot have it both ways. If we have introduced a simultaneity bias into our model because it includes a variable measured in 1975 along with a dependent variable measured for the period 1960-1982, what about Jackman's own model, which covers the years 1960-1975, and which contains as an indicator of social mobilization a variable measured in "c. 1965 to 1966" (Jackman, 1978, p. 1273)? Jackman simply assumes away the problem of simultaneity bias by remarking that this "is a variable that *probably* underwent little pronounced change in the preceding six years" (1978, p. 1273, our emphasis). While plausible, in this statement he assumes for himself what he asks us to demonstrate.

Model Validity

The comments made by Jackman and O'Kane primarily attempt to call into question the internal validity of our model, but without presenting alternative models demonstrably superior to it. We did demonstrate (Johnson et al., 1984, Table 4) the shortcomings of Jackman's model when we applied it to our more comprehensive data, and then presented what we continue to believe is a superior model.

O'Kane suggested a direct test of the competing models when she wrote "It would be very interesting to see how Johnson et al.'s model performs on data from Jackman's 29 countries only. My expectation is that it would perform either little different from or less well than Jackman's model performed at 50% for the 35 countries." Actually, Jackman's model,

when fitted to our data, performed at the 43% level, with only one out of seven explanatory variables remaining significant (Johnson et al., 1984, p. 628). Table 1 undertakes this evaluation. The first column repeats our original 1984 result. Column two fits our model to Jackman's 29 cases. The statistical evidence indicates decisively that it performs at least as well on these 29 countries alone as it does when 35 cases are examined. Our parameter estimates are all of the same sign as before, and they evidence little change in magnitude. If our indicator of industrial job creation is now only significant at the .10 level—something Jackman (1978, p. 1271) was willing to accept for his interaction term for party dominance and participation—the total regression model explains slightly more variance than before.

Moreover, because we have continued to collect TMIS data beyond 1982, we can provide an additional test of our model's explanatory validity. During 1983 and 1984 our 35 states experienced 4 successful coups, 6 coup attempts, and 12 reported coup plots (McGowan and Johnson, 1985a, Tables 2 and 3). These events add a total of 50 TMIS points to our index, and represent two additional years of behavior. In column three of Table 1 we have fitted our original model to this temporally expanded data set, and it fits very well. Our results are stable and valid with respect to both time and space, and for our research problem our model is demonstrably superior to Jackman's.

This belief is based not just on our original analyses and the new regressions reported in Table 1, but also on our model's demonstrated utility as a forecasting tool. In an exercise in *ex post* forecasting (Orkand, 1983, pp. VI5-VI10), we used Jackman's model in a discriminant function analysis to assign our 35 African countries to "coup-prone" and "non-coup-prone" groups for the years from 1960 to 1982. Jackman's model mis-

Table 1. Criticism and Validity: Further Evidence

Variables	Our Previous Result ^a	O'Kane's Suggested Test ^b	Our New Result ^b
Dependent Variable	TMIS ^c 1960-82	TMIS 1960-82	TMIS 1960-84
Constant	79.708 (10.115) ^d	75.708 (13.658)	74.503 (11.237)
Army Cohesion, ca. 1970	.319 (.113)	.371 (.117)	.433 (.126)
Army political centrality, ca. 1965	.0037 (.0016)	.004 (.002)	.004 (.002)
Political pluralism, 1975	-76.312 (8.863)	-43.823 (16.610)	-75.610 (9.846)
Turnout, pre-independence	-17.986 (6.521)	-26.940 (8.639)	-18.354 (7.244)
Percentage of population in agriculture, 1960	-.653 (.095)	-.620 (.123)	-.600 (.106)
Percentage of increase in the population of the capital city, 1950-1960	.043 (.008)	.055 (.009)	.044 (.009)
Absolute increase in percentage of industrial jobs, 1960-1978	-180.339 (68.881)	-131.534* (74.598)	-118.802** (76.517)
Percentage increase in GNP, 1965-1970	-16.633 (4.228)	-16.133 (5.374)	-17.333 (4.697)
Ratio of exports-imports to GNP, 1965	-.763 (.137)	-.815 (.221)	-.710 (.512)
Percentage increase in export commodity concentration, 1960-1965	.150 (.034)	.143 (.035)	.174 (.038)
Number of Cases ^e	35	29	35
R Square	.906	.921	.900
\bar{R} Square	.872	.884	.864
F	23.24	20.95	21.69
p of F	.0001	.0001	.0001

^aSource: Johnson, Slater, and McGowan (1984, p. 635). Calculated by SAS PROC SYSREG, release 79.6, in March 1983.

^bCalculated by SAS PROC SYSREG, release 82.3, in September 1985.

^cTotal Military Involvement Score.

^dStandard errors are reported in parentheses.

^eFor the sub-Saharan African countries in each regression, see Johnson, et al. (1984, p. 627).

*Only significant at the .095 level.

**Only significant at the .134 level.

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Table 2. *Ex Ante* Forecasts of Serious Military Intervention Probabilities for 1985

High Probability States (N=15)			
Burkina Faso	.99	Chad	.94
Ghana	.99	Mauritania	.94
Nigeria ^a	.99	Sudan ^a	.94
Benin	.98	Niger	.92
Burundi	.98	Zaire	.91
Congo	.98	Ethiopia	.89
Uganda ^a	.98	Sierra Leone	.88
Central African Republic	.97		
Average Probability = .952		Average TMIS ^b 1960-1984 = 26.9	
Moderate Probability States (N=11)			
Somalia	.73	Zambia	.50
Guinea	.67	Rwanda	.43
Togo	.66	Kenya	.38
Mali	.63	Senegal	.34
Madagascar	.58	Liberia	.30
Tanzania	.56		
Average Probability = .525		Average TMIS 1960-1984 = 10.3	
Low Probability States (N=9)			
Cameroon	.23	Malawi	.03
Gabon	.09	Gambia	.01
Swaziland	.07	Lesotho	.01
Botswana	.03	Mauritius	.01
Ivory Coast	.03		
Average Probability = .057		Average TMIS 1960-1984 = 1.8	

Source: Modified from McGowan and Johnson, 1985a, Table 5.

^aStates where successful military coups have occurred between January 1, 1985 and September 20, 1985.

^bTotal Military Involvement Score.

classified 20% of the cases, indicating that Botswana, Guinea, Lesotho, Malawi, Swaziland, and Tanzania were coup-prone, when in fact they did not experience coups in these years. Madagascar was misclassified as non-coup-prone, when in fact it had coups in 1972 and 1975 (note that Jackman's model did correctly classify two of our cases that he did not study, Ethiopia and Mauritius, and that Guinea, Malawi, and Tanzania were part of his original 29 states). When this exercise was repeated with our ten-variable model (Orkand, 1983, pp. VII1-VII7)

only 8.6% of the cases were misclassified—Guinea and Zambia were said to be coup-prone and Liberia was non-coup-prone. This superior performance led us to undertake subsequent *ex ante* forecasts using our model.

As recently reported (McGowan and Johnson, 1985a, Tables 2 and 3), in March 1983, again using discriminant function analysis, we classified 35 sub-Saharan African states into a group of 16 coup-prone countries where we forecast that serious military intervention events were most likely during 1983 and 1984, and

into a group of 19 non-coup-prone states where coups and coup attempts were relatively unlikely. As noted, we continued to collect military intervention events, and we observed 4 coups, 6 coup attempts, and 12 plots, representing a total of 50 TMIS points during these two years. Seventy-four percent of these points happened in our smaller group of coup-prone states. Even more significantly, eight serious events, comprising 3 coups and 5 attempts, occurred among our coup-prone group in Burkina Faso (formerly Upper Volta), Ghana, Nigeria, Sudan, Niger, and Mauritania. Only two events, a coup in Guinea, to which we gave a .51 probability, and an attempted coup in Cameroon, which we forecast as only .13 likely, happened in our larger non-coup-prone set of countries.

These favorable results encouraged us to generate in January 1985 *ex ante* forecasts for the current year (1985), given in Table 2. As reported elsewhere (McGowan and Johnson, 1985a, pp. 11-13), we used simple Bayesian techniques to transform our prior probabilities from 1983-1984 into posterior probabilities for 1985 based upon the historical experiences of all 35 states since 1960. As of 20 September 1985, coups d'état have taken place in the Sudan ($p=.94$), Uganda ($p=.98$), and Nigeria ($p=.99$). While we would be the first to point out that our probabilities should not be taken to represent point predictions, we would argue that our threefold classification into high, moderate, and low probability states does make a great deal of sense, and is predictively accurate—the coups that have so far occurred in 1985 have all been among our high probability states.

Conclusion

Following Hermann's (1967) classic discussion of model validation in international and comparative politics, we

would conclude that it is incumbent upon Jackman and O'Kane to account for why our "misspecified," "best fit" model which "... contributes nothing to our understanding of the forces that underlie coups d'état" has proven to be both a powerful explanatory device and an efficient forecasting tool. We found many of the comments by Jackman and O'Kane interesting, and we thank them for taking our work so seriously. Nevertheless, we remain convinced that a model so adequate in accounting statistically for past behavior, and which also renders useful forecasts, clearly represents a contribution to our understanding of coups d'état and to the literature of comparative politics.

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Notes

*I would like to thank Mary Jackman and Brian Silver for their comments and criticism.

1. Johnson et al. (1984, pp. 636-37) suggest as a "rule of thumb" that countries with populations of less than one million be excluded from cross-national analyses. The rationale for this is unclear. Had they taken their own advice, three of these four countries that became independent after 1965 would also have to be excluded from their analysis. As of mid-1982, Botswana, Mauritius, and Swaziland each had estimated populations of less than one million (World Bank, 1984, p. 276).

2. Nor is it clear why total military manpower is the relevant quantity, since, all other things equal, one would expect this to vary directly with population size. To what extent, then, is total military manpower serving as a very crude proxy for population size, and why should the latter have a bearing on coups? When comparing the relative size of military organizations, the usual procedure is to calculate military participation ratios, which are formed by

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dividing the total military manpower by the size of the population at risk of being enlisted or conscripted, namely, the number of working-age people.

3. Since army political centrality is a product, it also represents a statistical interaction between government sanctions and defense expenditures, with the additive effects of the latter two components again assumed (with no discussion) to be zero.

4. Johnson et al. (1984, p. 634, n. 12) report that they calculated fractionalization according to the formula in Bornschier and Heintz (1979, p. 267). The measure employed is that popularized by Rae and Taylor (1970) and earlier developed by Greenberg (1956). Problems with this measure of fragmentation are discussed by Jackman (1978, p. 1265, n. 5).

5. The logical issues involved here have been addressed in previous literature on the military in politics (see, e.g., Dowse, 1969).

6. There are two practical problems with the measure of urbanization (see, e.g., Taylor and Jodice, 1983, p. 162). First, what is the population size cutoff point that distinguishes urban from non-urban areas? It is difficult to resolve this issue in a satisfactory way, and the problem is compounded by the variety of national definitions of what constitutes a "city." Second is the even more difficult issue concerning the demarcation of city limits. However, Johnson et al. neither invoke these problems to justify their own approach, nor do they obviate them.

7. In their attempt to simulate coups based on this model, Johnson and McGowan select six countries for detailed analysis (1985, Table 2). For three (Ghana, Togo, and Uganda) the variable political pluralism had to be ignored because these states had no legislative body in 1975 (1985, n. 11).

8. In their Table 1, Johnson et al. report my estimates (col. 1) along with their own, using the same data (col. 2). The figures are identical, except that their estimates involving social mobilization and party dominance are 100 times larger than my own. Johnson et al. offer the following explanation for this discrepancy: "Jackman omitted decimal points from his percentage measures (sic) of social mobilization and party dominance, whereas we did not, which accounts for the different placement of the decimal points for the regression coefficients of these two variables in our two regressions" (1984, p. 625, n. 4). Johnson et al. seem unaware of the difference between percentages and proportions.

9. Johnson et al. consider the problem of potential collinearity in my model (pp. 629-630). This discussion is not fruitful, for at least two reasons. First, while it is true that my interaction terms are somewhat collinear with their components, it is also true that my parameter estimates all have acceptable t-ratios (see their Table 1, col. 1). Since the principal effect of collinearity is to inflate standard errors, it is

evident that collinearity was not seriously leading me astray. Secondly, it is almost always true that collinearity is present when interaction terms are formed in the manner I followed, but other criteria (such as improved goodness of fit) are available to evaluate the importance of statistical interactions. The relevant arguments are addressed in greater detail by Friedrich (1982).

10. Recent developments in methods of event-history analysis seem to offer a particularly promising perspective for future analyses of coups (see Allison, 1984).

11. See Jackman (1978), O'Kane (1981), and First (1970).

12. This application to the military logically follows from Bachrach and Baratz (1962), where the argument is applied to decision making behind the scenes.

13. It is important to note the implication of the restricted data set for Jackman's model. Because he performs a *ceteris paribus* experiment, he cannot demonstrate the importance of conditions conducive to coups which are similar for all tropical African countries (as Zolberg suggests). As a consequence, Jackman cannot be sure about his claim to have estimated "a model of the structural determinants of coups d'état for the new states of black Africa in the years from 1960 through 1975." It is equally possible that he has simply discovered some additional obstacles to coups.

14. See O'Kane (1981) for an explicitly general theory concerned with dependency on primary exports, which produces a statistical model, and which, furthermore, explicitly tests for structural stability by calculating the F-Ratio (Johnston, 1963, pp. 136-37) for the two subsets of "developing" and "developed" nations and finds no significant difference.

15. Copies of the conference papers and technical report are available at cost from Johnson, while our data set is available from McGowan, again at cost.

16. Mauritius gained independence in March 1968. If we use 1968 as its first year of independence, then it was capable of experiencing coups for exactly 50% of Jackman's time span.

17. The percentages are the portion of the population sharing the same mother language, i.e., Tswana, Sotho, Rundi, Rwanda, and Somali (Morrison et al., 1972).

18. O'Kane seems to be unaware that Jackman excluded Ethiopia because he thought (incorrectly—see Morrison et al., 1972, pp. 102-3) that as a historical monarchy it did not have a history of electoral activity.

19. We observed 5 coups in Benin (formerly Dahomey) between 1963 and 1969 (see Morrison et al., 1972, pp. 231-32), and all we know from O'Kane is that Benin had at least 2 coups between independence in 1960 and the end of 1970. Her actual coup count may be as low as 18.

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THE IMPACT OF TAXES IN DEVELOPED CAPITALIST COUNTRIES

A research question of perennial interest is, "What are the exogenous variables which best account for the differential distribution of income across nations?" Better understanding of the causes of inequalities in income distribution has been sought by investigating the effects of tax policies on income allocation and growth in capitalist countries. The quantitative analyses of these matters have engendered some debate about the adequacy of data, and the sufficiency of measurement. A preliminary foray into this research vineyard by Claudio Katz, Vincent Mahler, and Michael Franz is criticized here by Ronald King and Steven Jackson. In turn, Katz, Mahler, and Franz respond.

In the current debate over the social, economic, and political determinants of income distribution across nations, surprisingly little attention has been given to the public policy instruments through which governments are alleged to make their mark. To this extent, the recent paper by Katz, Mahler, and Franz (1983) is an important contribution to the literature, examining the impact of taxes upon the growth and allocation of incomes in advanced capitalist countries. Nevertheless, that paper is liable to criticism. In a previous note in this *Review*, Przeworski and Wallerstein (1985) examined the issue of growth, suggesting a theoretical complexity in the relationship between marginal tax rates and business investment. In this comment, we wish to address the empirical issue of actual income redistribution. We will focus internally on the authors' specification of dependent and independent variables, and externally on their

general research design.¹ The results from our reanalysis suggest modification of the optimistic conclusion by Katz et al. "that the fiscal strategies typically adopted by liberal and social democratic governments to distribute income more widely do have the intended effect"—i.e., that their higher utilization of progressive tax instruments is statistically "associated with relatively egalitarian patterns of income distribution."

Dependent Variable

Katz et al. take their dependent variable from Malcolm Sawyer's study (1976, pp. 3-36) for the Organization for Economic Cooperation and Development (OECD), which generated reasonably comparable income distribution data for a limited number of countries for some single year in the late 1960s or early 1970s. The post-tax standardized data set selected by the authors operationalizes income as factor

earnings in labor and capital markets, plus public and private transfers, minus direct (but not indirect) taxes and social security contributions, all adjusted for cross-national demographic differences in household size. Surprisingly, this measure of post-tax income shares is used without comparison to the pre-tax original position. We thus prefer to use percentage change, pre- to post-tax, which is available from the Sawyer data in unstandardized form, and for 10 rather than 11 countries. Their measure possibly incorporates some negative feedback from taxation to market apportionment. Yet post-tax income, taken by itself, is the product of much more than tax redistribution effects, and thus it is suspect whether the authors are actually focusing upon the contributions specifically induced by government revenue mechanisms.

Regardless of the measure employed, the total amount of effective fiscal redistribution is empirically quite small. Calculated in terms of Gini coefficients (although the findings are not substantially altered by the use of any other conventional summary statistic), all Sawyer countries do experience some egalitarian improvement. The mean gain, however, is only 6.37%, and the correlation of post-tax to pre-tax distribution is .919. On the average, the bottom quintile of the population receives only 6.0% of income, even after government taxes and transfers, while the top quintile garners 41.0%. In just three countries (Japan, the Netherlands, Sweden) does the bottom quintile have a post-fisc income share greater than 7%, and the latter two countries drop below that threshold without the statistical adjustment for demography.

Independent Variable

For their independent variable, Katz et al. use the share of a country's total economic product seized through the main forms of government taxation.

Ratios of the tax instrument to the Gross Domestic Product (GDP) were constructed to tap the redistribution that occurs when fiscal tools extract money amounts unevenly across population deciles, and the implicit counterfactual is the income distribution that would remain if any such tool were dropped from the public repertoire, leaving the funds collected in private hands. Although this is plausible, it is possible to go a bit further and decompose the tax instrument/GDP ratios into their two component segments—tax instrument/total taxes and total taxes/GDP—roughly separating the structure of a revenue system from its overall level. This permits us to distinguish countries with low tax collection levels yet high proportionate utilization of progressive tax instruments from those with high collection levels yet low progressive utilization. It also permits us to assess statistically the impact of national differences in tax structure, while controlling for the differences in state size. Such an analysis implies an alternative counterfactual, in which the size of the fiscal state is exogenously set by expenditure needs and macroeconomic objectives. Thus the redistributive effect of a given tax instrument is assessed relative to possible replacements.

The separation of tax structure from tax level produces some important findings. Only 5 of the Sawyer countries collected as much as half of their taxes from ostensibly progressive revenue sources (i.e., income, corporate, estate, and property taxes); only 7 of the 23 OECD countries did so in 1980. For the Sawyer sample, the correlation of total taxes/GDP with progressive taxes/total taxes is $-.428$, which contrasts with the positive association ($r=.525$) of tax state size with welfare transfers/total expenditures. Similarly, it is state size ($r=.583$), not relative reliance upon progressive tax sources ($r=.082$), that correlates with socialist ministers. These results are really not surprising.

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Social security contributions are earmarked for transfer payments, implying a direct connection between regressive taxation and progressive welfare expenditures. Socialist governments rely upon income taxes a bit more than others, yet also upon regressive consumption taxes apparently necessary to raise needed revenues. High use of the property tax is principally a phenomenon of the English-speaking countries, while low corporate taxes are sometimes used by countries with low-unemployment fiscal policies to compensate business and encourage capital formation. The complexity of public revenue structures suggests that the authors' simple language of zero-sum redistribution is insufficient to capture the political logic of taxation policy.

Statistical Model

Advanced capitalist democracies, of course, do cause some redistribution through their revenue collection, although it must be noted that transfers apparently exert about double the impact of taxation. Our argument is that tax redistribution should be presented as a function of the original pre-tax position, the progressivity of the revenue structure, and the total size of the tax state. Translated into a formal model, the equation reads:

$$\begin{aligned} \% \text{ChangeDist}[\text{Pre-Post}/\text{Pre}] \\ = b_0(\text{ProgTax}/\text{Total Tax})^{b_1} \times \\ (\text{Total Tax}/\text{GDP})^{b_2}, \end{aligned}$$

which contrasts with the much simpler formulation of Katz et al.:

$$\text{PostTaxDist} = b_3 + b_4(\text{ProgTax}/\text{GDP}).$$

Ideally, our multiplicative model could be estimated using logarithms. Unfortunately, with only 10 cases, and with distinct outliers among them, it is impossible to evaluate such a multivariate regression equation meaningfully. Bivariate correla-

tion shows that the percentage Gini improvement, pre- to post-tax, is strongly associated with progressive taxes/GDP ($r=.604$), as Katz et al. would predict. Upon disaggregation, however, effective redistribution retains a significant relationship with state size ($r=.513$), while that for progressive tax share virtually disappears ($r=.123$). Table 1 shows that among the countries examined, only Sweden combined a high tax level with high progressive tax reliance. Importantly, Norway and the Netherlands, with below-median progressive tax reliance but large state size, also rank near the very top of the effective tax redistribution list. These countries all stand above the United Kingdom and Canada, which have high progressive tax reliance and median range state size, and far above the United States, Japan, and Australia, which have high progressive tax reliance but below-median state size. Germany and France, which have very low progressive tax reliance, also rank low in redistribution. The implication is that Gini improvement in the Sawyer sample is largely the effect of high levels of tax extraction by countries with average tax structures, rather than the effect of any extraordinary employment of ostensibly progressive instruments.

Graphic representations reveal the basis for the observed correlation differences. In addition, residuals were examined to see whether there is a progressive share impact that would otherwise remain obscured. A plot of progressive tax reliance against the redistribution residual not explained by the regression of Gini change with total taxes shows no visible relationship, and the insignificant correlation ($r=.399$) approaches zero upon the elimination of France, an extreme outlier. This analysis was then repeated for each major type of tax instrument. Redistribution might be affected by some of the parts, even if not by the package taken as a whole. When state size is controlled for,

Table 1. Gini Coefficients, Tax Redistribution, and Revenue Instruments

Country	Year	Pre-Tax Gini	Post-Tax Gini	Percent Gini Change ^a	Percent Progressive Tax/ Total Taxes ^b	Percent Total Taxes/ GDP ^b
Norway	1970	.354	.307	13.28	40.92 (.84)	39.19 (1.29)
Sweden	1972	.346	.302	12.72	51.56 (1.17)	42.79 (1.36)
Netherlands	1967	.385	.354	8.05	39.96 (.94)	38.05 (1.32)
United Kingdom	1973	.344	.318	7.56	54.61 (1.23)	31.87 (1.01)
Canada	1969	.382	.354	7.33	56.73 (1.32)	32.03 (1.07)
United States	1972	.404	.381	5.69	60.25 (1.37)	29.63 (.95)
Japan	1969	.335	.316	5.67	49.53 (1.15)	18.19 (.61)
Germany	1973	.396	.383	3.28	38.85 (.87)	36.27 (1.15)
France	1970	.416	.414	0.48	21.78 (.50)	35.58 (1.17)
Australia	1966-1967	.313	.312	0.32	62.78 (1.48)	24.82 (.86)

^aPre-tax Gini - Post-tax Gini

Pre-tax Gini

^bAbsolute percentage and ratio to OECD mean (in parentheses) for appropriate year. Sources: Sawyer (1976); Organization for Economic Cooperation and Development (1982).

correlations of instrument reliance with Gini change residuals all showed the expected sign—personal income, profits, and property taxes were positive; social security and consumption taxes were negative—but only the income tax result approached statistical significance ($r=.481$). Again, this association was entirely caused by the exceptional position of France. With France excluded, no relationship remains ($r=.042$).

Tax redistribution is the joint product of tax level and tax structure. That joint product is somewhat positive for all the countries in the limited Sawyer sample, although there are notable differences among them. From our evidence, we cannot safely infer that those differences in redistribution are influenced by differences in the way those countries tax, but we can assert that they are strongly influenced by the amount of taxes those countries collect. One must be wary when making multivariate inferences from a small and irregular sample. To the extent our findings are true, however, egalitarians should generally be distressed by recent neoconservative attacks upon the size of the state and the amount of taxes

extracted, but they should also maintain doubt as to whether mere manipulations of public finance, as evidenced by the current agenda of "liberal and social democratic governments," can be sufficient to achieve desired ends.

Project Assumption

Finally, it must be remembered that the entire enterprise of Katz et al. (and these emendations as well) rests upon the critical assumption that greater use of progressive tax sources, relative either to GDP or to total taxes, corresponds proportionately to greater effective progressivity, both within each country's tax system and comparatively across them. Given the complexity and the cross-national differences in revenue codes, grounds for suspicion exist. On the basis of our model, once supplied with the pre-tax Gini distribution, the amount of post-tax redistribution, and the total amount of taxes extracted, it is logically possible to deduce effective tax structure progressivity. This has been performed by Tachibanaki (1981). Importantly, the correlation of his deduced progressivity coef-

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ficient with the progressive taxes/total taxes ratio is only .486; with the progressive taxes/GDP ratio (the Katz et al. indicator) it is .346; and it is lower still for each tax instrument taken individually. Progressive tax reliance is apparently not the same as tax progressivity, and provides a very poor measure for it, casting doubt upon the foundations of the authors' project design.

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The central point at issue in King and Jackson's comment has to do with our independent variable, tax instrument/GDP. Although they concede that our variable is "plausible," they prefer to decompose it into two components, tax instrument/total taxes and total taxes/GDP, thus allowing them to "distinguish countries with low tax collection levels yet high proportionate utilization of progressive tax instruments from those with high collection levels yet low progressive utilization." When they reanalyze OECD countries on this basis, they find it is the level of taxes, rather than the relative utilization of progressive tax instruments, that is associated with income equality, thus casting doubt on our "optimistic conclusion" that reliance on personal income taxes is associated with a relatively egalitarian income distribution.

In responding, we begin by looking closely at the proposed alternative variable, the tax instrument/total taxes ratio. We see this variable as a very narrow indicator of the relative tax burden across countries, and one not at all appropriate to the study of the relationship between taxes and income distribution. Only the level of a given tax instrument—tax instrument/GDP, the variable we use—can demonstrate the effects of a tax on any income group, since only this indi-

cator measures the actual amount of wealth extracted from individuals and firms, and, concomitantly, the extent of the inducement to alter their economic behavior. The alternative indicator, tax instrument/total taxes, does not really do this: across countries, all it measures is whether a particular tax mechanism is more or less prominent as a proportion of overall tax revenues. Clearly, in a country characterized by a high progressive taxes/total taxes ratio but a small government sector, taxes could have only a small impact on income distribution, because whatever the internal structure of such a country's tax system, its taxes are low in absolute terms. Conversely, in a country with a lower progressive taxes/total taxes ratio, but much higher taxes overall, the burden of progressive taxes might be greater in absolute terms than in the first country, even if such taxes did represent a smaller share of a larger tax state.

To use an admittedly extreme hypothetical example, consider a country that relied on progressive taxes for 80% of its revenues, but whose total taxes represented only 5% of GDP: its progressive taxes/total taxes ratio would be 80%. Suppose that another country relied on progressive taxes for only 25% of its revenues, but that its total tax revenues represented 40% of GDP: its progressive taxes/total taxes ratio would be only 25%. However, the amount of revenue actually raised by progressive taxes—the figure we tap—would represent only 4% of GDP in the former case, but 10% in the latter. The second figure would seem to be the only one that reflected the impact of taxes on income distribution, since it represents the actual amount of income subject to progressive taxes. Or, to use the actual examples cited in King and Jackson's comment, the tax codes of the United States, Japan, and Australia are relatively progressive internally (in the sense of the tax type/total taxes ratio), since they rely heavily on income taxes,

but the commitment to progressivity in these countries is shallow: taxes in general, including progressive ones, have a limited impact on income. By contrast, those countries with internally less progressive tax systems (in the tax type/total taxes sense) but high overall tax levels—Norway and the Netherlands in the examples given—would in fact be demonstrating a greater commitment to income distribution, at least to the extent that their progressive taxes raised greater amounts of revenue and had a greater impact on the income of the upper classes. In light of these considerations, it is not at all surprising that the tax type/total taxes and total taxes/GDP ratios are negatively related; nor is it surprising that overall tax levels, rather than tax progressivity (in the sense of the comment), are associated with relatively equal distribution. The latter finding, indeed, confirms our general conclusion that the size of the tax state, particularly that associated with personal income taxes, is associated with relative income equality.

In large measure, the argument at issue is one of semantics. This is particularly true when the comment criticizes us for focusing primarily on the level of taxes rather than on the internal progressivity of tax systems. This is indeed what we do, and our intentions on this point are very clear throughout the article. Thus, we begin our analysis by considering the level of all taxes as a proportion of GDP (Katz, Mahler, and Franz, 1983, p. 875)—what King and Jackson term "state size." We then go on to argue that this rather imprecise indicator might profitably be decomposed into its major constituent parts: taxes on income; profits and capital gains by individuals and corporations; social security contributions; property taxes; and taxes on goods and services. We find that the overall modestly positive association of all taxes/GDP and an egalitarian income distribution masks a fairly strong relationship between personal income

taxes and an egalitarian distribution, coupled with no significant relationship in either direction between a relatively egalitarian distribution and most other tax instruments. We note that this is in part a function of the fact that personal income taxes are often very prominent in the tax state of the countries in question (see Katz et al., 1983, footnote 5). Further, we emphasize that other tax mechanisms, although potentially very progressive in themselves, are simply so limited in absolute terms that they cannot be expected to have much effect: for example, estate, inheritance, and gift taxes, and taxes on net wealth (Katz et al., 1983, p. 876).

King and Jackson also question our use of the dependent variable, post-tax income shares standardized for household size, as assembled by Sawyer (1976). They regard it as "surprising" that this measure was used without reference to pre-tax income shares, and note their preference for figures based on the percentage change, pre- to post-tax, of income shares.

We would agree that this proposed alternative formula indeed assesses the distributive impact of taxes in an immediate and formal sense: the difference between pre- and post-tax income shares represents by definition the discrete effect of taxation on various groups' income shares. What this indicator does not tap, however, is any impact a particular tax may have had on the economic behavior of individuals or firms which may in turn have affected their status with respect to distribution. These wider effects of taxation, rather than immediate distributive consequences, have been at the core of the current debate on the impact of taxes on distribution and growth. Conservatives, for example, have argued that while the use of progressive taxes to finance the welfare state may be immediately redistributive, it is in the long run counter-productive. Progressive taxes, in their

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view, impose on upper income groups a disincentive to save and invest, inhibiting economic growth and thus increasing unemployment among the poor, while welfare payments create a climate of dependency and complacency that ultimately erodes the standing of low-income groups. Liberals, of course, have disagreed, arguing that the ability of the state to shelter groups from the unbridled vicissitudes of the market is a central component of sustained economic growth that extends to all income groups (see Katz et al., 1983, p. 875). These wider repercussions have been at the core of recent debates on tax policies, with much less attention devoted to the immediate impact of taxes on income distribution. Thus, when King and Jackson make the point that "post-tax income, taken by itself, is a product of much more than tax redistribution effects," we respond that our variable has advantages for this very reason.

Finally, we note that there appear to be some rather serious technical problems with constructing an indicator of pre- to post-tax percentage change in income shares from Sawyer's data, problems which would render any study based on such an indicator questionable. For one thing, Sawyer explicitly cautions that "the data which are presented cannot be used for assessing the degree of progressivity of various countries' tax systems" (1976, p. 13), and himself refrains from directly relating pre- to post-tax income share figures. This is primarily because data unavailability and other problems with the figures for a number of countries forced Sawyer to derive one of the distributions, pre- or post-tax, from the other, making an indicator derived from the difference between them problematic. A further problem is that the pre-tax figures used by King and Jackson are not adjusted for household size. As Sawyer notes, "it is well known that the income of one-person households tends to be much lower than that of larger households," with the result that a country that "has a

wide dispersion in the size of households will, *ceteris paribus*, show a larger degree of inequality" (1976, p. 18). The potential seriousness of this problem is suggested by the fact that Sawyer's adjusted and unadjusted post-tax figures are strikingly different for several countries (compare Table 3 with Table 10).²

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Notes

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2. King and Jackson report that their reanalysis—based on their alternative indicator, progressive/total taxes—was dominated by the extreme outlier, France. This problem does not, however, affect our findings. As we reported (1983, footnote 12), an examination of Cook's distances (an indicator of influential cases in regression) revealed that for none of our findings was Cook's distance statistically significant at the .10 level. For France, the lowest significance level for Cook's distance in any equation linking personal income taxes to income shares was .63.

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RESEARCH NOTES

POWER SEEKERS ARE DIFFERENT: FURTHER BIOCHEMICAL EVIDENCE

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In an article previously published in this Review I demonstrated that power-seeking, by which I mean the pursuit of social dominance, has a biochemical marker, namely, whole blood serotonin (WBS). Those individuals who are especially hard-charging and competitive have especially high WBS levels. This paper presents the results from an investigation of dynamics in the response of high WBS individuals to actual competition. My expectation—that they would exhibit special physiological activation in the face of challenge—is fully borne out by the evidence. Four hormonal indicators of activation were followed in blood samples taken during a series of social competitions. All four show distinctive patterns for the high WBS group. These results, in addition to providing new evidence on the behavior of the endocrine system in competitive settings, impressively support the view that WBS is a biological property having fundamental significance for behavioral political science.

Those human beings who gain power or dominance over others have long been of keen interest to students of political life. Yet for all of the attention, scientific knowledge about *homo politicus* remains scanty. Little is known about the inner drive of those who devote themselves to the capture and use of power roles in social arrangements.

This report comes from a research program addressing that void. In a paper published earlier (Madsen, 1985) I presented evidence that the pursuit of social dominance has a biochemical marker, namely, whole blood serotonin (WBS).¹ Those individuals who in questionnaire responses showed themselves to be especially hard-charging and competitive—"Type A," in the jargon of medical scientists—were very likely to have high WBS levels. The multiple correlation of defining questionnaire items with WBS reached .70. As noted in

that paper, this discovery echoed similar findings in a species of subhuman primates, where dominance was marked by high WBS levels (see Raleigh and McGuire, 1980; McGuire et al., 1982; McGuire et al., 1983).

It was clear from the outset that questionnaire data were less than ideal for my investigation. An obvious, and indeed crucial, concern was validation: How would high WBS individuals react to actual rather than merely remembered rivalry? That concern is addressed in the analysis reported here. In what follows, high WBS subjects are compared with *normals* in their responses to a series of competitions. The measured responses are physiological (and, it is important to note, involuntary). Four blood-borne hormones, each intimately involved in the body's preparation for action, have been followed in timed blood samples taken while the

competition unfolded. The results show plainly that high WBS subjects are distinctive in their response to this challenge. Actual competition does indeed have a remarkable effect upon these individuals.

Methods

The present research was launched for the purpose of examining the relationship between psychological stress levels and distributions of influence in small social units processing problems of collective importance. Concern with serotonin level and its implications came later. Research design and methodological details are fully described in the earlier report from this project (Madsen, 1985). However, the following points bear repeating: the experimental subjects were 72 young, healthy males; they were processed through the competition in groups of six; the competition itself consisted of a series of logic puzzles that were to be solved collectively; sessions were all given at the same time of day but on separate days; and all sessions were conducted at a university hospital with trained medical staff doing all physiological procedures.

Eleven timed samples of venous blood were collected from each subject. The initial sample was taken when a subject arrived at the hospital, about 90 minutes before the competition began. Starting about 50 minutes later (after a quiet period and a standard dinner), a sample was taken every 20 minutes until 20 minutes after the competition ended.² All samples were subdivided, frozen, and maintained at -80°C until assays of their hormonal content could be undertaken. (The methods used in all assays are available upon request.)

Recall that in keeping with the earlier finding, whole blood serotonin values are used to define power orientation. Thus, the six cases with very high WBS levels (greater than 209 ng/ml, with a mean of 251 ng/ml) were separated out as strongly

competitive, dominance-seeking individuals. They were to be compared primarily with a normals group ($N=55$; WBS from 90–200 ng/ml, with a mean of 144), but also with a low serotonin group ($N=7$; WBS from 40–82 ng/ml, with a mean of 72).

Four endocrine responses were followed, each one intimately related to the complex process through which the human body prepares itself to cope with threat or crisis. A few prefatory remarks about that process and the four hormones being followed are in order.

For human beings, the onset of stress, physical or psychological, brings forth what has aptly been called an endocrine cascade. Moreover, it is a cascade with elaborate feedback mechanisms, which in complex interaction regulate the extent of the endocrine response to that stress (Axelrod and Reisine, 1984). Prominent roles in this process are played by the hormones adrenocorticotropin (ACTH), cortisol, epinephrine, and norepinephrine.

ACTH is released from the anterior lobe of the pituitary gland, located at the base of the brain and triggered by a signal from the hypothalamus. The ACTH travels via the bloodstream to the adrenal glands (situated near the kidneys, one on each side of the body), where it causes in the outer layers of these glands (the adrenal cortex) the formation and release of a group of hormones called glucocorticoids, among them cortisol. Cortisol has a half-life of about 70 minutes. Along with the other corticoids, it exerts an important influence on metabolic processes, blood pressure, brain function, and, in general, on getting the body ready for action. This system is called the pituitary-adrenal cortical system, and its response to stress is neither as quick nor as transitory as it is that of the system to be considered next.

Also important in the human readiness reaction are the hormones epinephrine (E) and norepinephrine (NE), both classified

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as catecholamines and both a part of the sympathetic-adrenal medullary system. With the activation of this system by sympathetic nerves, E and, to a lesser degree, NE are secreted from the adrenals' inner portion, an entirely separate gland called the adrenal medulla. (NE is also secreted directly by sympathetic nerves, for which it serves as a transmitted substance.) The system responds to stress almost instantaneously, E and NE both flowing into the bloodstream. The hormones are quickly destroyed as well: the half-life for E is just over 1 minute, and that for NE about 2.5 minutes. With this quick on-off feature, the system is crucial to mobilizing the body for action in acute (as opposed to prolonged) challenge situations.

Although ACTH, cortisol, E, and NE are all involved in the stress response system, they do not all respond to an environmentally posed challenge in the same degree or at the same rate. In fact, there is still considerable uncertainty about the exact behavior of these four hormones in the face of such a challenge. That being the case, my predictions of their behavior in the present study could only be very general.

In the few relevant studies of plasma catecholamines there is evidence that psychological stress of the kind experienced when facing public speaking or mental arithmetic assignments produces a much stronger E than NE response (Dimsdale and Moss, 1980; Leblanc et al., 1979; Ward, Mefford et al., 1983) and does so especially among Type A individuals (Glass et al., 1980; Ward, Parker et al., 1983). The challenge presented by the competition designed for this study was psychological, but also was very different from the stressors used in previous work. Nonetheless, it seemed that I should find similar results if the high WBS individuals were specially activated by social rivalry. Hence, I expected to see marked elevation of E levels, and a more muted elevation of NE levels.

In contrast with the catecholamines,

ACTH and cortisol have been the object of numerous plasma studies in humans. Few of these studies, however, look at the type of competitive challenge posed here, and none look at the WBS connection. However, again there is some important work on vervets to consider. McGuire, Raleigh, and Brammer (in press) have found that in groups from which the dominant male has been removed, cortisol levels were highest for those males which eventually emerged as dominant. Recall that male dominance here is accompanied by elevated WBS levels. This study, which came to my attention after my data already had been analyzed, suggests what I had in fact expected to find in those data: that high WBS individuals would show special elevation of cortisol levels. And since ACTH is a part of the same system, I anticipated similar results for that hormone, though perhaps with an earlier peak.

Measurement of the hormonal responses to environmental stimuli in the study proved to be anything but simple. Both ACTH and cortisol are subject to sizeable daily—i.e., circadian—rhythms, with zeniths coming roughly between 0700 hours and 0900 hours and nadirs roughly between 0100 hours and 0300 hours for normal subjects who adhere to a routine of nocturnal sleep and daytime activity. What I did not know when designing my study was that in the time of day it covered, roughly from 1600 hours to 2100 hours for each session, both hormones normally are falling rather rapidly. Thus, to measure the stress effect on ACTH and cortisol in my subjects, I could not assume a stable baseline, but had to establish the baseline for my comparisons empirically. Fortunately, after another complication discussed below, that baseline could be reasonably estimated from 24-hour data on healthy subjects collected in the Clinical Research Center of the University of Iowa Hospitals and generously made available to me by its director.

There also is some preliminary evidence

of circadian rhythms in the catecholamines (Linsell et al., 1985). However, in the case of NE the evidence suggests only a slight downward trend during the period in which I took samples. And in the case of E, though the downward trend in the latter part of that period is more noticeable, the really striking finding is the presence of very substantial variance around that trend line during the waking hours. Much more work needs to be done to decipher these curious patterns. Lacking any baseline data, I could only proceed with possible circadian effects in mind.

Further complication came from the discovery of a seasonal rhythm in the plasma ACTH and cortisol for my subjects. Returning to the Clinical Research Center's data, which happened to have been collected over the course of nine months, I found and measured the effect of seasonality on this group's cortisols after the effects of age and sex, which also turn out to be influential factors, had been removed. (This finding in 24-hour plasma

cortisol data is new and will be discussed fully elsewhere.) The measured seasonality effect was then used to build a correction into the cortisol data for my subjects.

It was now possible to examine the responses of the entire set of subjects to the challenge imposed by the puzzle competition. The evidence is presented in Figure 1 for cortisol and ACTH, and in Figure 2 for E and NE. In Figure 1, the metric is percent above the healthy patients' baseline values. In Figure 2, the metric is percent above the minimum value in the 10-sample sequence. (The minimum value here may still be well above normal for the individual involved, but it is the only available estimate of such a baseline.) In these figures, and in those which follow, the plotted numbers are median values. Medians are used because the distributions are invariably skewed.

In Figure 1, note first that neither cortisol nor ACTH gets close to baseline levels in the 10-sample sequence. Cortisol does not drop below a 15% elevation;

Figure 1. ACTH and Cortisol Patterns

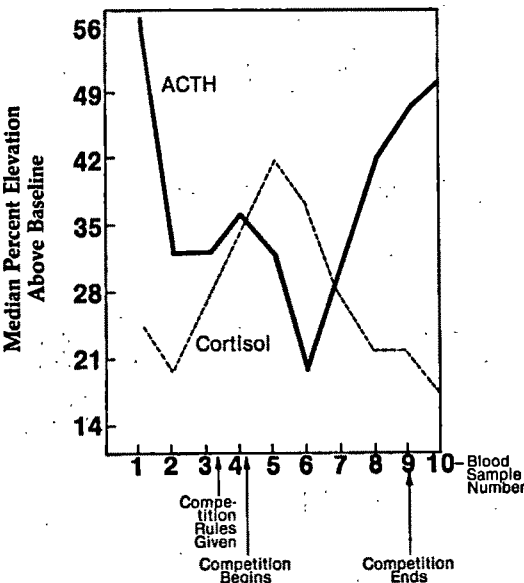
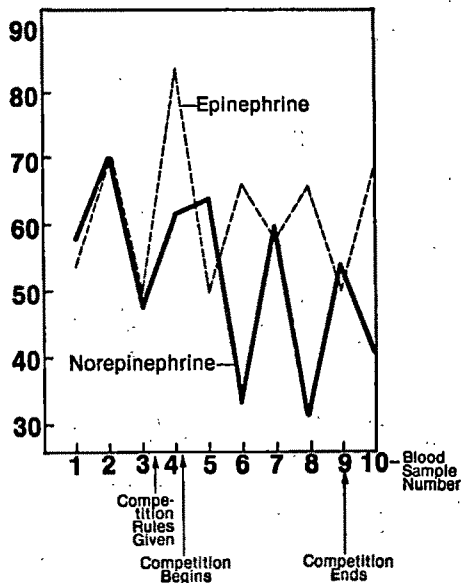


Figure 2. Epinephrine and Norepinephrine Patterns



ACTH does not drop below a 19% elevation. Even allowing for inexactness in baseline estimation, it is clear that on average these individuals were responding to the competitive context.

The contrast between the two patterns was a surprise. ACTH is highest at the time of venipuncture (when the first sample was taken)—perhaps not surprising given the fear many people have of this procedure. But median cortisol was not close to its highest point then; in fact, it was not far above its low. Cortisol climbed markedly after instructions for the competition were given, and reached its peak after the first two puzzles had been addressed. Thereafter it began a gradual fall. However, just as cortisol was falling, ACTH was climbing, ending the series not very far below the peak value registered with the first measurement. These results, almost mirror images of one another, mean that the point made earlier must be expanded. Not only may hormones in the same stress response

system respond to challenge in different degree and at different rates; they may actually respond in different directions. Obviously, my simple assumption about the ACTH pattern was in need of revision.

In Figure 2 we also find substantial elevation of endocrine values. However, there is much more jerkiness to the trend lines for the catecholamines, which perhaps is to be expected with such short-lived substances. Were lines fitted to these greatly fluctuating data points, that for NE would slope downward (perhaps a circadian decline), whereas that for E would be almost flat. But the differences between these two catecholamine responses are more dramatic than that. On the whole, one is struck by the extent to which the two are completely out of phase.

It is plain that these two figures show mysterious differences in the behaviors of the separate elements of the stress response system, but it is equally plain that all four trend lines show significant acti-

Figure 3. Cortisol Patterns for WBS Groups

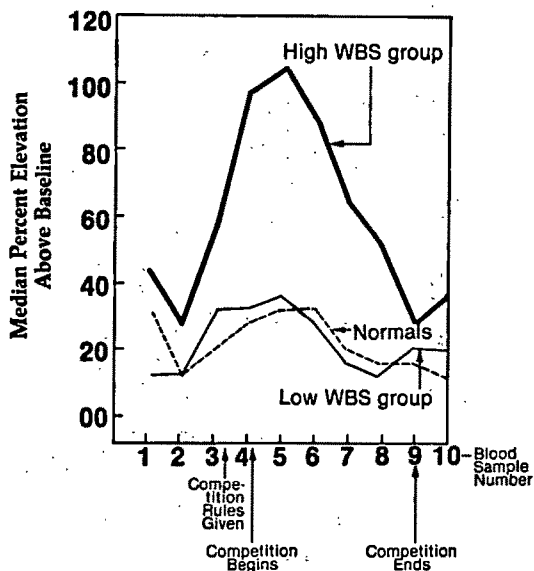


Figure 4. ACTH Patterns for WBS Groups

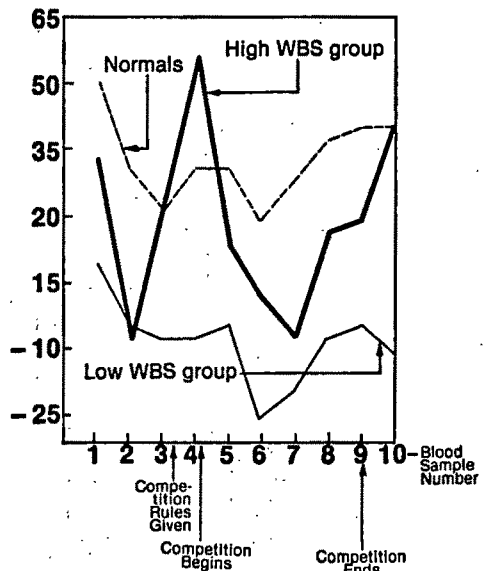


Figure 5. Epinephrine Patterns for WBS Groups

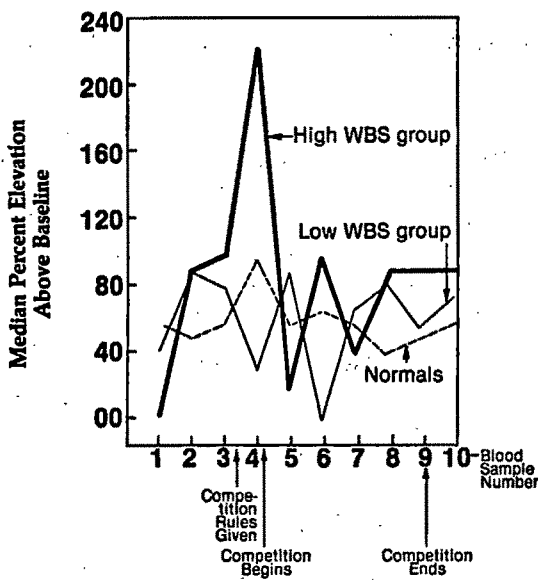


Figure 6. Smoothed Epinephrine Patterns for WBS Groups

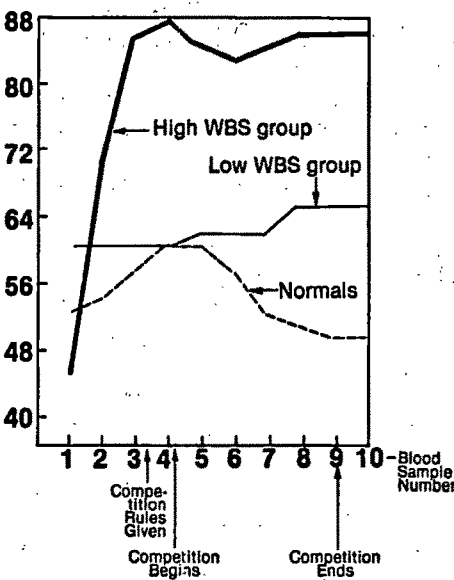


Figure 7. Norepinephrine Patterns for WBS Groups

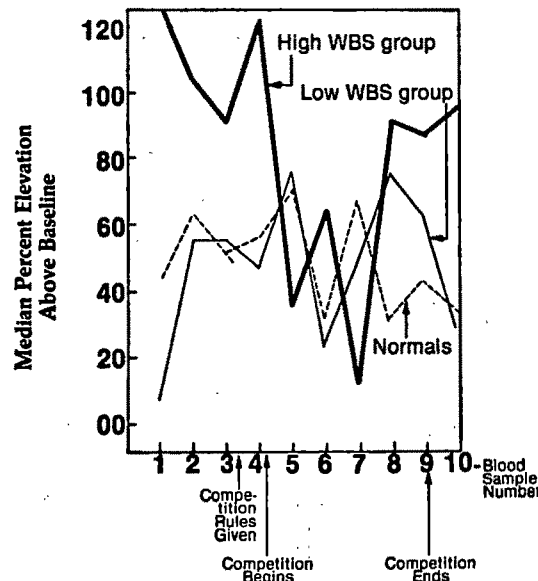
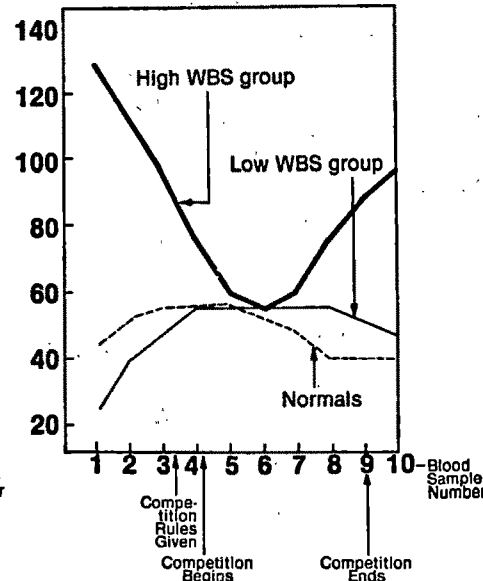


Figure 8. Smoothed Norepinephrine Patterns for WBS Groups



vation of the adrenal mechanisms. These subjects, on average, appear readied for a challenging situation. But were some more ready than others? It is to that question we now turn.

Findings

Hormonal responses for all three of the WBS groups are presented in each of the figures which follow. Figure 3 presents the cortisol series, Figure 4 the ACTH series. Figures 5 and 6 present two views of the E series, and Figures 7 and 8 two views of the NE series. Again, what is sought in these figures is evidence of a special activation for the high WBS group.

Figure 3 presents a clear and dramatic finding. The average cortisol values for the high-WBS group soar with the onset of competition. The peak, coming in the first sample taken after the competition had begun, shows just over a 100% increase. Neither the normals nor the low-WBS group show any such effect, although both show small elevations in the early stages of the competition and then a decline.

Figure 4, presenting the ACTH series, is puzzling. The high WBS subjects do not look here at all like they did in the cortisol series. In most instances their values, though above baseline, are not as high as those for the normals. What is distinctive for this group is the extraordinary variance in the series. The great peak, coming right after the nature and rules of the competition have been spelled out, is intriguing, as is the sharp drop for the main part of the competition. But while it is true that this variance is unique, the high-WBS series does have two things in common with the other series: all show at least a brief downward dip in the middle of the contest, after puzzle 4 or puzzle 6, and then all climb upward from that dip.

Figures 5 through 8, which give the catecholamine responses, must be interpreted with caution. In the E series miss-

ing data reduce the number of cases to between two and four.³ Less consequentially, the number of cases in the NE series is reduced to five. Obviously, estimates become more problematical with smaller numbers of observations.

Figure 5 presents the E series. In the clutter of trend lines, that for the high-WBS group is notable in several respects. It actually begins at a level of zero elevation, much lower than any other, then climbs to the highest of any group for measures 2 and 3. Just before the beginning of the competition there is a great spike, followed by a jerky but typically high trend line to the end of the series. Unfortunately, the spike at measure 4 is based upon only two cases. To mute the effect of this estimate and to provide a sense of the overall E pattern, I have presented a second view of the series. A robust nonlinear data smoother (running medians) developed by Velleman (1980) yields a trend line based upon averaging adjacent values with a particular measurement point. Hence, in this case it downplays a value like that in the spike just prior to the onset of the competition. The results are presented in Figure 6, where the distinctiveness of the high WBS group is again found to be dramatic.

Figure 7 presents the NE patterns. The high WBS group is special, but in a wholly unanticipated way. The initial NE value for the group is the highest of the entire series and the highest to be found anywhere in the figure, a full 129% above the minimum baseline. The series stays high through the first four measurements, again showing a spike, though much smaller, at sample 4. But then the trend line plunges to much lower values through most of the puzzle solving, before once again climbing to high levels at the end. The smoothed version of the data for all three groups is given in Figure 8. Obviously, much is lost in Figure 8, but what remains is a very interesting and distinctive pattern for the high WBS group.

Indeed, the general shape of their response curve is the opposite of that for the others, with the high start, the low middle, and the high finish. What these patterns might mean must be left to endocrinologists to explain. I can only underscore the point that the high WBS group is again found to be very different indeed.

Discussion

This study is only a beginning. Nonetheless, the findings presented here are much more than suggestive. The simple and intriguing fact is that the high serotonin individuals are indeed a special lot. A behavior pattern which had been tied to WBS only through questionnaire data has now been given striking physiological representation. The dynamics of the best understood of the plasma hormones here presented, cortisol and epinephrine, reflect for the high WBS group precisely the kind of internal response that one would expect from aggressive competitors in the context of rivalry. The patterns for ACTH and NE, while fascinating and equally distinctive, are much more mysterious.

There are some complementary studies in humans (Lewis and Sherman, 1984; Ward, Mefford et al., 1983; Ward, Parker et al., 1983) and in subhuman primates (McGuire, Raleigh, and Brammer, in press). Most intriguing is a new line of research by McGuire and his colleagues (personal communication) testing whether pharmacological enhancement of central nervous system serotonergic function raises the probability of becoming dominant. These tests have been run on groups of male vervet monkeys. After the experimenter has removed the dominant male from the group, a randomly chosen male is treated with fluoxetine, a drug which increases serotonergic function. In each of the three tests run, the treated male became dominant! (An early report

from the larger project of which this is a part is Raleigh et al., in press.)

With respect to the investigation at hand many fascinating and important questions arise. Findings inevitably lead to questions. But the crucial point here is that there indeed are strong findings. All indications are that whole blood serotonin is a biological property of fundamental significance for behavioral political science.

Notes

I am grateful for the financial support for this research provided by the Harry Frank Guggenheim Foundation, and by the General Clinical Research Program RR59 of the National Institutes of Health. Dr. Barry M. Sherman, Professor of Internal Medicine and Director of the Clinical Research Center at the University of Iowa, provided facilities, medical staff, technical support, supplemental data, and advice. Dr. Michael McGuire, Professor of Psychiatry and Director of the Biobehavioral Science Program at the University of California, Los Angeles, provided technical support and advice. I am most grateful for these invaluable contributions.

1. Serotonin, also known as 5-hydroxytryptamine (or 5-HT), is a hormone with neuroregulatory functions and is found in the central nervous system, in the blood, and in several other body locations. Blood serotonin is relatively stable, and not subject to significant seasonal or daily rhythms, or to short-term perturbations as a function of situation (for example, diet changes or social encounters). How serotonin works its effects, and exactly what those effects are, are matters of limited understanding at this point. Direct tests of its role in the dynamics of human behavior, few in number, are ambiguous in their findings. For extensive and revealing work on animal models, however, see McGuire et al. (1982, in press), McGuire et al. (1983), Raleigh and McGuire (1980), Raleigh et al. (1981), Raleigh, Brammer, and McGuire (1983), Raleigh, Brammer et al. (1983), and Raleigh et al. (in press).

2. The blood samples were taken in the following contexts: sample 1 (1600 hours) when the subject arrived at the hospital and had the catheter placed in his arm; sample 2 (1730 hours) after a quiet period and a standard dinner, followed by the move, along with five other subjects, to the experimental room; sample 3 (1750 hours) after 20 minutes of (seated) social conversation; sample 4 (1820 hours) after the rules for the competition had been given and a short paper-and-pencil questionnaire completed; sample 5 (1840 hours) after puzzles one and two had been finished by the group; sample 6 (1900 hours) after

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puzzles three and four; sample 7 (1920 hours) after puzzles five and six; sample 8 (1940 hours) after puzzles seven and eight; sample 9 (2000 hours) after puzzles nine and ten; samples 10 and 11 (2020 hours) after a second questionnaire, some general discussion, and payment for participation. Sample 11 was used for the WBS assay.

3. The number of high WBS cases for each estimate in the epinephrine series is, in order: 3, 4, 4, 2, 3, 3, 3, 3, 3, and 3.

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THE CARRYOVER EFFECT IN PRESIDENTIAL ELECTIONS

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The carryover hypothesis asserts that participants' candidate support behavior in the general election is affected by their preferences for their party's nomination. This paper examines data from activist and national sample surveys for evidence of a carryover effect in the 1980 election. There is a detectable carryover effect on voting behavior both for activists and for citizens generally. Among activists, where evidence about candidate support behaviors beyond voting is available, the effect on such activity is substantial, and increases with the effort associated with the general election activity in question.

The American presidential selection process is sequential, with the intraparty nomination campaign followed by the interparty general election. This paper examines the consequences of participation in the first stage for both candidate and party support in the second. The carryover hypothesis asserts that participants' behavior in the general election will be affected by candidate support in the nomination phase. In particular, support of candidates losing the nomination will depress general election activity below the levels expected based upon other predictors such as perceived candidate and party differentials, ideology, party identification, or past levels of activity. Sullivan (1977-1978, p. 637) stated the hypothesis succinctly: "The long pre-convention campaign can only serve to increase the psychological investment each delegate has in his/her candidate. These facts, we

think, make it even more difficult for losers to accept the convention outcome and recommit their energies to the winner."

Sullivan's view is in accord with those who see the contemporary nomination process as destructive of the parties' ability to form strong coalitions during the fall campaign (Kirkpatrick, 1976; Polsby and Wildavsky, 1980). In this view, the open character of the process heightens tensions within the parties during the nomination stage, contributing to the fragmented nature of American politics (Epstein, 1978; Wayne, 1984). This relationship between the two stages may contribute to the carryover effect described by Sullivan.

Despite this prevailing understanding of presidential nominations, studies of voting have not incorporated the carryover effect into their explanations of voting choice. Perhaps more surprisingly,

explanations of activist participation have not considered the possibility of a carry-over effect. The influence of the Downsian model of voting based upon expected party differentials is substantial (Downs, 1957). In an admirable attempt to explain party activism using the rational choice perspective, John Aldrich (1983, p. 982) explicitly states intraparty dissension is irrelevant to the decision to participate in the general election campaign. The utility-maximizing citizen during the general election, then, is one who forgets the possibly more desirable candidates who might have been nominated and makes the most of the choice he or she is offered. For activists in the context of a general election who are committed to the party and its prevailing ideology, calculating candidate or party differentials without regard for nomination preferences is presumably a relatively easy thing to do.

Data Sources and an Analytic Model

To test for a carryover effect, we require a model capable of measuring the effect of intraparty nomination preferences in the context of the interparty general election. Data from 1980 are available for a sample of Iowa party activists and for the national electorate. Delegates to the two Iowa state presidential nominating conventions were surveyed in June of 1980 before the national conventions, and contacted again immediately after the November election.¹ Similar data are available on the national electorate from National Election Study (NES) 1980 surveys. For the first time, NES conducted a study of the entire presidential campaign by surveying two independent panels at several points during the nomination and general election stages (Markus, 1982). In order to increase the number of cases, I have pooled the first waves of the major and minor samples.² These were then matched with

the postelection waves to provide information on respondents during both stages of the process.

The model permits us to observe the effects of nomination and general election preferences on participation in the November campaign. Additionally, insofar as possible, it controls for "predispositional" factors which may stimulate (or depress) an individual's activity in the general election. In its simplest form, the model may be expressed as follows:

$$Y = a + b_1N + b_2G + b_3P + e, \quad (1)$$

where Y = general election participation for presidential candidates or the parties; N = nomination candidate preference; G = general election candidate and party differentials; and P = predispositions to participate.

The dependent variables are constructed to reflect the decision to participate (or remain inactive) as well as the partisan direction of the participation. The coefficient associated with nomination preference estimates the carryover effect from the nomination to the general election stage. Measures of the predisposition to participate are included to control for the possibility that people may be drawn to participate in nomination campaigns who are not in the pool of those the winning nominee (or the party) could realistically hope to recruit for the general election campaign.

Results from the Activist Data

Because the measures available to operationalize equation (1) are richer for the activist data, I begin with a discussion of the Iowa results before moving to an analysis of the NES data.

The model for the Iowa activists is as follows:

$$Y = a + b_1N + b_2G_1 + b_3G_2 + b_4P_1 + b_5P_2 + b_6P_3 + b_7P_4 + e, \quad (2)$$

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Table 1. Nomination, General Election, and Predispositional Predictors of Activists' General Election Participation

Independent Variables	Presidential Vote	Involved in the Presidential Campaign	Presidential Activity Score	Party Activity Score
Carryover Effect				
Nomination Candidate Preference	.015	.148***	.288***	-.055
General Election Comparisons				
Candidate	.421***	.242***	.297***	.089
Party	.182***	.077	-.057	.106
Predispositions				
Ideology	.121*	.220**	.066	.142*
Past Activity	-.006	.045	.094	.187***
Party Offices	.076*	.076	.094	.066
Party Identification	.156**	.087	-.034	.356***
Adjusted R ²	.804	.654	.444	.696
N	589	599	599	573

Note: Cell entries are standardized regression coefficients.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

Source: Author's surveys.

where

Y = general election participation, including voting, getting involved in the presidential campaign, and engaging in any of several intensive activities on behalf of the party's nominee. I also include a measure of intensive activity for the party. All are coded to reflect whether the respondent participated, and the partisan direction of the participation (second wave).³

N = prenomination candidate preference (first wave).

G_1 = general election candidate preference composed of relative ideological proximity to the Democratic and Republican candidates, and the relative evaluation of the two candidates (second wave).

G_2 = general election party preference composed of relative ideological

proximity to the parties, and evaluation of the parties (second wave).

P_1 = self-placement on the liberal-conservative continuum (first wave).

P_2 = level of activity in past campaigns (first wave).

P_3 = number of official party offices held by the activist (first wave).

P_4 = strength and direction of party identification (first wave).

I have noted whether the variables were measured in the prenomination wave of the survey (first wave) or after the general election (second wave).⁴

Table 1 exhibits the analysis of three indicators of participation in the presidential campaign, plus a measure of party activity. I present the presidential support measures in rough order of the effort they required of the activist. Voting is the least demanding form of participation. The

second is a response to a general question in the postelection survey asking whether the respondent was "involved" in any way in the fall presidential campaign. The activity scores for the presidential nominee and for the party are composed of identical items asking about relatively demanding forms of participation, including clerical support work, canvassing, and other organizational work.

The carryover hypothesis receives mixed support in Table 1. Activists' nomination preferences had no independent effect on voting participation. Indeed, activists were most affected in their voting by their comparisons of the two parties' nominees. Of somewhat less importance were comparisons of the two parties during the general election period, party identification, and ideology. Supporters of nomination losers were apparently not dissuaded from voting for their party's nominee.

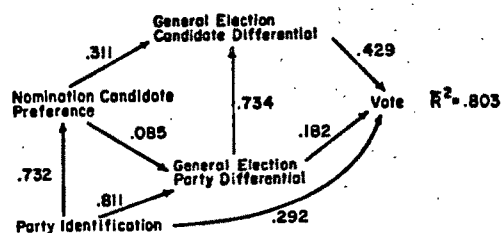
The same cannot be said, however, for more active involvement in support of the first round winners. Nomination candidate preference had a significant effect on both the general involvement indicator and on organizational activity for the presidential nominees. Indeed, the results suggest that the more demanding the form of support for the presidential candidate, the greater the carryover effect of nomination preferences. The psychological investment demanded by the first, intra-party stage of the process may be easier to overcome in the case of a relatively "easy" act like voting. But becoming more actively involved, especially participating in the candidate's organization or campaigning face-to-face for a candidate who was the opposition in the first round, may be psychologically very difficult.⁵ The relative effect of the general election comparison between the two major party nominees accordingly becomes weaker—and the prenomination commitment to a losing candidate more important—in determining whether an activist

becomes involved. Thus, the expectation of a significant carryover effect from the first stage holds up for participation beyond voting.

Interestingly, there is no evidence that party activity of exactly the same sort as measured by the presidential activity score is affected by nomination preferences. Neither do the general election candidate or party comparisons affect activists' willingness to work for their party. Rather, the predispositional variables, chiefly partisan identification and past levels of activity, explain participation for the parties.

The analysis in Table 1 may underestimate the carryover effect, since candidate preference during the nomination stage may affect general election candidate and party comparisons, which in turn have an impact on participation in the second stage. The path analysis of vote choice presented in Figure 1 provides a way of testing for the total effect of preference for the nomination candidate as it worked indirectly through general election candidate and party comparisons. The model shows that nomination preference had a weak (but significant) effect on the general election comparison between the parties, and a stronger impact on the comparison between Carter and Reagan. Supporting the loser during the nomination campaign influenced activists to be less favorable toward the party's nominee in the personal and ideological compari-

Figure 1. Path Analysis of Activists' General Election Voting



Source: Author's surveys.

1986 Carryover Effect in Presidential Elections

Table 2. Compound Path Analysis of Activists' General Election Presidential Support

Independent Variables	Presidential Vote			Involved in the Presidential Campaign			Presidential Activity Score		
	Direct Effect	Indirect Effect	Total	Direct Effect	Indirect Effect	Total	Direct Effect	Indirect Effect	Total
Nomination Candidate Preference	—	.175	.175	.197	.096	.293	.317	.109	.426
General Election Candidate Differential	.429	—	.429	.256	—	.256	.293	—	.293
General Election Party Differential	.182	.315	.497	—	.187	.187	—	.214	.214

Note: The estimated path model is presented in Figure 1.

Source: Author's surveys.

sons with his opponent during the second stage.

In Table 2 I report a compound path analysis of the effects of nomination preference and general election candidate and party comparisons on the three measures of general election participation for the presidential candidates. The models estimated for involvement beyond voting and organizational activity for the candidate are identical to that shown for voting. The carryover effect exceeds the total effects of either the general election candidate or party comparisons for both measures of activity beyond voting, when we take into account the indirect effect of nomination preference. Of course, the general election candidate and party comparisons together remain stronger in their effects on campaign activity than the carryover effect from the nomination stage.

In sum, the analysis in Figure 1 and Table 2 demonstrates that nomination preference affected general election participation from voting through more active forms of involvement by affecting activists' general election calculus. The carryover link between nomination preferences and general election candidate support is upheld, whereas a simple expectation that participants will base their general election activity upon compari-

sons of candidate and party differentials requires modification. The activists in our sample come closest to a model of choice based only on general election alternatives in their voting behavior; but as the costs of a mode of participation in the second stage go up, the commitment activists made during the first stage affects to an increasing extent their willingness to participate for the nominee. These findings suggest that rational choice models such as Aldrich's (1983) should find a way to incorporate intraparty conflict if they are to comprehend party activism in the general election.

A Partial Replication with National Election Study Data

The model employed to estimate the carryover effect from the National Election Study data is similar to equation (2), but the selection of variables is more impoverished. We have but two dependent measures: the vote and a general indicator of campaign involvement.⁶ The independent variables include nomination preference and a general election candidate comparison as the basis for comparing the effects of the nomination with the general election stage. The "predispositional" variables include ideology, how often the

Table 3. Nomination, General Election, and Predispositional Predictors of General Election Participation

Independent Variables	Presidential Vote	Involved in Campaign Beyond Voting
Carryover Effect		
Nomination Candidate Preference	.043	.034
General Election		
Candidate Differential	.648***	.516***
Predispositions		
Ideology	.053	.069
Past Voting Activity	.107*	.129*
Education	.109*	.192**
Political Interest	.030	.124*
Party Identification	.129*	.105
Adjusted R ²	.621	.410
N	774	772

Note: Cell entries are standardized regression coefficients.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

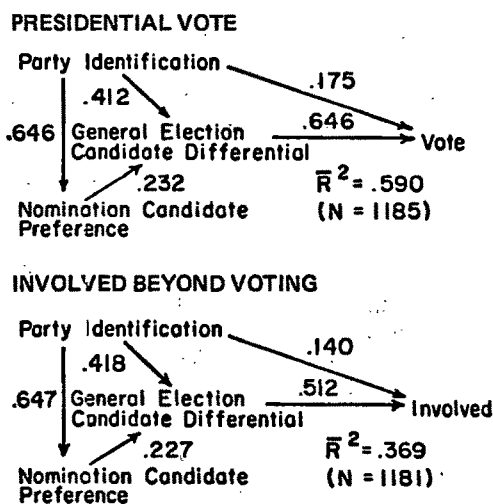
Source: National Election Study 1980 survey.

respondent reported voting in past elections, education, interest in politics, and party identification.⁷

Table 3 presents the results from the full model, and Figure 2 depicts the path analysis from a simplified version designed to test for an indirect effect of nomination preference on general election participation. The findings with respect to voting are very similar to those reported from the Iowa activist surveys. There is no evidence of a direct effect of nomination preference on November voting in Table 3, but Figure 2 shows that first-round candidate preference had a modest impact on the comparisons citizens made between general election nominees. Thus, a compound path analysis reveals a weak total effect of nomination preferences on voting in the general election of .150. The findings pertaining to involvement most closely parallel those for general party activity among Iowa activists. Table 3 shows no evidence of a direct carryover effect, although again an indirect effect of

nomination preference is present in Figure 2, resulting in a weak total effect of .116.

Figure 2. Path Analysis of General Election Participation



Source: National Election Study 1980 survey.

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Conclusion

That there is evidence of a carryover effect, however slight, on mass participation in the general election is consistent with the expectation that contemporary nomination campaigns intrude on the interparty fall campaign, even for the ordinary citizen. Among more partisan and committed activists, there is clear evidence of a stronger carryover effect, especially for presidential campaign activities of the sort activists uniquely contribute. It is this sort of support candidates can ill afford to lose, since the secondary and tertiary effects may translate into a significant number of lost votes. In years when the opposition has no nomination fight, these losses could spell defeat. That the carryover effect does not seem to contaminate activity for the party ticket generally should help calm the fears of those who see presidential nomination races as sapping the vitality of the party organizations.

Appendix

The response rate to the Iowa activist survey was about 58% to the prenomination wave, and 57% to the postelection wave. Comparisons between the first and second waves show that nonresponse to the postelection survey created no serious differences (details are available from the author on request).

The questions used to estimate equation (2) are presented below:

Presidential vote (second wave): "How did you vote in the 1980 presidential election?" Response categories were "Reagan," "Carter," "Anderson," "Other," and "Didn't vote."

Involvement in the presidential campaign (second wave): "Please indicate which of the following general election campaigns you were actively involved in during the fall of 1980 (check as many as

apply)." Responses analyzed in this paper included those who checked "Democratic Candidate" or "Republican Candidate" opposite "Presidential Campaign."

Presidential and party activity scores (second wave): "Which of the following activities, if any, did you perform on behalf of the campaigns listed below, between the summer conventions and the November election?" Activities respondents could check were "clerical work," "door-to-door canvassing," "telephone canvassing," "arranging coffees, socials," "fundraising," "writing ads, press releases," and "planning strategy." For each activity, respondents could check boxes under the headings (for Democrats) "For Carter" and "For the Ticket," or (for Republicans) "For Reagan" and "For the Ticket."

Nomination candidate preference (first wave): "Please rank your preferences for President among the following candidates from (1) most favored, to (6) least favored." If a Democrat ranked Carter higher than Kennedy, he or she was scored as favoring Carter, and so forth.

G₁, G₂ (first wave): Relative ideological proximity to the two parties' nominees was constructed from an ideological self-placement question ("How would you describe your own political philosophy?" Respondents were given a five-point scale ranging from "Very Liberal" through "Very Conservative") and a perception question ("How would you describe the political philosophy of each of the following individuals and groups?" The same five-point response scale was used for this item, and respondents placed several leading political figures and the political parties). Respondents rated political leaders and the parties in response to the following question: "Please indicate your opinion of each of the following individuals and groups." Responses ranged on a five-point scale from "Very Favorable" through "Very Unfavorable." The appropriate ideological proximity and ideo-

logical proximity scores were combined in additive indexes to create G_1 and G_2 .

P_1 (first wave): "How would you describe your own political philosophy?" Answers ranged on the same five-point scale described above.

P_2 (first wave): "Which of the following activities, if any, have you performed in political campaigns?" An additive index was created out of the activities identical to those used to build the presidential activity score from the second wave (see above).

P_3 (first wave): "Please indicate which, if any, of the following positions you now hold or have held in the past." Response categories included local party committee (member or chair), other local party office, congressional district party committee, and state central committee membership.

P_4 (first wave): "How would you describe your own party affiliation?" Responses ranged on a seven-point scale from "Strong Democrat" through "Strong Republican."

Notes

An earlier version of this paper was presented at the 1983 Annual Meeting of the American Political Science Association. I am grateful to Charles N. Brasher for his outstanding work as a research assistant. Generous grants from Grinnell College supported the activist surveys, and the Inter-University Consortium for Political and Social Research provided data on the national electorate (Study Number 7763). None of the above is responsible for any errors of analysis or interpretation.

1. The details of the Iowa survey are reported in the Appendix.

2. Respondents from the first "P" wave were combined with those from the first wave of the "C" panel to form a pooled preconvention sample. The post-election waves from both panels were likewise pooled to match the nomination samples.

3. Nonparticipation is coded as a zero; a negative score is support of Reagan (or the Republican Party); a positive score reflects support of Carter (or the Democratic Party). In the case of voting, a zero means either nonvoting or a vote for John Anderson.

4. The independent variables are coded so that a negative score is a pro-Republican (or pro-Reagan)

score; a positive score is pro-Democratic (or pro-Carter); and a zero indicates indifference or absence of the variable in question. For prenomination candidate preference, a zero indicates the respondent preferred a candidate other than the party's nominee, a -1 is a nomination preference for Reagan, and a 1 is a preference for Carter.

5. I assume that becoming active in a general election campaign depends wholly upon the individual's willingness to participate. This is reasonable, since campaign organizations are open and permeable, and because the nominee has every incentive to include as many participants as are potentially available.

6. The NES measures of campaign involvement beyond voting do not identify the candidate on whose behalf the individual was active. I assume the activities were in support of the party for which the respondent voted at the presidential level, but we cannot be certain the individual was active in the presidential campaign. Activities were talking with others to influence their vote, attending meetings or rallies, working for a party or candidate, and belonging to a political club. As with the Iowa data, the variables are coded to reflect partisan direction of activity (negative is pro-Reagan or pro-Republican, positive is pro-Democratic or pro-Carter), and a zero indicates no activity (or a vote for Anderson).

7. I employ candidate preference rather than primary vote as the measure of nomination preference. Among those voting in a presidential primary, the correlation between the first choice variable and reported primary vote is substantial ($r = .81$). Using the primary vote, however, does reduce the N substantially. The general election candidate differential is a comparison of the candidates' scores on the thermometer questions. Because the nomination is a partisan process, and to promote equivalence with the activist data, I analyze only party identifiers (including independent leaners).

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PRESIDENTIAL ELECTION COALITIONS IN 1984

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This report provides an update of major characteristics of the Democratic and Republican party coalitions for the recent U.S. presidential election. Earlier reports published in this Review have provided this kind of analysis from 1952 to 1980.

The 1984 presidential election presents the occasion to conduct an analysis of electoral coalitions again, as has been done for these elections beginning in 1952 (Axelrod, 1970, 1974, 1978, 1982). As before, the analysis requires an iterative procedure to correct for the overreporting of the turnout and the vote for the winner.¹

The coalitions are described in terms of the three components of a group's contribution to a party; namely, the size of the group; its turnout, and its loyalty to the given party. The contribution of a group to the electoral coalition of a party is the proportion of all the party's votes that come from members of that group. The loyalty of a group to a given party is simply the proportion of the votes of that group which are cast for that party.

The 1984 election was a landslide. The Democrats received only 40.5% of the popular vote, their second lowest percentage since 1952. The Republicans received 58.8%, up just over 8% from their bare absolute majority in the three-way race of 1980. The turnout for the 1984 election was a mere 53% of those

over 18 years old, consistent with the poor turnouts of the last dozen years (Table 1, column 25).

What happened to the "traditional" coalitions of the two major parties? Let us look first at the Democrats (Table 1). The Democrats are often thought of as a coalition of diverse overlapping minorities: the poor,² blacks (and other nonwhites), union families, Catholics and Jews, Southerners, and city dwellers. As in 1980, the Democrats did well with three of these groups: the poor, blacks, and city dwellers (cols. 19, 20, and 24). These three groups were each at least 25% more loyal to the Democrats than was the country as a whole (cols. 27, 28, and 32). But these three groups were relatively small, each containing no more than 12% of the population (cols. 7, 8, and 12). Moreover, the poor and the blacks had low turnout (cols. 13 and 14). Once again, as in 1980, the Democrats did not do nearly as well with the larger groups in their traditional coalition: the union families, the non-Protestants, and the Southerners (cols. 21-23). These groups were only 3% to 14% more supportive of the Democrats

than was the nation as a whole (cols. 29–31).³ The Democrats did a little better with union families and non-Protestants after their abysmal performance in 1980 (cols. 21–22) but Mondale's attractiveness to the poor, blacks, and Southerners was not quite as strong as Carter's (cols. 19, 20, and 23).

The Republicans in 1984 once again attracted a majority of all their traditional groups: the nonpoor, whites, nonunion families, Protestants, Northerners, and those outside the central cities (Table 2, cols. 19–24). In fact, the Republicans got at least 60% of each of these traditional groups. Moreover, their effectiveness was magnified by their very large size (cols. 7–12).

In 1984, as in 1980, there was a noticeable difference in how women and men voted. This time women voted 44% Democratic, while men voted only 36% Democratic. Since women were 56% of the sample and had the same turnout as men, they provided 61% of the Democratic votes.⁴

The basic structure of party coalitions is weakened but still recognizable: both major parties still attract a disproportionate share of all six of their traditional groups. The Republicans have now won four of the last five presidential elections, and are finally beginning to close the sizable gap in party identification that the Democrats have always enjoyed. The number identifying themselves as Democrats is still 37% (down 4% from 1980), compared to 28% who identify themselves as Republicans (up 4%).⁵ Since the Republicans attracted 59% of those under 30 years of age, this trend might well continue. In short, the advantages for the Republicans and the problems of the Democrats are deeper than the personalities of their respective candidates in 1984, and are reflections of basic trends cutting

across the traditional cleavages in the American population.

Notes

I thank Louis Erste, my research assistant. I also thank the Institute of Public Policy Studies of the University of Michigan for financial support, and the Inter-University Consortium for Political and Social Research for making the survey data available.

1. The survey material is again from the Survey Research Center of the University of Michigan; this time the total sample size was 5,753, and the post-election sample size was 2,247. All definitions and procedures used here are as described in the original article, except that for greater accuracy the 1980 and 1984 contributions have been calculated directly from the adjusted vote rather than from the formula at the top of Table 1. Several minor corrections have also been made in earlier figures.

2. The definition of "poor" is a family whose income is under \$5,000 per year since 1980, or \$3,000 per year before then. The categories of the survey do not allow a finer gradation of income to reflect inflation. The amount of inflation is indicated by the Consumer Price Index, with 1967 = 100. The Index was 125 in 1972, 170 in 1976, 247 in 1980, and 316 in 1984.

3. This survey data apparently exaggerates the Democratic vote in the South, including the border states. The election returns show only 38% of the vote there went Democratic which is even less than the national average. I thank Paul Abramson for pointing this out.

4. Census figures indicate that women were actually 53% of the voting age population. If so, their contribution to Democrats would have been 58%.

5. There are also 30% independents (up 5%), and 5% with no preference (down 6%).

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REVIEW ESSAY

Antonio Gramsci: Cronache torinesi (1913-1917).

Antonio Gramsci: La città futura (1917-1918).

Antonio Gramsci: Il nostro Marx (1918-1919).

Edited by Sergio Caprioglio. (Turin, Italy: Giulio Einaudi editore, 1980, 1982, 1984. Pp. xii + 898; 1033; and 707. L. 35,000; L. 35,000; L. 40,000.)

The Two Revolutions: Antonio Gramsci and the Dilemmas of Western Marxism. By Carl Boggs. (Boston: South End Press, 1984. Pp. xii + 311. \$9.50, paper.)

The Gramscian Challenge: Coercion and Consent in Marxist Political Theory. By John Hoffman. (Oxford: Basil Blackwell, 1984. Pp. xii + 230. \$27.95.)

Le Strategie del potere in Gramsci: Tra fascismo e socialismo in un solo paese, 1923-1926. By Leonardo Paggi. (Rome: Editori Riuniti, 1984. Pp. xxxv + 504. L. 30,000.)

Antonio Gramsci, Selections from the Cultural Writings. Edited by David Forgacs and Geoffrey Nowell-Smith. Translated by William Boelhower. (London: Lawrence and Wishart, 1985. £15.00.)

Lingua, intellettuali, egemonia in Gramsci. By Franco Lo Piparo. (Rome-Bari: Laterza, 1979. Pp. xviii + 292. L. 11,000, paper.)

As a political thinker and leader, Antonio Gramsci (1891-1937) defied imprisonment in any intellectual system. Instead, he produced his own theory of politics. His aversion to established intellectual schemes helped him develop the qualities of political theory at its best, including openness to new developments, disinterestedness, the ability to distinguish the essential from the accidental, a determination to speak truth to power, and the discernment of the difference between a paradigm and a utopia.

Gramsci called himself a "critical communist." Today, the idea that one can be critical and a communist at the same time seems out-

rageous to the powerful in Washington and in the Kremlin. Nonetheless, a critical communist is precisely what Antonio Gramsci claimed to be.

The subject of hundreds of books and thousands of articles, Gramsci has been called everything from a democrat to a Stalinist. However, prospects for understanding Gramsci on his own terms have improved significantly with the recent publication of the first three volumes of his early writings. These volumes, edited by Sergio Caprioglio, constitute the first major breakthrough in Gramscian studies since Valentino Gerratana's critical edition of the *Prison Notebooks* (*Quaderni del carcere*) in 1976.

The scholarly world owes Caprioglio an immense debt. He has convincingly established scores of new attributions, filled in most of the articles partially or totally deleted by Italy's wartime censorship office, cast out some spurious articles, and provided copious annotations. These three volumes—*Cronache torinese*, *La città futura*, and *Il nostro Marx*—covering the period from February, 1913 to May 1, 1919, deserve prompt translation. Caprioglio plans to produce five more volumes of Gramsci's pre-prison writings.

Caprioglio breaks new ground because his first three volumes demolish the widely held assumption that Gramsci wrote nothing of consequence before the factory council movement. The outlines of the arguments Gramsci advanced in the *Prison Notebooks* are visible in the early articles. The rejection of deterministic Marxism, ridicule of intellectual reductionism, advocacy of non-authoritarian leadership styles, condemnation of "state-worship," contempt for fascist "gladiatorial politics," insistence on the "educative" role of the party, commitment to internationalism, opposition to the working class's acceptance of capitalist "philanthropy," insistence on linking party policy with effective social forces, disgust with "mandarin" bureaucrats in the party

and the unions—these and other themes prominent in the “mature” Gramsci’s *Prison Notebooks* are anticipated in the “young” Gramsci’s journalism. Thus, the thesis that Gramsci’s thought shows sharp “breaks,” moving from an idealistic and utopian “youth” (1914–1919), to an experiment with the factory councils (1919–1920), to an acceptance of the “Bolshevik” idea of the communist party (1921–1926), and ultimately to a self-consciously theoretical view of politics (1926–1937) is unlikely to survive close inspection of Gramsci’s early journalism.

The most prominent quality of Gramsci’s mind and spirit was empathy for those at the “margins” of social life. Gramsci was the first important Marxist to look at the political world from the perspective of the “backward” periphery instead of from the “advanced” center. Gramsci combined empathy for the *emarginati* (those kept beyond the margins of mainstream society) with Marx’s concept of the class struggle.

Many of the articles and reviews in Caprioglio’s three volumes discuss specific injustices committed by those with power and prestige against “the other class,” or those beyond the periphery. Accounts of shepherds and peasants who could not afford to buy shoes because protective tariffs from the mainland kept the price of leather artificially high show vividly the economic oppression of Gramsci’s native Sardinia. Instead of presenting impersonal statistics, Gramsci evoked the image of innumerable Sardinian men, women, and children having to walk barefoot—summer and winter—on the dried-up riverbeds that substituted for roads. In other articles Gramsci took the side of three provincial women, seeking work in Turin, falsely arrested for prostitution; of an artisan who had been dismissed for the “crime” of arguing with his private employer who had accused the socialist party of being in the pay of the Germans; of some ex-prisoners of war who returned to Turin to find themselves vilified as “cowards”; and of a poor worker killed by a policeman after a nonviolent demonstration against Italy’s participation in World War I.

Despite the paucity of references to Benedetto Croce in Gramsci’s early writings, critics have claimed that Croce influenced Gramsci’s anti-economistic version of Marxism. More likely, Gramsci rejected the positivistic and

materialistic Marxism of the Second International because he was repelled by its coldness and impersonality and its tendency to create a caste of scientific “experts” who interpreted Marx and Engels literally. Gramsci’s early articles leave little doubt that the roots of his political theory lay in his early personal experiences. Philosophical idealism appealed to him during his university years because it made room for empathy and the subjective element. However, Gramsci did not become a philosophical idealist; he used philosophical idealism to explore his personal experience.

The same can be said for Marxism. Gramsci used Marx’s insights to develop a “critical communism.” He rejected completely any notion that Marxism comprised an exact “science” of “iron laws.”

There can be nothing in common between critical communism and positivism [Gramsci wrote in October, 1918]. That Marx introduced positivistic elements into his work comes as no surprise. Marx was not a professional philosopher and sometimes [like Homer] he nodded. What is certain is that the essential part of his doctrine has its basis in philosophical idealism. . . . One need only think of the frequent use socialists make of the word ‘consciousness,’ as in ‘class consciousness’ and ‘proletarian and socialist consciousness’; the conception that one ‘is’ when one ‘knows oneself,’ or is conscious of one’s own being, is implicit in this language: a worker ‘is’ a member of the proletariat when he ‘knows’ himself to be such and works and thinks according to this ‘knowledge.’ (*Il nostro Marx*, pp. 348–349)

To Gramsci, “Marxism” is not Holy Writ. Marx was neither a messiah nor a shepherd of mindless sheep. As a social analyst, Gramsci wanted the working class to decide how to use his methods of research in responding to changing situations. Marxism was not a code to be enforced from the outside by experts. Despite occasional “incrustations” of positivism, making him sound like a proponent of iron economic laws, Gramsci contended that Marx was a fighter with a simple message: “Proletarians Unite!”

If Carl Boggs had better understood Gramsci’s aversion to system-building, he would have spared his readers references to the allegedly “Hegelian-Crocean predisposition” of Gramsci’s “early period,” to the “Leninist orthodoxy” of the 1924–26 years, and to Gramsci’s effort to “transcend the extreme

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polarities of Luxemburgian spontaneism and Leninist Jacobinism," in the prison writings. We are told that ultimately Gramsci "outgrew Lenin." However, according to Boggs, Gramsci's final teaching incongruously mixed Marx, Machiavelli, Croce, Labriola, Sorel, and Lenin. Nonetheless, although "fragmentary and incomplete," Gramsci left behind "a reconstituted vision of revolutionary change that at once transcended the outlook of the classical Marxists, the spontaneists, and the Jacobin vanguardists," Boggs concludes.

Even in these brief quotations, Boggs reveals his passion for putting Gramsci in boxes. Indeed, what troubles him most is Gramsci's refusal to remain in any of them. Boggs's picture of Gramsci shows him lurching one way and then another, as if trying to touch all bases at once. We are told that Gramsci left a "conflicted legacy," that he could never quite make up his mind whether he favored force or consent, centralization or decentralization, participation by the masses or elite guidance by the party ("vanguardism"), and so on down Boggs's line of jawbreaking antinomies. Then there is the parade of "influences" of early, middle, or late Lenin, of Sorel, of the Machiavelli of the *Prince* (no mention of the *Discourses*), and on and on. Every page illuminates a new "influence."

One may wonder whether other thinkers fit into Boggs's boxes, and why he puts them there in the first place. The answer may lie in Boggs's esteem for system-building. Boggs reports that "bold but not very systematic efforts to break through the barriers of [Marxist] orthodoxy were made by [Antonio] Labriola, Gustav Landauer, Luxemburg, and, on the fringes of Marxism, by Sorel and the Council Communists . . . to seriously address the issue of consciousness."

According to Boggs, in Gramsci's early writings one "searches in vain for any treatment of the state, political power, [or] of the role of the party (not to mention unions and councils. . ." [p. 60]). Apparently he has not searched far enough, for on p. 30, footnote four, he declares that "the bulk" of Gramsci's writings for the period 1916 to 1919 "is contained in Paolo Spriano, ed., *Scritti politici*, published by Riuniti in 1967." Why did Boggs rely on Spriano's 1967 anthology when he could have used the first two of Caprioglio's volumes (published in 1980 and 1982, respectively) plus

other published sources to see at once that Spriano's anthology contains only a small fraction of Gramsci's journalistic output for the period described? Caprioglio's research has demonstrated that between January 1916, and May 1, 1919, Antonio Gramsci wrote 853 articles and 188 reviews of plays, covering a total of 2,543 pages of text. Spriano on the other hand reprinted only 57 articles covering 190 pages. If Boggs had examined Gramsci's entire output for the period, he would have discovered in it quite a bit about "the state," "political power," the "role of the party," and maybe even "unions and councils."

In *The Gramscian Challenge* John Hoffman relies exclusively on English translations of Gramsci, thereby placing himself at a disadvantage. Only about one third of Gramsci's writings have been translated into English. According to Hoffman, neither Marx nor Gramsci has properly distinguished and integrated the moments of force and consent into an adequate theory of politics. Marx synthesized force and consent without analyzing either term. Gramsci did the reverse, analyzing force and consent without synthesizing them.

Hoffman's book at times reads like a tract for the converted. The non-Marxist reader may feel like an intruder on a family quarrel. Although in a sense refreshingly polemical, Hoffman at times resorts to incomplete sentences and exclamation points to sustain reader interest. Nonetheless, in criticizing the artificiality of some dichotomies in the literature about Gramsci, Hoffman performs a service.

Hoffman contends that Gramsci identified vulgar materialism with materialism as such, that he had a "mechanistic" and "abstract" view of nature, and that "it is only toward the end of the factory occupations (Spring of 1920) that Gramsci accepts the need for a revolutionary party" (p. 53). I think the first two statements are wrong, and I know the third one is, because the factory occupations occurred in September, 1920, and as early as May 18, 1918. Gramsci wrote that a revolutionary party, "morphologically distinct from any other party," was indispensable to the revolution against capitalism. "The socialist party," wrote Gramsci, "does not conquer the state—it replaces it. . ." (*Il nostro Marx*, pp. 29, 30).

The pages devoted to Stalin (pp. 186-98) would have given George Orwell a field day. Hoffman defines Stalinism as "a proletarian

state form which combines exceptional violence with exceptional legitimacy" (p. 198). There is something terribly wrong with an approach producing such euphemisms about Stalin, whom Gramsci diagnosed as a practitioner of "statolatry."

Leonardo Paggi's book on Gramsci's political thought and practice from 1923 to 1926 is of major importance. Scrupulously and exhaustively documented, *Le strategie del potere in Gramsci* begins with Gramsci in Moscow, where he served as Italian representative on the Third Communist International's Enlarged Executive. Returning to Italy in May, 1924, Gramsci wrested the leadership of the communist party from Amadeo Bordiga and charted a "new course" aimed at reorganizing the party for closer relationships with ordinary working people. Gramsci opened the party ranks to new elements. Rather than requiring party members to pass a litmus test of loyalty to a doctrinaire version of Marxist-Leninism, Gramsci, elected secretary-general in August, 1924, sought to develop the critical capacities of all party members in various ways. He also sought to bring the Italian party into line with the Comintern's "united front" policy, which Bordiga had fiercely resisted. By "Bolshevizing" the Italian communist party, Gramsci wanted to rescue it from the organizational shambles into which it had fallen under Bordiga. Gramsci never expelled a single individual, and the sinister associations of the word "Bolshevization" should not cloud our appreciation of Gramsci's role as party secretary.

Paggi argues that Gramsci's famed idea of "hegemony" originated in the context of practical political activity. Hegemony was not an academic theory but a strategic doctrine. As Gramsci affirmed in 1924 at Como, "our party must become the party of the greater masses of Italians; it must become the party that realizes the hegemony of the proletariat in the larger framework of alliances between the working class and the peasant masses" (quoted in Paggi, p. 207). If, as Lenin had said, conditions in Russia in 1917 were unique and "unrepeatable," western Europe needed a new strategy for revolution. Paggi documents the fact that, well before he elaborated the idea of hegemony in his *Prison Notebooks*, Antonio Gramsci had worked out its essentials in Vienna in the winter of 1924. As a strategy, revolution must

first establish a new moral, intellectual, and cultural leadership to correspond with the increased economic power of the proletariat and the peasantry, and it entailed a politics of alliances with social forces outside the communist party itself. Paggi brings out the originality and subtlety of Gramsci's strategy which differed from Bordiga's in many respects, chiefly that the party could not just sit around and wait for the revolution to happen. Paggi argues convincingly that Gramsci's call in the prison writings for the precedence of a "war of position" over a "war of maneuver" in Italy and western Europe was anticipated in the strategy he had followed as leader of the Italian communist party.

Gramsci scholars will wish to consult Paggi's book, even though it contains major errors of judgment. Paggi does not understand that Gramsci's vision of a new politics directed toward including those groups and social forces hitherto at the margins of society enabled him to transcend traditional dichotomies. Gramsci did not have to argue "consistently" in favor of either Schumpeter's or Rousseau's kind of democracy, or of either Lenin's or Luxemburg's theory of the party. When will even the better Gramscian scholars cease trying to impose their own procrustean categories on Gramsci and realize that Gramsci's thinking was alive and contextual? Gramsci did not fabricate abstract intellectual schemes in advance. He immersed himself in the details of the everyday struggle to check the expansion of Mussolini's dictatorship. He made painful choices about whom he could and could not trust as allies. He did not expound a literal theory, valid for all times and occasions, about the role of the party in the revolution, or about "force," "consent," "power," or any other abstraction. The point is that Antonio Gramsci deliberately broke with the prevailing way of doing political theory in our century. The best means of understanding his new way of theorizing is by attempting as best one can to put oneself in Gramsci's place, by empathizing with his perspective of politics as seen from society's periphery. One is unlikely to understand Gramsci by grading his ideas according to one's own idea of consistency.

Paggi's eagerness to judge Gramsci according to a system-builder's criteria leads him to overlook the basic continuities in Gramsci's

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life. Thus, Paggi discerns an "idealist" and an "activist" approach in Gramsci's "youthful" writings "in conformity with the dominant currents of Italian and European culture of the first two decades of the twentieth century" (p. 37). Next came a quasi-Machiavellian stage when Gramsci thought of politics as a "science." The Machiavellian Gramsci was attracted to a "Jacobinism" of Sorelian "inspiration." (Both Boggs and Paggi are fascinated by Gramsci's different evaluations of the Jacobin role in the French Revolution.) Such chopping up of Gramsci's life into phases governed by this and that "inspiration" derived from Croce, Lenin, Daniel De Leon, Machiavelli, Sorel, Salvemini, or Mosca makes it impossible to understand what Gramsci himself created. Indeed, why study Gramsci at all if he was only a derivative thinker? In the end, Paggi produces only a more sophisticated version of the tendency we saw in Boggs to erect straw men and to judge Gramsci according to them. To call Machiavelli a "Jacobin" political "scientist," is to ignore all the recent decades of serious scholarship have taught us. The author of the *Prince* also wrote the *Discourses*.

Then there are Paggi's post facto judgments on Gramsci's observations about fascism. In concluding that Gramsci "underestimated fascism as a political force capable of providing the basis for . . . the reorganization of the bourgeoisie" (p. 242), Paggi forgets that Gramsci was supremely conscious that he was not writing a scholarly treatise about events observed from a distance. Gramsci knew that he was both an analyst of and an actor in the struggle against fascism, and one of his objectives was to rally his supporters and keep up morale. His ridicule of the fascists as "monkey people" who thought that they were the best inhabitants in the jungle, but who in fact could only make ridiculously pompous speeches, did not represent his serious political estimate of fascism's capabilities. Gramsci's many articles on fascist violence from 1919 on show how seriously he took the movement that itself was to take his life. He knew the risks he was running in remaining in Italy after Mussolini's speech of January 3, 1925, "resolving" the Matteotti crisis.

If Paggi had only attended more closely to the documentation and resisted his temptation to interpret events in abstract categories, he would have written a better, more readable

book. The reader must wade through a thicket of dense prose to find a flower of insight. Even then, he is left at the end of the book feeling uncertain about where Paggi would have him turn to find the real Gramsci, alternately praised as original and exposed as derivative. Thus, the last lines of Paggi's text (followed by an appendix of over 100 pages) declare that while Gramsci succeeded in "enlarging" our notion of politics, he fell short of producing a "critique" of it. Gramsci's thought was burdened by a "perennial internal contradiction" which now must be "resolved" and "surpassed" (p. 381).

Paggi fails to see that precisely through "enlarging" our concept of politics, Gramsci achieved a "critique" of it. From his childhood and adolescence in Sardinia to his prison cell at Turin, Gramsci sought always to fight for a new political order in which "all living men, as many men as possible, all the men in the world will have united in society to work and struggle and better themselves" (letter to his son Delio, quoted in Paggi, p. 380).

David Forgacs and Geoffrey Nowell-Smith have compiled an annotated anthology entitled *Antonio Gramsci: Selections from Cultural Writings*, superbly translated by William Boelhower. The first section contains articles and reviews primarily from Caprioglio's critical edition of Gramsci's early journalism. The remainder of the book includes what the editors call "cultural" topics discussed by Gramsci in the *Prison Notebooks*. Gramsci's notes are grouped under the following headings: "Problems of Criticism," "Pirandello," "Canto X of Dante's Inferno," "Language, Linguistics, and Folklore," "People, Nation, and Culture," "Manzoni," "Father Bresciani's Progeny," "Popular Literature," and "Journalism." The editors provide a useful introduction explaining their criteria for selecting and organizing their texts. They also announce the imminent publication of another Gramscian anthology—on philosophy, science, and religion.

While any effort to make more of the Gramscian corpus available in English can only be welcome, the material is distorted when arranged according to criteria imposed by the editor. Gramsci himself rejected the division of human activity into politics, culture, science, art, etc. "Everything is politics," he declared, including philosophy

and culture. By dividing Gramsci's writings into some labeled "politics" and others labeled "culture," the editors obscure precisely what was most radical in Gramsci's architecture of a new politics.

Although the Forgacs and Nowell-Smith anthology of Gramsci's "cultural writings" is welcome, it is no substitute for reading Gramsci's text. The time is overdue for English translations of both Caprioglio's volumes of the early Gramsci and the full text of Gerratana's 1976 critical edition of the *Prison Notebooks*, preferably together with the complete index compiled by Renzo Martinelli and published in 1977 by the Istituto Gramsci in Florence. Then, and only then, can the English-speaking reader who does not know Italian go directly to Gramsci without having him filtered through the lenses of well-intentioned editors.

In *Lingua, intellettuali, egemonia in Gramsci*, Franco Lo Piparo argues that the roots of Gramsci's theory of hegemony extend more deeply into the past than anyone had supposed. Through creatively using Gramsci's elaborate notes of Professor Matteo Bartoli's lectures on linguistics at Turin University in 1912-1913, Lo Piparo demonstrates how greatly Gramsci's linguistic studies influenced his later political thought. Gramsci learned from Bartoli that language is much more than grammar and vocabulary; it is a component of a worldwide process in which language, politics, and culture are linked.

Lo Piparo's methodology contrasts favorably with that of Boggs, Hoffman, and Paggi. Instead of judging Gramsci by ill-defined terms ("Jacobin," "vanguardism," "neo-Hegelianism" and the like) Lo Piparo explores the meaning of key Gramscian terms such as "prestige," "spontaneity," and the "people-nation" as they evolve within the Gramscian texts themselves. In this respect, Lo Piparo's humanistic approach could serve as a model for scholars in the history of political theory.

Lo Piparo helps lay to rest the conventional view of a radical break between the "young" and the "mature" Gramsci. His interpretation reinforces Caprioglio's message. Lo Piparo also shows that the dates for Gramsci's "youth" must be pushed back from 1916 to at least 1912-1913.

Franco Lo Piparo is the first scholar to have investigated thoroughly the impact of linguis-

tic studies at Turin University on Gramsci's political theory. He skilfully relates passages from Gramsci's 1912-1913 lectures notes to passages in the *Prison Notebooks* on the idea of "hegemony."

Generally faithful to what Dilthey called the method of *verstehen*, Lo Piparo lapses into judgmentalism, however, when he dismisses Gramsci's paradigm of the *società regolata* as "fantastic" and as occupying "a marginal place in Gramsci's thought" (pp. 125, 150, n. 71). This statement is surely arbitrary; one might more plausibly argue that the concept of the "regulated society" crowns Gramsci's thought. For the Sardinian revolutionary, the "regulated society" dispenses with both coercive government and conformist social pressure. It is a condition in which human beings really (and not just on paper) "agree to accept the laws spontaneously, freely," rather than through coercion (*Quaderno 6*, Gerratana edition, 1976, p. 764, paragraph 88).

In the study of writers of rank, the interpreter must take pains to place understanding first, and his own criticisms second. Lo Piparo has every right to criticize Gramsci's theory of the regulated society, but as a serious scholar, he should not have dismissed it arbitrarily.

It is in the nature of a paradigm to appear unreachable. Whether Gramsci's idea that men have the capacity to shape the world according to the designs of a new politics committed to including every human being equally is feasible or not, that vision hardly holds a "marginal place" in Gramsci's political thought. The conviction that all the world can be an oasis of freedom and equality and that human beings do not have to continue to live in the desert of the old politics was central to Gramsci's political vision. Antonio Gramsci knew that "a desert is still a desert, even if one sees an oasis with palm trees here and there." He had faith that in time all the world's people will be able to lead the life of self-critical reason and to appropriate the great cultural treasures of the past, hitherto the preserve of restricted elites. Such a happy issue out of our present afflictions would not occur automatically, however. "Pessimism of the intellect, optimism of the will" was his motto to the last.

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POLITICAL
THEORY

Polis and Praxis: Exercises in Contemporary Political Theory. By Fred R. Dallmayr. (Cambridge, Mass. and London, England: The MIT Press, 1985. Pp. xi + 297. \$25.00.)

Fred Dallmayr, Packey Dee Professor of Government at the University of Notre Dame, calls this provocative collection of essays "exercises" to signify the exploratory character of his work. The term also indicates the difficulty of Dallmayr's enterprise and reveals his understanding of philosophy as philosophizing. The author's intention is to clarify and redefine salient social and political concepts whose current problematical meanings contribute to the crisis of our time. "Little inquiry," writes Dallmayr, "is required to show the deep opacity shrouding the concept of freedom in contemporary Western thought and its deterioration into an ideological slogan (akin to the status of 'socialism' or fraternity in the East): Mankind . . . is steadily drifting toward a political cataclysm, on both sides propelled by watchwords or doctrines whose significance is only barely intelligible" (p. 107).

Dallmayr does not attempt to impose a conceptual system of his own. Closed doctrines violate his idea of what philosophy is about. Instead, he conducts a many-sided dialogue of search in the form of reflections on the ideas of leading thinkers who have devoted their lives to formulating a new way of ideas. Dallmayr's philosophic openness is revealed in the range of opinion he canvasses: the thought of Martin Heidegger, Michael Oakeshott, Leo Strauss, Hans Georg Gadamer, Max Weber, Hannah Arendt, Edmund Husserl, Maurice Merleau-Ponty, Juergen Habermas, Michel Foucault, Richard Rorty, Richard Bernstein, Duncan MacRae, and others. The essays restate, compare, analyze, select, and synthesize pregnant ideas that Dallmayr draws from this grand array of thinkers. Each essay is a kind of Socratic conversation.

The book's seven chapters focus, seriatim, on (1) the meaning of political philosophy, (2) "experience" as "praxis," (3) the idea of

"power" (4) Heidegger's ontological meaning of "freedom," (5) critical theory and humanism, (6) evaluative discourse in the study of public policy, and (7) politics and communication. Building on foundations laid in *The Twilight of Subjectivity* (1981), Dallmayr reveals the pitfalls of solipsistic freedom; of separating theory from practice and fact from value; of confining experience to sense experience; of understanding power as a term signifying domination and control; of an instrumentalist approach to public policy studies. He leads us toward a post-individualist political philosophy by asking that we consider the fruitfulness of anchoring freedom in "being" (following Heidegger), that we engage in philosophical innovation which maintains continuity with tradition while avoiding slavery to it, and that our political theorizing be action-oriented. Above all, he wishes to persuade us to understand political philosophy as the communication of free minds in a spirit of friendship. He closes his final chapter with a reflection on Oakeshott's thought that "the world *sub specie amoris* is unmistakably the world of practical activity. . . ; nevertheless, love and friendship generate a peculiar contemplative engagement, an acceptance of 'whatever it turns out to be' " (p. 223). This is a statement of hope, not of resignation.

Dallmayr's essay on Heidegger's ontology of freedom (which appeared in modified form in *Political Theory* in 1984), is the centerpiece of the book. It presents a fresh and exciting reading of Heidegger and reveals as false, dominant interpretations of his work as fatalistic. Heideggerian freedom is the defining characteristic of the human person, and is grounded in the freedom conceived as "the comprehensive and pervasive dimension of being" as such (p. 124). The problematic of this idea, it seems to me, lies in the difficulty of persuading a modern mind of the plausibility of Heideggerian "being" as a fundamental ontological category.

Polis and Praxis is evocative, erudite, and philosophically rich. Its message will be pondered seriously by persons of good will who

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are ready to engage, with "rebellious gentleness," in the necessary work of rethinking our political lexicon.

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Democracy and the Rule of Law: Liberal Ideals and Marxist Critiques. By Bob Fine. (London: Pluto Press, 1984. Pp. viii + 231. \$11.95, paper.)

Bob Fine's analysis of the idea of the rule of law starts in discontent. His disagreement is with two polar perspectives in contemporary Marxist thought on the question of Marxism's relationship to liberalism, particularly the ideology of the latter on the question of the coherence of the liberal conception of the rule of law. The first of these views sees Marxism as compatible with—perhaps even an extension of—liberalism on questions of legality and democracy. In other words, to be faithful to Marxism, socialist thought need not jettison a recognition and appreciation of liberal principles and the idea of the rule of law. Instead of opposing liberal ideals, Marxists can, without sacrificing ideological integrity, incorporate liberal notions of equality and justice within the framework of a Marxist jurisprudence.

But there is a danger in such an accommodation: by incorporating liberal values within a Marxist framework, the liberal forms of class-based authority will be masked and the antinomious character of liberal political theory obfuscated. It is in response to this danger of ideological infiltration that the opposing pole of Marxist thought rejects outright any attempt to accommodate within Marxism any part of liberal political ideology, in particular the idea of the rule of law. Thus, the rule of law is rejected as an obfuscation of liberal forms of power and domination.

Need Marxism be completely antithetical to all facets of liberal thought? Fine thinks not, and he devotes the first two of seven sections in his book to a review of the development of Marx's jurisprudence against a background of the classical jurisprudence of Hobbes, Rousseau, Adam Smith, and Hegel between the seventeenth and nineteenth centuries. In approaching Marx's jurisprudence from the perspective of intellectual history, Fine

attempts to demonstrate Marx's debt to classical jurisprudence, and at the same time (in Sections 3 through 6), seeks to show the development of Marx's thought away from the classical view to a point more in keeping with his mature writings on political economy—in particular, the *Grundrisse* and *Capital*.

The last section of Fine's book is devoted to two twentieth century attempts to develop Marx's critique of jurisprudence in a new direction, and in one instance, to displace it. The effort at displacement is that of the late Michel Foucault to transcend the left/right distinctions in favor of an historical/analytical discourse on forms of power. The revisionist attempts at reconstruction are those of the Russian legal theorist Evgeny Pashukanis and the contemporary English Marxist historian E. P. Thompson. Fine takes up Thompson's declaration in the latter's *Whigs & Hunters* (1977) that the rule of law is "an unqualified human good" and finds that in the course of rejecting vulgar Marxist conceptions of law and state (e.g., L. Althusser), Thompson himself "comes close to abandoning Marxist criticism of the rule of law in its entirety in favor of a resuscitated liberalism" (p. 175).

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Retrieving Democracy: In Search of Civic Equality. By Philip Green. (Totowa, New Jersey: Rowman and Allanheld Publishers, 1985. Pp. ix + 278. \$19.95.)

Philip Green has taken seriously the view that theorists are revolutionaries with a pen. In his book, *Retrieving Democracy*, he attempts to set a revolutionary agenda for the United States. The first step is to dispel the notion that we currently live in a democracy. Although Green finds some admirable qualities about our current system, especially when contrasted with other existing systems, he nevertheless believes it to be a "pseudodemocracy." Control of the polity simply is not in the hands of the demos.

Green's objective is to seek a true democracy, one that captures the spirit of Rousseau's general will without the potential authoritarian qualities that can be attached to it. This is to be

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achieved through political equality, participation, and the effective use of representation. The excesses of majoritarianism are to be curbed by an expanded use of judicial review.

However, political equality cannot be achieved without social equality. To be civic equals, we must do away with our class system (capitalistic economic divisions) and our caste systems (social divisions based on sex and race). Green focuses upon the division of labor within workplaces as a critical determining factor. Divisions of labor are necessary, but they should be democratically determined. People should be free to determine for themselves the type of work they wish to do.

Green captures his major prescriptions presented within the body of the work in the final chapter. There he poses a program for the future of the United States—a future he believes is closer than we think. His proposals include items such as the social control of corporations, equalization of wages and job opportunities, democratization of trade unions, and the insulation of politics from money.

Retrieving Democracy makes for good reading. The arguments are clear and the writing is concise. Although there is nothing new about the basic contention that to change the political world we must first change the social context in which politics takes place, there is something novel in the way Green blends what is worth keeping in the current system with what must be changed. For example, many of the aspects of liberal politics and the constitutional institutions that are created within advanced capitalistic states become cornerstones of the democratic future envisioned by Green.

How convincing Green is probably is determined more by whether he is preaching to the converted than by how persuasive are his arguments. As with most polemical pieces, documentation is kept to a minimum. Although he has attempted to be practical at the same time he is utopian, I do not know that he has set an agenda for social and political action. I do not know that he has offered the reader much guidance as to what to do about the sexism and racism that could continue to undermine his hopes for a more egalitarian future.

Setting the agenda for radical change in the United States is a serious enterprise. Green is at

his best, however, when he is not taking himself so seriously.

RONALD M. MASON

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Sports and Political Ideology. By John M. Hoberman. (Austin, Texas: University of Texas Press, 1984. Pp. 315. \$24.50, cloth; \$9.95, paper.)

This book offers a fine survey of political theory and its relationship to body fitness. Along the way, Hoberman delves into the different approaches that European intellectuals have taken toward sports. This book is important in that it offers a comprehensive survey of the role of sport ideology in Nazi Germany, the German Democratic Republic, the People's Republic of China, and the U.S.S.R.

Hoberman does not offer any new theories about sports and ideology, but he does provide strong scholarship in his presentation of the views of others. He assumes that every political ideology has an official "anthropology" and that ideologies differ on how they relate to the body. Moreover, he assumes political ideologies have permeated most every form of culture within the various societies and that the conduct of sports in a society reflects the political ideology of the country.

He examines European intellectuals on their views toward labor versus play as human fulfillment. For example Marx and Engels argue for the "primacy of labor" and Ortega y Gasset and Johan Huizinga argue the importance of the play dimension in a society while not negating labor. Next, Hoberman looks at the body as an ideological variable in states. The athletic body has served as a symbol for the body politic and as a symbol for political leaders to manipulate for ideological reasons. According to Hoberman, the fascist view of the athletic body as virile, heroic, masculine, and dynamic contrasts with the Marxists' refusal to play the narcissistic intellectual game. Nazi sport theory emphasized racial heroism while early Soviet sport ideology attacked the competition of bourgeois sports.

Yet as Hoberman observes, modern Soviet views toward sports rejected the earlier attacks on competitive sports when the state realized

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that there are certain elements of play which the state cannot control. Still, problems exist in Soviet society when the extraordinary Soviet athlete is valued above the masses.

East German political ideology looks at the body as a machine that can be made to perform at levels which are beyond normal expectations by the use of technology and science. Leisure in the East German state cannot be idle, rather it must be productive, and move toward a goal of "all-around human development." Maoist sport in the People's Republic of China on the other hand does not emphasize competition or the development of superior athletes. Sports are good for the masses in that they develop healthy bodies for all. Hoberman then indicates shifts that the post-Mao society has undergone with respect to sports. Sports may be pursued in an effort to develop a productive, technically superior, competitive Chinese athlete who will bring honor to the state.

Although this book is well written and extremely literate, there are some points of disagreement that this reviewer must raise. Hoberman fails to deal with American political ideology and sport, and he should have been able to develop this chapter in the story of sports and political ideology. He decides not to pursue this matter because ideology plays a very modest role in American political life according to Hoberman. I think he erred in leaving out the importance that political ideology has on the conduct of American sports.

There are also other ways to investigate sports and political ideology rather than exclusively centering on what political thinkers think and say. One could just as easily conducted empirical investigations as to the ideological beliefs of various athletes and how these related to their conduct of sports.

Regardless of my minor disagreements on research strategy, Hoberman has written a major book in the field of sports and politics. He makes explicit connection between sports and political ideology in areas which some readers might think are obvious. Yet they were not obvious until Hoberman pointed them out.

JOHN ORMAN

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Anarchism. By David Miller. (London: J. M. Dent and Sons, 1984. Pp. 216. £10.95.)

The tradition of anarchist thought is a rich and varied one which, in the West, represents the most radical challenge to political authority and, more precisely, the modern state. Historians of anarchism James Joll and George Woodcock have provided us with excellent, authoritative accounts of this tradition. Joll's approach is that of the historian of ideas while Woodcock's history is more sensitive to the complex interaction between ideas and socio-historic contexts. Yet both effectively combine historiography with biography in their respective constructions of "the anarchist tradition." David Miller's recent work on anarchism is designed to complement these two fine histories by focusing on the ideological character as opposed to the historical dynamics of this revolutionary tradition. Anarchism is reassessed as an ideology, and as a system of ideas and set of beliefs about human nature, history, society, politics, economics, the state, revolution, and radical reconstruction. Like other major enduring doctrines of the modern age, anarchism is treated as a coherent world view capable of holding its own when compared with the likes of liberalism, conservatism, socialism, Marxism, and their more contemporary offspring. Thus, this exercise is reminiscent of Daniel Guérin's *Anarchism* with its emphasis upon the relationship between theory and practice. However, unlike Guérin, Miller distances himself from the ethos of the old New Left. His ideological stance, which is revealed early on, is that of a "market socialist" sympathetic to some of the ideas and ideals of the anarchist thinkers while unwilling to endorse anarchism as a theory.

Miller's work is divided into three parts. Part one briefly surveys the "varieties of anarchism"; Part two examines the difficult relationship between anarchist theory and revolutionizing practice; and Part three offers a summarizing evaluation of constructive achievements, critical questions, and noble failures. A preliminary sketch of this radical ideology establishes a common ground shared by all self-proclaimed anarchists and identifies two major premises which hold anarchism together: anti-statism and voluntary association. Yet while there is unity in the anarchist ranks, there is also diversity. Thus, Miller pro-

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ceeds to analyze three different schools of anarchist thought: philosophical, individualist, and communist. Philosophical anarchism, the intellectual assault on the principle of authority, is repudiated by way of a critique of Godwinian utilitarianism, Stirner's egoism, and the radical neo-Kantianism of Robert Paul Wolff. American anarcho-individualism, the libertarian tradition, is represented by the thought of Josiah Warren, Benjamin Tucker, and Murray Rothbard. Here the discussion centers on individual sovereignty and the radical return to a purified free market system. Anarcho-communism, by far the most substantial anarchist stream in both theory and history, brings us into contact with the discourses of Bakunin, Kropotkin, Malatesta, Berkman, Goldman, and Bookchin on communitarianism, anti-capitalism, mutual aid, positive freedom, and mass revolution. Miller concludes that (1) all the philosophical arguments for rejecting authority are unsatisfactory, and (2) for the most part individualist and anarcho-communism argue past each other due to their unresolvable views on human nature and radical reconstruction.

These conclusions may be correct, yet Miller's overall exposition and critique of these three variants are simply too brief to do justice to the anarchist perspective. As such, the reader is given only an introductory flavor, a taste of anarchist thought. Perhaps this was his intention. Yet Godwin's *Enquiry Concerning Political Justice* cannot be dissected in less than four pages, even if its enlightenment rationalism betrays certain contradictions, eccentricities, and elitism. Stirner's creed of conscious egoism and nihilistic disobedience is also, perhaps surprisingly, more complex than presented. Further, it can be argued that Stirner is the precursor of a possible fourth school of anarchist thought, existential anarchism. But Miller's most glaring weakness is his failure to include Jeffrey Reiman's reply to Wolff's classic *In Defense of Anarchism* as developed in his *In Defense of Political Philosophy*. The absence of a serious treatment of the Wolff-Reiman debate is disappointing. However, Miller's general portrayal of individualist anarchism and communist anarchism as extremist versions of classical liberalism and communitarian socialism, respectively, is well argued. Both schools are far too one-sided and thus blind to the greater complexities of human

identity, socio-political organization, and radical change. Anarchist thinkers are especially weak in economics. Proudhonian mutualism is the most plausible theory that they can offer.

Part two, "Anarchism as a Revolutionary Ideology," the heart of this book, focuses on varying conceptions of human nature and historical progress as they inform anarchist practice, the relationship between anarchist practice and Marxism and syndicalism, organization and strategy, violence and terror, and the New Left. Here the relation between theory and practice is more rigorously analyzed with regard to specific issues. Anarchists emerge from this scrutiny as better theoreticians of human nature than they are of history. While Kropotkin's mutual-aid theory of social evolution comes closest to a theory of history, it, like Marx's historical materialism, is found to be more ideological than scientific. The anarchist-Marxist debate is also well done, with anarchists scoring high points in their critique of Marxism. This attack includes refutations of historical materialism, "scientific socialism," state theory, revolutionary elitism, agency, and post-revolutionary reconstruction. The historic outcome of this debate in turn yielded the "syndicalist synthesis." Miller may criticize the anarcho-syndicalists in theory, but he has a difficult time dismissing them in practice, given the relative success of the Spanish experience. The New Left, unfortunately, is afforded a narrow and stereotypical treatment. Here the focus is on the French "Situationists," and the claim is made that the New Left fundamentally diverted anarchism⁹ from its revolutionary ideals because of its emphasis upon creating a parallel, alternative society. Alternativism replaces anti-statist action.

The means-ends uncertainty in anarchist thought draws its arguments from a rich history of noble failures. Miller's judgment is that while anarchists tend to be better means-ends thinkers than Marxists, their strategies have failed while Marxists' have succeeded. Why? Because anarchist organization is almost a contradiction in terms. And, ironically, the most committed anarchists often in the end tend to be radical individualists. Finally, is anarchist terrorism the solution to the problem of means and ends? An examination of the 1890s and 1970s sheds light on this question. While ter-

rorism is ruled out as ineffectual in the long run, so is non-violent resistance. Strategic violence linked to populist insurgence seems the most practical solution. While not endorsing this approach, Miller reminds us of Engels' dictum that revolutions are ultimately contests of arms.

If the lofty ideals of anarchism for the most part fail to pass the test of rigorous theorizing and effective practice, then why concern ourselves with this political ideology? Miller offers us two reasons: anarchism as critique and the importance of small-scale social experimentation. It is in these two areas that the anarchist tradition lives on. As a mode of critique, anarchism constitutes a potent moralizing force which keeps alive the most radical vision of freedom, exposes the pervasiveness of power relations, draws attention to the negative consequences of state power, and reaffirms an organic communitarian ethos in the face of increasing techno-bureaucratization. Furthermore, anarchist-inspired experimental communities have proven to us that radical freedom and social solidarity can exist, if only briefly, and that the almost hypnotic power of the modern nation-state can be transcended. Miller's survey of anarchist thought serves as a fine introduction to this too often forgotten political tradition. It forces us to rethink several critical questions which plague political societies to this day. Current anarchist thought, which builds its case upon these more classical questions, has developed far more sophisticated arguments which take us beyond the traditional state and revolution paradigm to examine the multiplicity of dominating forces present in our society. Indeed, the most powerful contribution to the contemporary anarchist critique has been developed by none other than Michel Foucault. His analysis of our post-modern age of panoptic control and surveillance, disciplinary technologies, and microphysics of power fits the anarchist critique. What is needed is an equally sophisticated micro-political theory of resistance and renewal.

WAYNE GABARDI

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Conceptions of Liberty in Political Philosophy.

Edited by Zbigniew Pelczynski and John Gray. (New York: St. Martin's Press, 1984. Pp. 438. \$37.50.)

This book is a collection of 17 essays addressing the thought of 16 writers on the subject of liberty in political philosophy. In their expositions, the contributing authors also address, either explicitly or implicitly, Sir Isaiah Berlin's distinction between negative and positive liberty. Several use Berlin's categories as their themes, concentrating upon ways in which the writers who are their subjects do or do not vindicate the form or substance of Berlin's arguments.

As is well-known, Berlin's distinction between negative and positive liberty rests, most simply, upon a distinction between liberty as noninterference and liberty as self-government. Lurking within the latter category is the potential existence of a rational or "real" will distinct from an individual's expressed will, the content of which may be interpreted and imposed by an individual or group claiming a monopoly on its "correct" understanding. Linking a number of the essays in this volume is a consideration of the degree to which liberty in general, sometimes including the conception of a rational or real will, is in fact compatible with an emphasis upon the value of freedom to the individual, as opposed to its value to society as a whole. In what follows, I shall attempt to draw upon this theme.

The two poles can perhaps be marked by Berlin himself and by Fichte, by a belief in the incommensurability of values as opposed to a commitment to an extreme rational monism. John Gray suggests that although Berlin is highly critical of metaphysical rationalism, his preferred conception of liberty is not an unreservedly negative one. It is compatible with minimal requirements of rational choice and is consistent with a definition of freedom as the nonrestriction of options. Central to Berlin's thought is his acceptance of the competition of incommensurable goods as "an unalterable feature of the human predicament" (p. 344). At the opposite extreme is a vision like Fichte's, foreseeing, as Roger Hausheer writes, an eventual rational harmonization of all human interests and ends. "Men will cease to stretch out for what they cannot attain and

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so will no longer experience frustration and failure. . . . Men will desire what they can perform and do what they desire" (p. 146). Gray's Berlin and Fichte both grapple with the notion of a real or rational will. In Berlin, it may empower the individual *qua* individual, whereas in the later Fichte it does not.

Between these poles, many viewpoints are represented. James Tully argues that Locke's identification of civil liberty with a law that is constitutive rather than regulative of freedom, completing rather than opposing it, calls into question Berlin's conviction that negative freedom is the sole basis for contemporary "liberal" freedoms. Life in political society is for Locke both natural and free in terms of the fulfillment of human potential. Along related lines, Patrick Gardiner observes that in Rousseau's thought, an individual may act without external pressure (negative liberty) and in response to a genuine desire of his own (positive liberty), yet may still perceive himself as unfree if his desires somehow preclude the realization of ideals at the center of his vision of a worthwhile life. And G. W. Smith argues that J. S. Mill's conception of liberty contains positive as well as negative elements (a view shared by Gray). It includes an emphasis upon the self-mastery necessary to autonomy and self-development, even if it relies upon an optimistic assessment of people's desires for self-development when conditions inhibiting the occurrence of this desire are absent. Tully, Gardiner, and Smith all imply that in the work of the philosophers they are interpreting, the notion of a rational or real will may enhance the value of freedom to the individual.

On the other hand, Richard Mulgan points out that although liberty was important in ancient Greece, it was justified by its value to the community as a whole, rather than by its value to individual citizens. For T. H. Green, freedom depends not simply upon acting according to one's preferences, but also upon the nature of the preferences. Yet, suggests John Roberts, freedom in general is justified by Green for its development of the individual's capacity to contribute to the common good. Even Hayek, according to Norman Barry, often implies that freedom's chief value is in the way it serves society. And his identification of law with liberty on a formal and anti-rationalistic ground, unlike Locke's similar identification, deprives him of a substantive

basis for critiquing restrictive general rules. Moreover, although the final end of communist production for Marx was to secure for individuals time to be utilized according to individual preference (negative liberty?), Andrzej Walicki suggests that true freedom "was not 'negative' in the sense of being aim independent. It was a means for the realization of the final end of history" (p. 239), the development of the new, regenerate species being.

This volume also contains essays on Hobbes by D. D. Raphael, on Spinoza by George Parkinson, on Kant by Charles Taylor, on Hegel by Zbigniew Pelczynski, on Oakeshott by John Liddington, on Arendt by Ronald Beiner, on Rawls by Jeffrey Paul, and on Habermas by Jay Bernstein. The essays are of consistently high quality and induce thoughtful reflection, whether or not the reader agrees with the specific conclusions of individual contributors. Since all essays are at least implicitly informed by issues raised by Berlin's discussion of liberty, their coherence is markedly greater than in many similar volumes in which the essays do not seem related. This book will repay the attention of those interested in epistemology, political thought, and the human condition in general.

EMILY R. GILL

Bradley University

Lenin and the End of Politics. By A. J. Polan. (Berkeley: University of California Press, 1984. Pp. 229. \$22.50, cloth; \$9.95, paper.)

First premises shape politics. Political philosophers, whose *metier* is first premises, come in two varieties: those who erect their first premises into total systems, and those who scrutinize first premises and their consequences. In *Lenin and the End of Politics*, A. J. Polan, a first-rate philosopher of the second variety, scrutinizes V. I. Lenin, a second-rate philosopher of the first variety. Polan offers an "effective history" (p. 53) of Lenin's *The State and Revolution* (1968) by which Polan means (following Gadamer) a hermeneutical reading involving "the reasoned interrogation of tradition and the traditional interrogation of reason" (p. 49). Polan concludes that Lenin's "first move," as the philosopher whose premises shape politics both in contemporary

"Marxist" states and in the socialist tradition, "is . . . to eradicate politics" (p. 175).

Polan's book is a meditation on the meaning and possibility of politics in our time, one in which the Gulag and Polish Solidarity both exist. Puzzling over "why the threat of Solidarity was so profound" (p. 5), shaking the foundations of Eastern European regimes, Polan is led to Lenin's "conceptual starting-point" (p. 59) in *The State and Revolution*. That starting-point—Lenin's first premise—concerned the problem of bureaucracy. Lenin contended that bureaucracy was rooted in "capitalist culture" (p. 59) and that, after the capitalist state machine had been "smashed," state functions could be reduced to "simple operations" (p. 59) along lines analogous to the postal system.

Polan's second chapter evaluates Lenin's critique of bureaucracy in light of "an alternative body of work, from Weber to Habermas" (p. 70) which includes such divergent theorists as de Tocqueville, Bukharin, and Gouldner. Polan's assessment is pointed and persuasive: "Lenin's unsophisticated anti-bureaucratism ends up conspiring with the bureaucracy in the maintenance of power" (p. 70). Chapter three extends Polan's interpretation of the "continuing effectiveness of *The State and Revolution* in the Russia of the Gulag" (p. 53) to encompass "how later writers . . . have contributed to a continuing misunderstanding of the world Lenin made" (p. 90). This chapter is a tour de force. Ranging from refutation of various theses about the "degeneration" of the Russian Revolution—through perceptive criticism of the theory of the authoritarian state as developed by Frankfurt theorists Horkheimer, Adorno, and Marcuse—to a compelling analysis of similarities and differences between the American and Russian revolutionary experiences with regard to the rule of law, Polan builds his relentless case against Lenin and Leninist discourse. This reader concurs with Polan's verdict: "[T]he text, in all its moments—libertarian and authoritarian—is guilty of subsequent developments: that is, the features of the authoritarian Soviet regime are present within every line and concept of the text. And it is not just a question of *similarity* between what was written and what later happened: the *cultural* effect of *The State and Revolution* can be suggested as the *causal link* between text and subsequent events" (p. 129). Chapter four is an

anticlimactic postscript to the foregoing analysis in which Polan gilds the lily, arguing that "Lenin's cosmology, Lenin's concept of parliamentarism, Lenin's culture, and Lenin's theory of political motivation" (p. 136) were philistine.

Returning to form in chapter five, Polan executes a brilliant exegesis of Sartre's *Critique of Dialectical Reason* (1960). Polan suggests that Sartre "ruptured the [Leninist/utopian] discourse which has previously prevented revolutionaries from grasping the consequences of acting in the world" (p. 191). Here Polan sounds the theme that is his first premise, and his major contribution. From Sartre, Polan derives an appreciation of the relationship between politics and time. Human beings experience the terror of scarce time. Time is the ultimate finite resource and politics is humanity's means of dealing with this fact. Polan writes, "Politics is an ontological failure because it cannot halt the passage of time, the inevitability of change and the passage of generations" (p. 204). Still, politics is all we have. Lenin's "crime" (p. 130) is that he promised a society lacking nothing, a world in which time would have been stopped, thereby rendering politics unnecessary—indeed, intolerable. Lenin's "seductiveness" (p. 206) is that he promised what mortal humans desire. Polan's accomplishments are the repudiation of this promise, and the reminder that, paraphrasing Hannah Arendt, though we must die, we are not born in order to die, but to begin.

JAMES C. FOSTER

Oregon State University

Property and Political Theory. By Alan Ryan. (Oxford: Basil Blackwell, 1985. Pp. iv + 198. \$24.95.)

Despite its centrality to the history of political thought, the idea of property and its various manifestations has received surprisingly little attention from modern scholars. There are a few exceptions to this: Paschal Larkin's *Property in the 18th Century*, Richard Schlatter's *Private Property*, some of C. B. Macpherson's work. Pocock on Harrington and others, Tully on Locke, the NOMOS collection on the subject, a 1979 edition of essays by Anthony Parel and Thomas Flanagan, Lawrence Becker's

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Property Rights, H. T. Dickinson's *Liberty and Property*, and recent or forthcoming work by Janet Coleman, Istvan Hont, Donald Kelley, Andrew Reeve, and others. But this list still bears little relation to the importance of the subject, and despite new interest in the eighteenth and nineteenth centuries in particular—when disagreements about property threatened to, and perhaps did overturn western political theory—there are many problems untreated, writers neglected, and questions begged in the field.

One short book could scarcely begin to meet these deficiencies, and Ryan makes no pretense of so doing. Instead, an attempt is made at a philosophical analysis (not a history of ideas) of the relationship between work and property among a number of key western thinkers, concentrating upon how property is justified or criticized, and how it has been related to notions of citizenship. Ryan begins with Locke. Grotius is mentioned once, and, inexplicably, Pufendorf not at all, though the latter is certainly as important to eighteenth century thinking about property as Locke—on his own as well as through Vattel, Hutcheson and others.

However, Locke is primarily honored, and secondly Rousseau, because Ryan finds that in these thinkers two modern traditions of thinking about property begin. One of these is the "instrumental" view, in which social institutions, like private property, are justified by their contribution to the mutual well-being of all. The second is the "self developmental" theory, in which mankind is also viewed as God's creature, expressing itself through fulfillment of its duty to God (though this obligation is gradually secularized). After Rousseau, Kant and Hegel can be understood as exemplifying the chief defense of private property from the self-developmental perspective, and Bentham from the instrumental, utilitarian viewpoint. Marx and Mill are then both treated as protagonists in a nineteenth and twentieth century debate about how best to set aside the eighteenth century defense of private property, with Marx attacking largely from a self-developmental, and Mill from an instrumental basis. Ryan's conclusion is an attempt to explain why both Marx and Mill can be understood as having failed to predict the future development of capitalism, and is set in the form of the question, "why are there so few

Socialists?," though it might well have been put in the converse, "Why are there so many social democrats?"

Ryan's presentation of modern debates about property in terms of these two traditions is extremely helpful, and commends this book to every student of modern political thought. It is perfectly pitched at the senior undergraduate level and will serve very well for teaching purposes. The discussion of individual thinkers will probably not upset the expectations of most scholars. A longer book would certainly need to say more about the background and connections; i.e., about Aquinas and the medieval natural law tradition, the discussions of money, usury and just price in their political context, as well as the accounts of the origins of private property and its justification given in this tradition. There is far too little about the relation of theories of property to many of the central concepts of political theory, especially liberty (the Physiocrats beg inclusion in such a book).

Despite the gaps the fact remains that the chapters are good introductions to the treatment of the topic by various important writers, and in this sense the book is of greater value for its individual chapters than the sum of its argument. Hence, on Locke, Ryan summarizes current views, on Rousseau he is a good companion to the discourse on inequality and political economy, while the inclusion of Kant helps to stress the latter's importance. A chapter on the Utilitarians makes the telling point that utilitarianism and private property were not inevitably connected in the early nineteenth century, and concentrates on Bentham. On Hegel, Ryan explores the notions of ownership and possession, showing the centrality of these to the structure of Hegel's thought. "Mill and Marx and Socialism" links the two nineteenth century thinkers more closely than is customary, finding them both to be defenders of socialism on the grounds of its greater service to freedom, though the wide gap evident in the positive/negative liberty distinction belies much of this apparent coalescence. Finally, the non-existence of modern systems of voluntary socialism is connected to the general prosperity of contemporary capitalism and to greater popular concern for instrumental, than for other notions of property, as well as to the lack of working-class inclination for self-government.

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There is much that is valuable here by way of suggestion and innuendo, but it is a pity that a longer book was not written to explore the topic more fully.

GREGORY CLAEYS

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Ideology and the Urban Crisis. By Peter J. Steinberger. (Albany, N.Y.: State University of New York Press, 1985. Pp. ix + 175. \$34.50, cloth; \$10.95, paper.)

Ideology—along with pollution, crime, and poverty—seems to be symptomatic of the American urban crisis. Peter Steinberger has attempted to make philosophical sense of its various manifestations in the large body of literature inspired by the misfortunes of the nation's cities. The modest aim of his short book is to link each of several urban ideologies to a particular tradition of political thought and to evaluate the coherence and plausibility of the philosophical material contained in each ideological position.

Steinberger distinguishes ideological works from those of political philosophy or policy advocacy by referring to the purposes for which they were written. An ideological writer does not offer prescriptions for all time, like a political philosopher, but expects them to endure longer than the recommendations of a policy advocate, who addresses specific events and programs. Ideologies can also be distinguished by the degree to which the judgments that they advance are based upon explicit and systematically developed principles. Here again, Steinberger locates ideology midway between political philosophy and policy advocacy.

One wonders how seriously these criteria were followed in practice. For example, Steinberger identifies Daniel Patrick Moynihan's *Maximum Feasible Misunderstanding* as an ideological work even though it deals with a specific federal program. The implication, perhaps, is that the ideological status of a book depends not only on the purposes for which it was written, but on the way in which it is read.

Steinberger outlines three urban ideologies—managerialism, communalism, and possessive individualism. Taking up each in turn, he

first identifies the essential tenets of the position, then surveys its philosophical roots, and finally turns to critiques of the ideology's theoretical presuppositions. Particular attention is given to the idea of human nature implicit in each ideology, its theory of moral judgment or practical reason, its notion of political obligation, and its conception of the public realm.

Communalism, the ideology to which Steinberger himself seems most sympathetic, receives the least adequate treatment. With the exception of Alexis de Tocqueville, Steinberger recognizes no important exponents of the communalist position in this country until after the emergence of community action programs in the 1960s. Such important writers as Lewis Mumford and Mary Parker Follett are thereby ruled out of account, and others who belong to the era of community action, like Jane Jacobs and Paul Goodman, are unaccountably ignored. Steinberger relies instead on other proponents of localism, citizen participation, and neighborhood government—most of them less influential than the writers whom he has overlooked.

Steinberger finds the origins of managerialism in nineteenth and twentieth century progressivism, and his account of the position relies heavily upon the writings of selected municipal reformers and urban planners, almost all of which were published over twenty years ago. More recent exponents of managerialism among the practitioners of policy analysis and policy science receive hardly any attention, and as a result Steinberger's version of the position suffers from a kind of arrested development.

Possessive individualism is the ideological position attributed to many of neo-conservative writers who have addressed the problems of the cities since the 1970s. Steinberger's account of this urban ideology is based on an analysis of only three exemplary works, but his description of the position is a persuasive one all the same, and his discussion of its Hobbesian affinities is insightful and helpful.

In general, however, Steinberger's effort to uncover fundamental principles is curiously indifferent to fundamental questions. It takes for granted the existence of an urban crisis without considering whether the crisis itself may be an ideological artifact. It asserts that urban politics is "deeply ideological," but

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never asks what there is about urbanism that evokes ideological responses. It argues that all three of the ideologies described are "insufficiently urban," but neglects to inquire how this came about. Steinberger's closing plea for a more philosophical approach to urban politics would seem to require responses to just such questions.

MATTHEW A. CRENSON

Johns Hopkins University

Marx's Critique of Politics: 1842-1847. By Gary Teeple. (Toronto: University of Toronto Press, 1984. Pp. xi + 303. \$35.00.)

Marx's Critique of Politics is a close reading of Marx's writings from his doctoral dissertation on Epicurus through the *German Ideology*. Teeple argues that much of the literature mistakenly characterizes the early Marx as a philosophical idealist or Hegelian, and a political liberal or democrat. These characterizations often lead to the view that there is a break in Marx's development between the early and mature works. By following the development of Marx's problems, Teeple wishes to show that there is a continuity both of philosophical method and political development. Philosophically, the continuity is to be found in a developing method of "critique," while politically there is a progression of questioning that leads from the seemingly liberal-democratic concerns of the *Rheinische Zeitung* articles of 1842-43 to the more radical social and economic concerns of the later writings.

Viewed strictly as a guide to the questions and concerns of Marx's early writings, Teeple's book is careful, systematic, and reliable. Moreover, the broader project is a good one: the literature on Marx has not focused on the philosophical and political continuities of Marx's early development as carefully as it might. The execution of the project, however, has some significant shortcomings. Teeple is clearly very close to Marx, and wishes to defend the continuity and integrity of his thought. But in many cases his defense is not the kind with which most students of Marx—defenders or not—are likely to be comfortable.

For example, on the question of Marx's critical method Teeple correctly sees the method as involving comparisons between

some kind of notion of the essence of human nature and society, and existing states of affairs. Thus, he points out that in the *Rheinische Zeitung* articles Marx posited a "real and rational" essence to the state, and used this to criticize the existing German state. But when it comes to showing what distinguishes Marx's critical use of the essence/existence distinction from its long history of use by others, we get little help, even though Marx's distinctiveness in this respect is one of Teeple's major claims. He dismisses secondary literature that treats the early Marx in light of his Hegelian milieu and discourse, but in so doing he describes Marx's critical method in pre-Hegelian, pre-critical (that is, pre-Kantian) terms which seem to support what he wishes to deny—that the early Marx was an "idealist." Problems of clarity are repeated for Teeple's characterization of Marx's critical method after the 1843 *Critique of Hegel*, where he claims that Marx takes a less abstract approach, and begins to find the conflict between essence and appearance within reality itself. Again, this is surely correct, and constituent accounts can and have been given. Teeple does little to illuminate the claim, however. Instead he vacillates between Marx's notions of essence (of human nature, society, property, etc.), and empiricist formulations; e.g., "Marx only provides the explanation" of the objective significance of the proletariat (p. 112).

Teeple's thesis regarding the development of Marx's critique of politics displays an insensitivity to current issues in Marxism. He argues that Marx's critique of politics was essentially a 'discovery' that "political relations are but the expression" of historical divisions between the individual's social essence and society (p. 197). Politics is strictly epiphenomenal—something that only occurs in class societies, and will disappear in a communist society. Moreover, according to Teeple, Marx equates politics with the activities of the state, and the state with the interests of the ruling class. Now it is possible to interpret Marx's early understanding of politics in this way, but it is certainly not new, and hardly seems the most defensible or interesting. Politics also has broader meanings in the early Marx: for example, he might be said to be restoring the classical meaning of politics in terms of self-determination. And although Teeple's interpretation produces a "reflex" theory of the state, there is no indica-

tion that this is an important and complex issue in Marx.

The conclusion Teeple draws from his account of Marx's critique of politics is especially likely to irritate political theorists. Modern political theory, he claims, is "nothing but the reflection of contradictions in existing liberal democratic political relations" (p. 212). As such, it essentially describes and justifies modern states. In a variation on Marx's "end of philosophy" theme (which is controversial, and by no means widely accepted even among Marxists), Teeple argues that because the problems of political theory only reflect the problems of capitalism, the demise of capitalism would bring political theory to an end (p. 211). This conclusion does little to reflect the range of problems of modern political theory.

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Human Rights, Ethnicity, and Discrimination.

By Vernon Van Dyke. (Westport, Conn.: Greenwood Press, 1985. Pp. xii + 259. \$35.00.)

Professor Vernon Van Dyke has written a provocative book about an unfamiliar, but important topic—the concept of group or collective communal rights. The principle of non-discrimination has become almost universal, at least in official documents around the world. The U.N. Charter requires members to promote human rights "without distinction as to race, sex, language, or religion." Yet, says Van Dyke, differentiation among groups may be necessary in deeply divided societies, and this differentiation should not be perceived as discrimination against individuals so long as its major purpose is the promotion of human rights. Simple equality of individuals before the law may be insufficient to assure genuinely fair treatment of all members of society. In fact, says Van Dyke, "some interests of individuals cannot be effectively served unless a right is conceded to the group to which individuals belong" (p. 16).

There may be a collision between the notion of individual rights and the rights of individuals as derived from their membership in groups. In the United States this collision has occurred, taking the form of a great debate

over the nature of affirmative action programs on behalf of minorities and women. According to the U.S. Justice Department (circa 1985) most affirmative action programs amount to "reverse discrimination," and are violative of the rights of individuals who are left out of preferential quota arrangements. But even before the Reagan Administration, Nathan Glazer's book, *Affirmative Discrimination* (1975) focused intellectual attention on the risks attendant upon the bestowal of rights upon groups, rather than upon individuals. Preferential treatment for minorities and women in America has been addressed by the Supreme Court which, in spite of convoluted efforts to save some aspects of affirmative action programs, has always avoided recognizing that blacks, women, Hispanics, Indians and other minorities enjoy some form of collective rights to special treatment. On the contrary, the Court has closely scrutinized these programs in order to discern whether they departed from some version of equal protection of the laws. Unfortunately, the Court is badly divided over the meaning of equal protection in the face of numerous public and private programs to give special opportunities to minorities and women. If the Supreme Court followed the Van Dyke argument in favor of some sort of collective group right attached to disadvantaged minorities, it could greatly clarify the precedents in this important area of public policy. However, American individualism is deeply rooted in our political ideology, as well as in our constitutional law.

Van Dyke is probing into a very sensitive area in liberal political theory when he builds his case for group rights. Only the principle of majority rule exemplifies some sort of collective right in liberalism. In spite of theory, Van Dyke demonstrates that the law of many countries accepts certain ethnic communities as "collective juridical personalities" (p. 15). His choices are selective: Belgium, Cyprus, Fiji, India, Malaysia, and South Africa. These countries, unhampered by liberal preconceptions about rights as exclusively individual in nature, have established some form of group rights, usually in the interest of the protection of communal differences of language, religion, or culture. Special political arrangements for minorities were created in all of these nations, although few would regard apartheid in any of

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its incarnations as giving rise to group rights.

The best case for special protections for groups can be made for indigenous people: the Indians of the United States, the Maoris of New Zealand, the "scheduled tribes" of India, the various Indian groups of South America, and the like. These people have usually been overrun by members of a more advanced civilization, and require protections against the further loss of their lands, their health, their culture, and their very lives. However, it is difficult to treat in the same way the South African homelands policy, which creates officially imposed, white-defined boundaries upon the citizenship, livelihood, property, and physical movement of various black peoples. Van Dyke does not defend South African policy, but one wonders what purpose is served by including South Africa as an example of group rights, since the government has done everything possible to avoid allowing black groups to organize to present their special wants and needs.

Political scientists should have been aware of collective rights before Van Dyke prepared this book, but the topic has been largely ignored. The first modern work in this field was Inis L. Claude's *National Minorities*, writ-

ten in 1955 from the perspective of international relations. In 1960 J. A. La Ponce expanded the treatment by concentrating upon legal and constitutional provisions which protected minorities. My own book, *Minority Rights* (1983) covers the ground in greater detail, and develops a definition of minority rights. However, Van Dyke raises the most profound theoretical questions about the implications for individualism of any theory of group rights. The final 30 pages of his last chapter are the fruit of his extended reflection upon this complex and misunderstood issue. In it Van Dyke proposes nine different criteria for assessing the appropriateness of a group rights claim. In an age when Basques, Palestinians, Sikhs, Tamils, fundamentalist Moslem groups, and other active minorities have projected themselves into the headlines, political scientists must take note of the growing significance of claims of group rights. The political salience of this topic is growing. Some readers may not entirely agree with this radical reassessment of liberal theory, but events have overtaken the theory.

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POLITICAL ECONOMY

The Politics of Production: Factory Regimes Under Capitalism and Socialism. By Michael Burawoy. (London: Verso, 1985. Pp. 272. \$8.95.)

Michael Burawoy's *The Politics of Production* is a learned and provocative book which, nevertheless, falls far short of fulfilling its promise. This is perhaps inevitable given its ambitious goal, which is nothing less than an attempt to formulate a general explanation for, as the author puts it, "the intervention of working class in history"—an attempt to specify how and under what conditions the working class has made its presence felt as a political actor. In doing so, Burawoy wants to rescue the working-class from those on the "left" who would either assign it an all-

powerful, messianic role, or ignore it entirely and replace it with sundry new movements like environmentalism or feminism. Burawoy's objective, then, is not only to see the working class as an active creator of its collective life but to specify the forces which determine why, when, how, and with what effects it does so. In his view, these forces are related in one way or another to what he calls the "politics of production."

Burawoy is, of course, not the first scholar to attempt to explain the complexities of the politics of the working class. He is the first, however, to attempt a general systematic analysis of such politics that is based on the labor process and social relations in production. The linchpin of this analysis is the concept of the factory regime. For Burawoy, the

factory regime has various political effects. The political apparatuses of production and the various institutions that shape and regulate struggles at the workplace are the key to understanding the activity of the working class. He goes so far as to claim that "variations in factory regime are sufficient to explain working-class reformism in England and a revolutionary movement in Russia," and implies the same explanatory power for it in relationship to other cases of working-class politics.

The book is an exploration of the different historical forms of factory regime, the determinants of such regimes, and most importantly the political effects that each type of regime may be expected to have on the working class. A series of case studies on England, Russia, the United States, Japan, Hungary, and Zambia puts the theory of factory regimes to the test in a wide variety of historical and political-economic settings. In my view, the theory fails the test, though we learn a great deal that is valuable during that journey.

The journey is endlessly fascinating. Along the way, for instance, Burawoy offers a brilliant discussion of the labor process in capitalism by way of an analysis of the work of Harry Braverman and his critics, and then extends that discussion into one of the most illuminating considerations in the literature on the capitalist mode of production (CMP). He shows, quite convincingly, that the CMP may be distinguished not by the separation of conception and production (as in Braverman), but by the relatively invisible extraction of surplus from the working class. There are also fascinating materials on the unique qualities of the CMP in the United States; short histories of material production in the U.S., England, and Japan; and some of the particulars of work life in places as diverse as pre-revolutionary Russia, Japan, and Zambia in the pre- and post-colonial period.

With all of that said, I am not convinced that the politics of production, and especially the concept and theory of factory regimes, explains as much about working-class politics as Burawoy claims. This is the case because what Burawoy has done in this book is not so much to construct a theory of the politics of production as to provide a diverse set of analytic categories, each illuminating some corner of reality, yet not holding together in

any sort of discernible whole. We are told, for instance, that factory regimes are determined by a complex mixture which includes the labor process, enterprise relations to the market and to the state, the mode of reproduction of labor power, and the place of the national economy in the international economy. Yet we are offered no hint of what weight to assign to each, or of how the parts systematically interact so that findings are generalizable beyond the single case being described. Burawoy's taxonomy of factory regimes, what should be the heart of the analysis, moreover, has a curious ad hoc quality to it as factory regimes proliferate and transform themselves without any comprehensible and systematic conceptual apparatus to help us to understand this multiplication and change. The reader, or at least this reader, is in the end unable to discern what it is that is most critical to Burawoy in the many complex processes that shape these regimes, guide their activities, and determine their effects on the working class. Strangely enough, while I learned a great deal about the labor process, the reproduction of the working-class, the technical division of labor, the forms of labor control, and the like, I cannot say that I know more in a systematic and theoretically parsimonious way about the politics of production and of the working class than before I read the book.

Having said that, let me reiterate that the book remains endlessly fascinating, replete with surprising information and provocative forays into some of the most compelling issues in the social sciences. In this regard, and despite its many problems, I would recommend *The Politics of Production* to a wide audience. Even in its failure, we see the bright promise and aspirations of the best of the social sciences.

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Modeling as Negotiating: The Political Dynamics of Computer Models in the Policy Process. By William H. Dutton and Kenneth L. Kraemer. (Norwood, New Jersey: Ablex Publishing, 1985. Pp. xiii + 261. \$39.50.)

The Dynamics of Computing. By John L. King and Kenneth L. Kraemer. (New York, N.Y.: Columbia University Press, 1985. Pp. xii + 283. \$40.00.)

Against the background of Orwellian nightmares portraying computers as efficient instruments of government oppression and control, these two books examine the routine realities of computer processing and computer-based modeling in local government decision making. One of the studies, *Modeling as Negotiating*, argues that computer models can and actually do promote negotiation and compromise among pluralistic interests in a democratic policy process. This general finding emerged unexpectedly from the authors' case studies and surveys, and it led them to formulate a "consensual" view of modeling as an improvement on the more traditional rationalist, technocratic, and partisan perspectives that dominate the literature. The other study, *The Dynamics of Computing*, surveys local government computing policies and practices during the period 1974-1979 in 40 American cities and 16 cities in nine Organization for Economic Cooperation and Development (OECD) member nations. It finds that computer utilization creates different types of problems and benefits depending on a government's prior experience with computers and its current goals and expectations. Management policies involving technological development, managerial structures, and "socio-technical interface" (e.g., user training) are not panaceas for problems that keep changing with each advancing stage of computer application in the policy process.

Of the two books, *Modeling as Negotiating* is by far the superior work in terms of study design, conceptual clarity, substantive findings, and provocative ideas. The authors base most of their analysis on four case studies (Irvine, CA; San Diego, CA; Tulsa, OK; Anaheim, CA) of local governments that used computerized fiscal impact models in making land-use decisions during the late 1970s. They

also conducted a mail survey of principal analysts and model users in the 21 local governments that employed one of the four fiscal impact models examined. (Fiscal impact models calculate the likely public costs and revenues generated by alternative development patterns associated with proposed general plans, major rezonings, housing developments, proposed freeways, new tax rates, etc.). Departing from most earlier studies, the authors focus on the politics of implementing these models in the policy process, rather than on technical aspects of model development. Their study was guided by two broad questions: Who controls how these models are used?, and, Whose interests are served by them?

Addressing the question of control, the authors conclude that "[n]o one individual or group systematically and uniformly dominates all activities and decision processes" (p. 157). They argue that modeling is "labyrinthian" in the sense that "there are multiple actors whose participation varies from stage to stage, and within stages" (p. 159). The cases clearly demonstrate that modeling "is not simply an activity of the model builders and consultants" (p. 160), and that elected officials, planners, bureaucrats, private developers, and community activists all play important roles in shaping the assumptions, data, and alternatives fed into the models. In adapting models to local circumstances, these actors find their attention increasingly fixed on the computer map rather than the actual territory of their disputes. Model-building unfolds as an iterative, n-person bargaining game and becomes democratized as many hands, in effect, tap at the keyboard to reprogram assumptions, set parameters, and execute runs. According to the authors, this kind of pluralistic "modeling as negotiating" tends to promote consensus as an unintended byproduct of the modeling process itself, and not primarily as a result of the model's outputs. This "consensual perspective" on modeling is offered both as an empirical generalization of their findings and as a normative guide to planners and consultants. They write: "When the modeling process is carried on as an isolated technical exercise or as an obvious partisan maneuver, the models tend to lack relevance or credibility, respectively, in the decision-making process" (p. 211).

Regarding the question of whose interests are served by modeling, the authors conclude that fiscal impact models "appear to systematically advantage those community groups and interests supportive of exclusionary, fiscal zoning policies. Lower-income renters and their advocates are disadvantaged" (p. 197). They worry that routine incorporation of such modeling can lead to the "automation of bias" in local government decision making (p. 215).

I found *Modeling as Negotiating* to be a valuable and insightful study of computer modeling in the policy process. The case studies almost stand alone as interesting and well-told stories of the politics of development. (Sample quote from an Anaheim developer commenting on a computer model's incomprehensibility: "[I]t was so complicated that it was unintelligible to non-experts. It was like handing a copy of *Gray's Anatomy* to a housewife to deal with her kid's cold.") Not only is the book an excellent contribution to the scholarly literature of political science and public administration, it also offers an open harvest of practical advice to those planners and consultants who have computers and will travel in the domain of politics.

The authors of *Modeling as Negotiating* make an interesting comment that is relevant to an evaluation of the second book under review: "Ironically, computers permit incomprehensible models to be built, in that computer programs can handle more variables, calculations, and data elements than any single person could fully comprehend" (p. 173). Computers also permit incomprehensible books to be written, particularly when the computer's numerical output is virtually dumped in undigested heaps on page after page of abstract prose. Despite the authors' worthy scholarly purpose, *The Dynamics of Computing* suffers from near-incomprehensibility in several chapters precisely because of human failure to select, organize, and interpret the machine's abundant output in theoretically meaningful terms. To illustrate, in one chapter three short paragraphs are devoted to an interpretation of two tables containing 346 Pearson correlation coefficients (pp. 119-125). Similar occurrences of statistical mountains accompanied by verbal molehills are found throughout the book. Relationships involving many interesting variables (e.g., degree of data linkage, centralization, and size of EDP staff) are explored using correlation and regression

analysis, but the authors rarely descend to the level of discrete linkages to make theoretical sense of their findings. They prefer to generalize in terms of such "variables" as "management policies," "computing environment," "benefits," and "problems," which are dimensionless vacuities lacking any metric or meaning beyond their service as category labels.

The sampling design of the study appears flawed. For unstated "statistical reasons" (p. 16), only 40 of the original 400 American cities surveyed were included in this analysis, 20 at the high end and 20 at the low end of six policy continua measuring such things as a city's degree of automation and degree of decentralization of computing resources. This selectivity undermines any generalization of findings to the total population of medium-sized and large cities. The study's analytical methodology is not articulated or defended in any one place but becomes manifest only as the book unfolds. Factor analysis is employed in analyzing certain categories of variables, but not others. Partial correlations are shown controlling for city size but not for other variables. Regression analyses were performed, but it is not clear what independent variables were specified in the regression equations. By conventional standards of scientific reporting, the table reporting the regression results (Table 7.1) is almost incoherent.

The authors aspire to answer the question of "what computing, broadly conceived, looks like *in vivo*" (p. 15). In fact, they examine not a single "living" local government system but only scattered traces in the verbal reports of survey respondents. The only "dynamics" in this study are applications of what the authors call "stage theory" to the cross-sectional survey data. Such theories certainly seem necessary in our efforts to track and comprehend the computer revolution, but the particular theory expounded here is rough and sterile. (Here is a sample "implication" derived from the "theory": "[C]omputing development is highly dynamic, but ultimately is controllable through concerted and informed policy action" [pp. 205-206].) On the whole, this is not a book to be recommended.

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What Is Political Economy? A Study of Social Theory and Underdevelopment. By Martin Staniland. (New Haven: Yale University Press, 1985. Pp. xi + 229. \$18.50.)

Martin Staniland attempts to answer the question posed in the title of his book, plus a number of other subsidiary inquiries, the most important of which pertains to the success of competing theories in explaining economic underdevelopment. Both questions are important and deserve reflective responses. His task is great; for many and varied approaches and theories now claim the highly fashionable label of "political economy." While Staniland's essay of little more than 200 pages is brief he manages to provide a good deal of informative and thoughtful concern in a fair-minded manner.

The range of political economy is examined primarily through a series of critiques of writings by mostly well-known contributors to the field. These authors and their analyses are variously classified as examples of "The New Political Economy," "Politicalism," "International Political Economy," and "Marxism." Although I am not a student of underdevelopment, I am both amazed and a bit chagrined at the absence of Peter Bauer's name and work. Those who are included seem eminently appropriate as leading proponents of the four schools of political economy.

According to the author, current concern over the relationship between economics and political science is justified and not just another fad. Appropriate relationships between the two disciplines have been at issue for more than 200 years. As Staniland sees it, most theorists have resolved the hiatus by making one or the other dominant—as is true of Marxism and what he terms "politicalism." In the former instance, politics is simply a product of or determined by economic relationships, while in the latter, markets and economic behavior are the creatures of political, rather than class interests. Some theorists have, of course, seen varying forms of interaction between politics and markets and, therefore, between economics and political science. The "New Political Economy" or "Public Choice" is considered by the author to be but the latest variant in the notion that economics dominates political science. His position seems somewhat confused in at least two respects:

first, the new political economy is considered on a par with much more specialized theories about international dependence when, as a matter of fact, the former is a generalized attempt to explain virtually all human behavior—not simply that of relationships among economies and states. Secondly, the new political economy does not claim the primacy of economic motivations, incentives, institutions over other motivations, etc.; instead, it maintains that the tools of microeconomics can be usefully employed in understanding and predicting human choice, whether found in market or non-market institutions.

Marxism, on the other hand, genuinely insists that economic considerations dominate all others. One of the reasons why I, personally, have never been persuaded by Marxism and its various manifestations in international political economy stems from this basic consideration. Ironically, the new political economy places considerable credence in non-economic factors and, ironically too, the new political economy stresses the substantive primacy of political incentives and institutions and might therefore fall in his category of "politicalism." Furthermore, Staniland does not seem fully to recognize the new political economy as a theory of political failure. Although political institutions and choices are viewed as dominating the private economy, their workings are such as to produce collective inefficiency and inequity not only in the polity but in markets. Robert H. Bates, whom the author considers the most able of the new political economists, surely agrees with his proposition as does Peter Bauer.

The author's understanding of these matters seems flawed because he makes no effort to examine the content of public choice in the area it has explored the most, namely, the internal functioning of western democracy. Scholars working in public choice will find nothing on public choice, proper; i.e., theories pertaining to voting, coalitions, political competition, bureaucracy, campaign strategies, and the supplies of public goods—all staples of the journal *Public Choice*. Had these areas been explored the author would have discovered that public choice is hardly as homogeneous as he appears to believe. Instead he would have found not only competing theories in each of the above-noted areas but,

more importantly, vigorous dissent over basic methodologies. The Buchanans, Tullocks, Rikers, Tollisons, McKelveys, Plotts, and others are as much divided as are other social scientists over the appropriate roles of mathematics, experimentation, polls, etc. Public choice has its positivists and Austrians, its game and exchange theorists. How could it be otherwise? Accordingly, Staniland informs us more on the subtitle than the main title of his essay; we learn more about efforts to explain underdevelopment than we do about political economy and its several schools.

Despite these oversights and possible misunderstandings, Staniland makes a useful contribution to political theory. He raises an important epistemological question about the applicability of modern political economy (ies?) to problems of underdevelopment. While Public Choice has yet to make a major assault on the matter, other theories purporting to explain underdevelopment seem hopelessly ideological. Perhaps, Staniland's small book will perform some consciousness-raising among political economists.

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The Disabled State. By Deborah A. Stone.
(Philadelphia: Temple University Press,
1984. Pp. xiii + 241. \$24.95.)

This is a book about the meaning of disability policy, but on a broader level it is an account of the manner in which societies seek to rationalize difficult distributive decisions. Deborah Stone's thesis in *The Disabled State* is that the disability category constitutes a moveable boundary that is manipulated to regulate access to social welfare benefits. Disability is a socially defined category of welfare that, unlike old age or childhood, can justifiably be defined in a variety of ways. Stone's intention in this research is to illuminate the manner in which disability has developed conceptually, and to explain in that light the intellectual basis of disability policy in "the modern welfare state" (p. 3). She contends that her approach will aid in explaining some facets of disability policy heretofore inadequately treated. Among her major questions are: Why have disability programs grown in size and cost in western democracies? Why has disability determination been increasingly

medicalized over time? Why, even in the presence of medical certification of the disabled, do definitions of what constitutes disability vary among nations? Why are citizens beginning to support cuts in disability programs? and, Why do the costs of and enrollments in disability programs in western democracies increase as unemployment increases?

The author is evidently well-read and imaginative: she seeks to answer these and other questions by drawing upon an eclectic assortment of materials, among them case studies of policy development in Great Britain, Germany, and the United States; empirical health and social policy studies; interviews with physicians and officials of health agencies; transcripts of hearings and meetings, mainly in the U.S. House and Senate; newspapers and other materials from the popular press; and, in the final chapter, speculative scenarios.

Stone's argument is anchored by her contention that the determination of who shall be granted access to the need-based versus rule-based distributive system is the major distributive decision facing societies. Entry into the need-based distributive system guarantees that, on some level, an individual will be taken care of by society. Disability determination presents a difficulty in this case because it may be feigned by malingerers who wish to free-ride. Societies seek some means for screening disability claimants to discourage those who would take advantage, and medicalization is one answer to the dilemma. However, medicalization is not a perfect solution; disability is difficult to measure, and disability determinations are subject to much debate.

Stone's discussion, in Chapter 2, of policy developments in England, Germany, and the United States indicates that virtually all forms of welfare determination are subjective, and that disability is the most subjective among the lot. She casts disability as a policy tool developed for regulating the mobility and composition of the labor force. English poor-law policies were restrictive, reflecting a perceived need to limit the mobility of the poor; German social-insurance policies of Bismarck's era are portrayed as having been designed in part to gain acceptance for the state and to maintain class distinctions. (The Germans distinguished between disability for white-collar workers as opposed to blue-collar

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workers.) Stone notes, rightly, that this was a much a reflection of Germany's rapidly developing industrial and urban economy as it was a desire to keep classes distinct for undemocratic reasons. Her case study of the development of disability policy in the United States, to which she devotes the majority of her text, tells a story of increasing pseudo-rational policy development and ultimately punitive treatment of even the truly disabled.

Stone also gives some space to the problems and politics of disability determination and measurement. Although developments in medical science since 1900 have increased that discipline's legitimacy, it remains virtually impossible to arrive at an objective method for determining disability. Thus there are conflicts in the disability determination process. Claimants and their personal physicians are at times placed in an adversarial role in dealing with agents of the state, be they administrators or physicians. Politicians and other upper level policy makers also face a dilemma: they must find ways to curb disability costs without appearing to be ogres. Each side tries to use medical science as the basis for their arguments, politicizing the role of physicians in the disability process and giving them tremendous power over the distribution of a public good. The process of disability determination is thus very subjective and political. Claimants seek expansion and policy makers must try to limit their access in order to ration public resources.

This conflict leads to increased attempts to make eligibility categories within disability determination more rigid through adjudication and other administrative acts. The advantage gained by covering the bases may be outweighed by the concomitant loss of administrative discretion in disability determination. Stone argues that the rigidity of definitions of disability may in fact contribute to the growth of policies and the perceived crisis: as people come to understand a given misfortune to be a reason for claiming disability, and as the number of misfortunes considered disabling increases, there is an increase in the number of persons who consider benefits an entitlement. Once people are on the disability roles, it is difficult to get them off. Programs grow, and policy makers are thus faced with the unpopular task of rationing access even to the truly needy.

Stone presents a number of interesting ideas

in this book. Her notion of social boundaries is a useful contribution to the study of eligibility determination, and her discussions of developments of welfare policies in England, Germany, and the United States, despite their being focused on disability, are thorough and should prove useful to students of welfare policy. This research also constitutes a useful contribution to the burgeoning "crisis of the welfare state" literature. The author has clearly thought long and hard about the meaning of disability policy and its attendant politics and presents a compelling case for her interpretation.

This is not to say that there are no problems with the book. First, the prose is at times tedious and so serious as to verge on pomposity. For example, "The intellectual problem for the scholar of social policy, therefore, is not simply to ferret out the existence of surrogate measures but to understand how such measures distort social reality and why particular distortions come about" (p. 117) and, "The transformation of the idea of inability to work into the clinical concept of impairment is a fascinating intellectual story" (p. 109). What is a "fascinating intellectual story" and how does it differ from a good tale? The research design is also at times bewildering: she introduces her research with a discussion of the role of disability in the "modern welfare state" (p. 3); presents data showing program growth in seven western democracies (p. 8); moves to a discussion of policy developments in Great Britain, Germany, and the United States; and then devotes the vast majority of the remainder of her text (Chapters 4 through 6) to a discussion of U.S. disability policies. Her rationale for studying the U.S. concept of disability is that "it embodies the clinical concept of disability *in extremis* [Stone's italics] and so presents a magnified version of the central definitional problem of all disability programs" (p. 14). Even though Stone has chosen to grapple with a difficult object of inquiry (the concept of disability) and is necessarily writing at a highly abstract level, I am not convinced that the concept of disability in place in the United States is necessarily comparable to that of, say, Finland, and would have been more swayed by her arguments had a more consistent methodology been applied. Despite these criticisms, this is an important book for students or researchers with an interest in

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social policy and suggests important theoretical work to come.

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The Share Economy: Conquering Stagflation.
By Martin L. Weitzman. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 167. \$15.00.)

Martin Weitzman has written a provocative and original book that outlines how modern capitalist economies can overcome stagflation. What is required, Weitzman argues, is to move from a fixed-wage contract system to a system where employees' compensation is tied to a share of company profits or revenues.

Weitzman traces the roots of our current stagflation to the institutional features of the post-war economy, particularly the rise of wage contracts which tend to fix labor costs in the short run regardless of what happens to the firm's profitability. This feature plus the existence of many large monopolistically competitive firms, which have some power to set prices, is a recipe for stagflation. Consider what happens when there is an unexpected drop in demand for a firm's product. The firm typically reacts by cutting output and laying off workers so as to maintain its monopoly price and profits. Lowering its price would reduce its profits since its unit labor costs are fixed in the short run. The predisposition of the system is for this contraction to be spread across the whole economy because laid off automobile workers cannot afford to buy washing machines and so on.

This tendency towards stagflation in capitalist economies was observed by Keynes, who proposed that government spend to maintain a high level of aggregate demand. To Weitzman the Keynesian solution was a "dazzling digression from the main route to economic prosperity" (p. 54) because it did not tackle the fundamental problem of wage rigidity. Indeed, the full-employment promise of modern welfare states convinces people that contradictions will be brief, and removes the potentially deflationary effects of recessions. Wages and prices increase even during economic contractions. Hence stagflation. Weitzman argues that a share system would permit labor costs to fall

in response to unanticipated declines in demand without producing significant declines in output and employment, and would encourage expansion without creating inflationary tendencies in the economy.

In a share system, large firms will behave quite differently than is currently the case. If there should be an unexpected fall in demand for an industry's product, the firms will now lower price to preserve their markets, knowing that labor costs will also fall sufficiently to permit profits to be made. The adjustments will be on the income side for labor rather than via layoffs and unemployment for some workers. Moreover, firms will actively seek to expand output because they will know that labor costs will eat up only a fixed share of any additional revenues. So even with a decline in the price of the product some profits will still be made. Thus, share firms will forever be "cruising around like vacuum cleaners on wheels, searching in nooks and crannies for extra workers" (pp. 98-99).

According to Weitzman, there are many other good effects of a share economy. For example, with many firms actively seeking additional hires, firms will have to compete on working conditions to attract labor. But can a system with all these good properties work in practice? Is it politically feasible? Weitzman recognizes that the main opposition to a share system would come from high-seniority workers who are currently earning good wages, and who enjoy reasonable security of employment. In the transition to a share system such workers would see their relatively high pay levels fall as they shared their portion of the revenues with additional hires, and also might experience greater variability in their earnings as the economy continued to experience structural changes. To persuade these workers to accept a share system Weitzman recommends an educational campaign "to infuse a sense of social responsibility into the collective-bargaining process" (p. 128) and the use of tax incentives (workers on share contracts would pay lower taxes than those on wage contracts). To qualify for the tax benefits workers must agree not to prevent additional hiring by the firm they work for.

One cannot help being impressed by the power and simplicity of Weitzman's idea. I do believe he underestimates the degree of earnings variability that might obtain in a share

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economy increasingly integrated into the world market. He also underestimates the likely extent of opposition from a large segment of the workforce who have been relatively insulated from the employment effects of recessions. Expecting a large proportion of the labor force to accept more variation in their income, so that five or ten percent of unemployed job seekers can have a job, may be assuming more altruism than our society has conditioned us to possess.

Nevertheless, advanced capitalist economies

are desperately seeking some solution to the current economic doldrums, and Weitzman's idea has the desirable quality of promising much. I suspect that the ideas contained in this book will find a friendly welcome in many social democratic parties (including the Democratic party in the U.S.) and may well form the basis for a reform of many societies' industrial relations systems.

LEON GRUNBERG

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AMERICAN POLITICS

Politicians, Judges, and City Schools. By Joel S. Berke, Margaret E. Goertz, and Richard J. Coley. (New York: Russell Sage Foundation, 1984. Pp. xxii + 278. \$28.00.)

Beyond Busing: Inside the Challenge to Urban Segregation. By Paul R. Dimond. (Ann Arbor: The University of Michigan Press, 1985. Pp. xii + 411. \$29.95.)

These two books have a number of things in common. Each contemplates educational reform in the interest of equalizing educational opportunities. In each, the desirability of reform is taken substantially as a given. These narratives begin posterior to such considerations, and hence rest upon a considerable prior body of assumptions and values. The same can be said of all writing, of course, but because these prior questions of value have been so controversial in recent American history, and because much disagreement persists over them, I am afraid that one's reaction to each of these books will depend more on one's ideology than upon what either actually has to say.

Berke and associates make a cogent statement of the problem. There are great inequalities in the taxable wealth of school districts, in the educational problems that they face, and in the amounts that they spend in support of educational programs. That local school districts are unequally endowed with taxable

resources comes as no surprise, nor that there are tremendous disparities in expenditures per pupil among school districts.

A further source of inequality among districts is the concentration of high-need students in urban school districts, and the more intense competition for tax dollars that characterizes metropolitan areas. When tax bases are compared, for instance, the "big five" districts of New York—New York City, Buffalo, Rochester, Syracuse, and Yonkers—do not appear to be disadvantaged given their relative abundance of commercial tax resources. Nevertheless, these districts have much heavier concentrations of students who make particularly high demands for educational services—students who require bilingual instruction, for instance, and those with educational and physical handicaps. Also, owing to the fact that the provision of routine services is more expensive in heavily populated areas, these districts find it more difficult to generate additional revenues locally to finance programs for high-need students.

This book is informative on a number of grounds. There is a very complete discussion of the types of programs that states have traditionally used to attempt to alleviate financial inequalities between districts. The state educational aid program in place in New York is analyzed in depth, and potential alternatives are exhaustively explored. The last chapter of the book offers a prototype equalization program that would substantially equalize finan-

cial support for educational programs across districts in the state.

All of these considerations make the book quite valuable, but any further promise is lost when, after constructing quite a case for reform, the authors substantially fail to avoid the rocks upon which previous vessels have foundered. The problem has never been an inability to build a case against school district financial inequality. As the authors note, most states have moved against these inequalities by adopting foundation and power-equalizing programs. However, as they further note, such programs have been ineffective in producing any lasting equality among districts (pp. 132-134). Foundation levels are set so low that wealthier districts can and do spend well above the mandated level; and power-equalizing schemes are always flawed by the compromises that are necessary to persuade wealthier school districts to agree to them. The price of wealthy district cooperation is always hold-harmless provisions that protect these districts from reductions in state aid. Once these compromises are built in, equalizing programs do comparatively little equalizing.

The prototype plan offered by the authors promises to overcome these difficulties and actually eliminate interdistrict disparities in New York. They are eventually driven to admit, however, that the cost of this plan is so great that it is difficult to imagine its adoption. Additionally, the cost of equalization produced by a revision in the state-aid formula is borne most heavily by wealthy suburban districts whose funds would be reduced. Their approval and subsequent political passage of such a reform could most probably be had only if hold-harmless provisions were built into the plan. The reader will note that at this point our vessel, like so many before it, has drawn perilously close to the shoals. The authors finally consider revisions to their program, but these require a reduction in the level of state aid and serious damage to the plan's ability to actually equalize across districts. In sum, this is a book that lucidly discusses the issues in school finance and provides valuable reference material, but which offers no new insight concerning the political problems that have traditionally confounded the best efforts of reformers.

Paul Dimond is an attorney rather than a social scientist. Where the work of Berke and

associates is clearly meant to be in the tradition of the literature of policy analysis, Dimond's book has more the quality of an extended memoir. He refers to the principals by their first names (Bill Lamson, Nate Jones, and Lou Lucas), makes fond references to their personal traits, and generally pays no attention to scholarly canons of argument and support. This is one man's perspective on the important litigation over racial equality in public education of the sixties and seventies (though a very interesting perspective it is), and his statement of personal principle and commitment.

The significance of this book should not be underestimated, however. It cogently discusses the notable cases of this twenty year period—*Swann v. Charlotte-Mecklenberg*, *Keyes v. Denver School District*, *Milliken v. Bradley*, *Columbus Board of Education v. Penick*, *Dayton Board of Education v. Brinkman*, and *Delaware Board of Education v. Evans*. The development of precedent in the school desegregation cases is treated quite thoroughly, and there is much elucidation of the processes by which the federal courts arrive at general rules of law, apply them, and choose between them. Those who are interested in constitutional law and judicial policy making will find this book essential reading.

The fact that Dimond does not take a traditional scholarly approach to his subject is a strength as well as a weakness. Political scientists should expose themselves to different approaches to things political every now and then, and particularly when there is a good deal to learn about the texture and structuring of federal litigation. Those who have participated as consultants in federal school desegregation litigation will appreciate Dimond's understanding of the role of social-scientific data in the building of such cases, and the way in which social-scientific arguments are linked to legal reasoning. Social issues are increasingly the subject of litigation in our society, and this book can inform social scientists about the underlying strategies and structures that are not always apparent in published records and opinions.

In considering this book, it should be understood that Mr. Dimond is a man with a purpose. His purpose is to decry the fact that ours is still a racially divided, and in many ways racist, society. He pursues this purpose by recounting this litigation, much of which he par-

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ticipated in directly. Though this litigation was frequently superficially successful in that it resulted in court-ordered integration, blacks and whites are still isolated from each other in their neighborhoods and schools. A proximate cause is the narrow focus of the federal courts in dealing with these questions. This proximate cause, however, rests on an underlying complacency about racism in this country. Racial isolation persists, Dimond says, because "it is easier to feign ignorance and claim innocence than to confront the issue and accept responsibility" (p. 400).

This review began by asserting that there was much that these books have in common. It should end by noting that they also share a common omission. In each book, the structure of educational governance in the United States is frequently implicated. Clearly the source of financial inequality among school districts is the fact that they bear a great deal of the burden for financing their own educational programs combined with the compartmentalization of taxable resources within school district boundaries. Just as clearly, one of the chief reasons for segregation in education is the ability of local populations to create autonomous school districts, and to protect district boundaries by various means against penetration by racial and ethnic minorities. These districts then stand as havens for middle-class whites who wish to avoid court-mandated desegregation within central-city school systems. Given these facts, it is curious that neither author makes any prescription for altering the locally autonomous structure of public school governance. It would seem at the very least that this fragmentation of educational government provides the arenas and points of access that permit parochial interests consistently to frustrate attempts to realize the promise of equality that is integral to our national ideology. In a more general sense, this political atomization is both symptom and cause of a persistent inability to conceive and execute any enduring public purpose.

GREGORY WEIHER

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The Nations Within: The Past and Future of American Indian Sovereignty. By Vine Deloria, Jr. and Clifford M. Lytle. (New York: Pantheon Books, 1984. Pp. 293. \$10.95, paper.)

The Bureau of Indian Affairs. By Theodore W. Taylor. (Boulder, Colo.: Westview Press, 1984. Pp. xiv + 220. \$25.00.)

"The power to tax members and non-Indians is surely an essential attribute of . . . self-government." With those words, the U.S. Supreme Court, just this past April, once again upheld American Indian tribal sovereignty. The Court held that a tax levied by the Navajo Tribe (in its "role as a sovereign," as distinguished from its "role as a commercial partner") on an oil company's reservation leasehold and its gross receipts was valid, even though the tax had not first been approved by the U.S. Secretary of the Interior (*Kerr-McGee Corp. v. Navajo Tribe of Indians et al.*, Slip Opinion, *U.S. Reports*, April 1985, p. 5).

There are about a million and a half American Indians, less than half of whom still live on reservations. But wherever they live, those who are members of the more than 300 recognized tribes are different from any other U.S. ethnic minority: their tribes—unlike the League of United Latin American Citizens or NAACP, for example—are units of government. Our federal system, then, is made up of the national government, the state governments (and within them, of course, local governments), and tribal governments.

Theodore Taylor's book on the Bureau of Indian Affairs (BIA), and the book of Vine Deloria, Jr. and Clifford Lytle on Indian sovereignty are both at their best in chronicling the history of federal Indian law and policy, with Taylor especially detailing well and interestingly the history of the BIA—the loved-loathed agency which, until anti-poverty days, administered just about the entire extent of federal interest in Indians and tribes. Through the years, federal Indian policy has fluctuated from one of conquest (up until 1871), to one of individual assimilation and the allotment of tribal lands (1871-1928), back to an era of tribal reorganization and the strengthening of tribal governments (1928-1946), then again to one of "complete integration" along with the termination of

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governmental recognition for some tribes (1945-1961), and finally to a policy of self-determination and tribal sovereignty (1961-present).

More recently, new tribal assertiveness has produced legal clashes with state and local governments, as well as with non-Indians and business corporations, over such issues as hunting and fishing, zoning, bingo gambling, cigarette and liquor sales, child custody, and taxation—in most of which conflicts the tribes have won out. There have been growing efforts by tribes and state and local governments to negotiate jurisdictional agreements.

During part of the present self-determination era, Theodore Taylor served as a BIA official, and Vine Deloria, Jr., a Standing Rock Sioux, was for three years (1964-1967) the executive director of the National Congress of American Indians. Taylor is now a lecturer and consultant. Deloria, who is most noted for his first book on Indian affairs, *Custer Died for Your Sins*, and his co-author, Clifford Lytle, are professors of political science at the University of Arizona.

The Deloria-Lytle book, which is subtitled *The Past and Future of American Indian Sovereignty*, leaves out too much of the present. Taylor's book, one in a Westview series on federal agencies, is better on this score. The Deloria-Lytle book does make some suggestions for the future—for example, that tribal councils be doubled or tripled in size as a step toward making the three branches of tribal governments more separate and independent from each other. But the Arizonians' book is hard to follow—and seems a little too romantic—when it discusses the future of the conflict between what is called the "realism" of tribal officials and the "idealism" of ill-defined "tribal peoples" or "traditional" members of tribes, in respect to tribal decision making.

There is still a need for a general book on American Indians and government that focuses on the present-day relationship of Native Americans individually to the federal government and on the politics, powers, and governments of Indian tribes in the federal system. Until one appears, these two books are welcome and valuable additions to the literature.

FRED R. HARRIS

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The Politics of the American Civil Liberties Union. By William A. Donohue. (New Brunswick, N.J.: Transaction Books, 1985. Pp. xix + 390. \$29.95, cloth; \$14.95, paper.)

William A. Donohue is a traditional conservative, influenced by writers such as Sidney Hook, Robert Nisbet, and Samuel Huntington. He affirms a commitment to the Bill of Rights, yet as a conservative, he strongly believes that at some point liberty must be balanced with social order if individual freedom is to be realized. As a civil libertarian, Donohue supports the idea of a citizens' litigation and lobbying group to protect the Bill of Rights, but he argues that the American Civil Liberties Union (ACLU) has put much more emphasis on working for the latest political liberal causes than on advancing the cause of civil liberties. Since the late 1960s, the ACLU has put great emphasis on expansive interpretations of civil rights legislation, backing the egalitarian platforms of black, women's, and other social movements of the recent past. Civil rights is not civil liberties, Donohue argues; the ACLU should concentrate on defending the first ten amendments and not put so much emphasis on extending the egalitarian implications of the Fourteenth Amendment.

From its founding in 1920 until 1940, the ACLU, Donovan maintains, was essentially a left-liberal fragment of the labor movement, concentrating on defending the rights of labor unions, while only incidentally expanding the civil liberties of the citizenry as a whole. The Molotov-Ribbentrop Pact put an end to the ACLU's fellow-traveling sympathies to communism, and the organization entered a relatively centrist twenty year period in which it did less for the unions and more for freedom of speech, and was particularly careful to avoid being branded as un-American by adopting a solicitous attitude to the anti-communist and national security concerns of those times. In 1966-67, however, the ACLU once again moved to the left, and since then has incorporated many of the egalitarian concerns of left-liberals into its interpretation of what constitutes civil liberty. In addition, the ACLU developed, in Donohue's eyes, extremist interpretations of freedom of speech (e.g., the Nazi march in Skokie controversy) and of the rights of criminals. The left-liberal platform and

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extremist individualism are likely to discredit the whole cause of civil liberties with the general public, Donohue believes, and accordingly the ACLU may now be doing the Bill of Rights more harm than good.

Most of the book's substance is directed at demonstrating that various ACLU positions, organized by time and category of action, are actually reflective more of the political-liberal notions of the day, rather than of a nonpartisan defense of civil liberties. Donohue has apparently spent two years or more pouring over the entire corpus of the ACLU's own written record, and his charges of liberal cooptation of the ACLU are copiously documented. In this reviewer's opinion, Donohue is basically successful in this major (although limited) goal of demonstrating the importance of liberal ideology within the ACLU. He is not entirely successful, however, due to his mode of presentation. Most of the book consists of a sort of adversarial brief in which Donohue presents his case that various ACLU positions demonstrate an adherence to liberalism, rather than to civil libertarianism. Because the ACLU itself, and several other scholarly authors have already argued the opposite case for nonpartisanship, Donohue sees no particular need to explain why the ACLU sometimes devotes considerable effort to oppose liberal causes, such as its opposition to the post-Watergate federal campaign finance laws. Nor does he explore the rather obvious question of whether the ACLU has recently adopted its own independent interpretation of liberalism, an ideology suitable to its organizational needs and which stresses an extreme individualism in civil liberties, even as many liberals have become more centrist in respect to affirmative action, the rights of criminals, school busing, and pornography during the relatively conservative 1980s. The presentation of Donohue's case as an adversarial brief, not considering at length opposite argumentation, leads the reader to be reluctant to accept the full range of Donohue's claims.

If the ACLU organization was compared to a mixed drink, then, the ACLU itself argues that its potion is nine parts civil liberties to one part liberalism. Donohue, on the other hand, argues that the ACLU mixes about three parts liberalism to one part civil liberties. After perusing Donohue's book, readers are likely to conclude that the ACLU is a half-and-half cocktail.

Most of the book is well-written and interesting. The reader should not be put off by a relatively boring section near the beginning (pp. 56-103) in which the author rehashes familiar arguments about affirmative action programs and other egalitarian goals of recent liberal social movements. After page 123, the book is often fascinating, particularly when Donohue recounts several incidents that illustrate the all too human foibles of various ACLU leaders. Especially interesting are: the narration of the ACLU's attempt to censor the content of an article about itself in the *American Mercury* in 1938; the purge of communists from ACLU leadership positions in 1940; the "palsy-walsy" relationships between ACLU officials and J. Edgar Hoover in the 1950s; and the details of the controversy over the 1977 march in Skokie.

Donohue was not trained in interest group theory, and thus the book will be a disappointment to research specialists in that subject. For the most part the book does not treat questions put forth by Michels, Truman, Olson, Salisbury, etc. There are no references to these theorists or to others of their ilk. James Q. Wilson's studies of crime are cited, but not his research on interest groups. Donohue's book focuses almost exclusively on ideology.

ANDREW S. MCFARLAND

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The Voting Rights Act: Consequences and Implications. Edited by Lorn S. Foster. (New York: Praeger Publishers, 1985. Pp. vi + 208. \$32.95.)

Although they are of uneven quality, Lorn Foster has successfully assembled a collection of essays on the Voting Rights Act of 1965 (VRA) which is required reading for scholars who engage in serious research in the areas of black politics and public policy. The essays seek to assess how and in what ways the VRA has produced changes in the American polity. Some of the essays also provide discussions of the implications of these changes. The essays are presented under five headings, the first of which is the "Overview." Congressman Don

Edwards is the sole contributor. Edwards briefly summarizes the discriminatory actions and practices employed against blacks following the Civil War, the continued voting discrimination against blacks in the 1960s and 1970s, and the controversy that transpired over the extension of VRA in 1982. He argues that there is an imminent and present need for legislation that prohibits racial discrimination in the exercise of the franchise.

The second major heading, "Judicial Impact," begins with an essay by Richard Engstrom, focusing on the development of legal standards to govern vote dilution litigation (e.g., gerrymandering and submergence). Following a lucid analysis of the cases, Engstrom cogently argues that the Supreme Court has pursued a course that limits the activity of the judiciary and the thrust of the federal government under the VRA. That is, the court's requirement of proof of discriminatory purpose provides it (the court) an "escape hatch" from confronting the problems inherent in vote dilution cases. Engstrom argues not for proportional representation, but simply for "neutral treatment." The emphasis of Katherine Butler's essay is the comparison of the Supreme Court's definition of the right to vote under Sections 5 and 2 of the VRA. The comparison is not as sharp as it could be. She argues that the Supreme Court "does not see dilution as an infringement upon the right to vote, at least not the right that is fundamental" (p. 47). Implicitly, Butler suggests that the focus of the VRA should be to provide the open competition of blacks "in the pluralist bazaar of politics."

The third major heading, "Legislative Impact," contains three essays. The first, and perhaps the most innovative in analysis, is Mack Jones's contribution. Using Edelman's *The Symbolic Uses of Politics* as his theoretical basis, Jones argues that the VRA and its implementation constitute intervention strategy, which means "that when a public policy issue involves a confrontation between dominant and subordinate elements, or elites and non-elites, the official action taken by the state will often appear to be differential toward the subordinate element or the nonelites, but the actual implementation may favor the elites" (p. 64). Jones contends that his findings more than adequately support this thesis. Because the VRA constitutes an intervention strategy,

Jones argues that the impact of the VRA has been more symbolic than substantive. Lorn Foster's contribution focuses upon the usage of political symbols by both the proponents and opponents of the extension of the VRA in 1982. In the final essay in section three, Mark Stern examines (1) the extent to which the voting of white Southern House members on the 1982 extension is influenced by the proportion of blacks in the district, and (2) variations in senatorial support for legislation that advances the rights of blacks. Stern's effort reveals some various interesting and provocative findings.

The fourth section, "Administrative Enforcement," contains two well-written essays. In the first, Dale Krane evaluates the implementation of Section 5 of the VRA in light of several myths (e.g., local officials must beg federal officials for clearances, and blacks have unlimited access to the Department of Justice), and from the perspective of local officials. Krane argues that negotiation, advisement, and informal bargaining are the major means of implementation. Hunter's contribution focuses on the VRA and the states' rules governing the right of voters in need of assistance in order to vote.

In the final section, "Electoral Response," Charles Bullock, in a longitudinal study of Atlanta, Georgia, examines the cohesiveness of blacks and whites in support of members of their race in elections. Bullock finds that blacks had greater cohesiveness than whites. Bullock also indicates the circumstances under which black and white cohesiveness decreases or increases. He contends that the slightly greater cohesiveness among blacks may be attributed to the historical discriminatory exclusion of blacks from the political process.

On the whole, the message of the collection of essays is that while the VRA—the most comprehensive and forceful piece of legislation enacted in the 1960s—has made significant inroads in reducing voting discrimination, such efforts must be continued.

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The Dynamics of Foreign Policymaking: The President, the Congress, and the Panama Canal Treaties. By William L. Furlong and Margaret E. Scranton. (Boulder, Colo.: Westview Press, 1984. Pp. xxii + 263. \$19.00, paper.)

This case study of the negotiation, ratification, and implementation to date of the controversial 1977 Panama Canal Treaties is presented within an analytical framework designed to facilitate broader comprehension of the dynamics of American foreign policy making. Utilizing source material drawn from interviews, public records, memoirs, journalistic accounts, and scholarship, Furlong and Scranton employ and integrate three perspectives: organizational—embracing the separation of powers, bureaucratic, and cross-cutting coalition approaches; bargaining; and ecological.

From the alternative vantage points, the pro-treaty authors survey the history of U.S.-Panamanian relations, explanations for the fundamental reorientation of Canal policy embodied in the treaties, and the activities of the occupants of key institutional roles in the treaty process: presidential, bureaucratic, and congressional. Ironically, they undermine the general applicability of their case by demonstrating that from the outset, Panama Canal policy making has deviated from conventional foreign policy making expectations and patterns. The original 1903 treaty's provision for American sovereignty over the Canal transferred it into the domestic arena. Procedurally, congressional participation has always been abnormally high, as has the level of interest and emotional involvement from pressure groups and the general public.

Decades of Panamanian demands for changing the terms of the relationship foundered on the dogmatic U.S. insistence upon maintaining effective control over the Canal. The authors pinpoint the first sign of official willingness to shift U.S. policy toward cooperation and partnership in the 1973 Nixon administration, in response to an internationalization strategy undertaken by Panamanian leaders, whereby the U.N. Security Council met in Panama and discussed the conflict. They place this catalyst in ecological context, amid easing East-West tensions and anti-interventionist sentiments

that were opening a window of opportunity for bargaining.

Administration policy makers elevated the priority assigned to issue resolution. A new chief negotiator was designated in 1973, and the secretary of state concluded an agreement on principles with his Panamanian counterpart in 1974. Within the executive branch, the Departments of State and Defense nominally reconciled long-standing policy differences in 1975, and three separate negotiating rounds with the Panamanians occurred in the periods 1973-75, 1975-76, and in 1977. Along the way, the Nixon administration passed into history, giving way to the Ford, and then the Carter presidencies. A systematic comparison of the three administrations with regard to seven expected executive activities, opportunities for action, actions taken, and impact results in more favorable evaluations for Nixon and Carter, with Ford's lower rating attributed to the politicization of the Canal issue during the 1976 presidential campaign.

Turning to Capitol Hill, the Senate's enthusiastic exercise of constitutional prerogatives, the surprisingly assertive role of the House in foreign affairs policy making, the House-Senate rivalry, and the drawbacks of congressional involvement in treaty making are major themes developed. The authors argue that Senate deliberations "strengthened, clarified, and improved" the treaties, generating a razor-thin margin of consent. In contrast, they disparagingly characterize the actions of the House on subsequent implementation legislation as engendering "antagonism, controversy, and misunderstandings" (p. 163). Again placing events in context, the window of opportunity was closing dramatically by the late 1970s, with heightened concern over communist aggression and revolutionary violence in Latin America. Fortuitous timing thus emerges as the critical factor accounting for the triumph of the treaty proponents in changing policy.

The authors perceive in this treaty battle within and between the executive branch and Congress a clash of world views: liberal versus conservative internationalism; or, more simply, cooperation versus confrontation. Their broader conclusion is that this enduring division precludes attainment of the consensus they hold essential for efficacious foreign policy making in a separation of powers

system. Supplementary reference material includes a chronology, maps, texts of the 1974 Kissinger-Tack Agreement and the 1977 Treaties, and a selected bibliography.

HAROLD F. BASS, JR.

Ouachita Baptist University

The Fraud Control Game: State Responses to Fraud and Abuse in AFDC and Medicaid Programs. By John A. Gardiner and Theodore R. Lyman. (Bloomington: Indiana University Press, 1984. Pp. xvi + 266. \$24.95.)

John Gardiner and Theodore Lyman have written a well balanced study of states' efforts to control fraud in AFDC and Medicaid programs. The issue is an interesting one from normative, methodological, and organizational perspectives. From a normative perspective, one may debate the significance of an estimated \$391 million in overpayments in the \$12.5 billion AFDC program or an estimated \$668 million in improper payments in the \$22.8 billion Medicaid program. A methodologist might explore techniques for accurately measuring fraud and abuse in these programs. Not only is there a serious problem of detection of fraud and abuse, but the programs are complex which leads to difficulty in determining when an over- or underpayment has occurred. Even if one ignores the complexity of the programs, the areas of uncertainty remain. A physician may charge for an "extended," rather than a "limited" consultation, but the difference between the two is not at all clear. Professors Gardiner and Lyman touch upon these normative and methodological perspectives, but it is as a contribution to organizational theory that their study excels.

Borrowing from Norton Long's work on the ecology of games, Professors Gardiner and Lyman note that within each game actors use their resources in following the rules of the game in order to win desired outcomes. In the area of welfare policy, several games affect the character of fraud control policies. Fraud control is affected by the activities of welfare rights groups, poverty lawyers, social work schools, and private charities. All of these groups are dealing with the interests of recipients. Providers of health care, health care

associations, insurance companies, and health departments are concerned with health policy issues and thus play a different game with different policies and goals than welfare-recipient interest groups. Criminal justice agencies are concerned with the processing of fraud cases, a different game entirely from the one of welfare recipients or health care providers. Numerous other games are played in the welfare policy area, and fraud control policies will be affected by the varying mixes of competing interests and goals affecting welfare programs.

As a result, fraud control policies will vary from state to state. Absent a major welfare scandal or other high visibility welfare fraud, control interests would tend to be minimal in relation to other interests pursued by crucial players. Thus, prosecutors would have little interest in minor fraud prosecutions since violent crime prosecutions get their primary attention. Additionally, a successful criminal prosecution of minor fraud offers little payoff. It wastes prosecutors' resources, angers low income voters, has little positive political visibility, and could cost local government more for family upkeep if a welfare mother is jailed. Similarly, welfare officials might tolerate provider fraud in the Medicaid program since elimination of that provider could limit health care for medicaid recipients.

In appendices to the book, Professors Gardiner and Lyman show in detail the variation in fraud control efforts in three states, and there, and in the text, they suggest how some structural changes could enhance fraud control efforts.

In sum, the book is an excellent treatment of how competing interests and goals in a policy area limit effectiveness in achieving one policy goal.

ANTHONY CHAMPAGNE

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The New American Dilemma: Liberal Democracy and School Integration. By Jennifer L. Hochschild. (New Haven and London: Yale University Press, 1984. Pp. xvi + 263. \$27.00, cloth; \$8.95, paper.)

Hochschild's book is compelling in several respects. Her work is neither another mundane

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program evaluation documenting yet another policy failure, nor an illustration of a more sophisticated statistical technique yielding unremarkable findings. Her effort is a refreshing addition to the literature of policy analysis—an artful integration of normative questions and empirical theory aimed at the specification of practical policy recommendations. *The New American Dilemma* illustrates her argument: “We can use the lessons of policy analysis and empirical theory to address our normative problems” (p. xiii).

Through an extensive review of the literature on school desegregation, she clarifies the reasons for program failures in many American schools. Specifically, she concludes that incremental strategies and popular control have generally frustrated the achievement of full-scale school integration. This conclusion clearly underscores the collective intention of halting progress of desegregation. In contrast, many other policy analysts have offered explanations focusing on factors that suggest an absence of community responsibility: white flight, anti-busing sentiments, preferences for educational quality in private schools, or black demands for neighborhood control of schools.

Having diagnosed the problem in typical strategies used for reaching the goal of integration, she is able to generate policy recommendations based on previous school district experiences where success stories involve rapid, massive adjustments in school systems—adjustments often facilitated by unelected federal judges or bureaucrats. As a result, she underscores the utility of policy analysis for determining which strategies are more effective for achieving program goals.

The broad-based design of her policy analysis allows Hochschild to add to an understanding of incrementalism. In this case, her conclusions reinforce the fruitfulness of the concept for accurately describing routine decision making. Additionally, there is evidence from her analysis that some popular control over school district policies actually exists. On the other hand, the argument that incremental strategies will gradually lead to major social change is not confirmed in Hochschild’s interpretation of the evidence. Furthermore, she directly confronts the issue of majority decision making versus minority rights; in other words, she finds little support for a belief that majorities in school districts (often white

majorities) will ultimately embrace meaningful—rather than minimal—school integration.

These two major conclusions lead America to its new dilemma—the question of how to achieve school desegregation, or racial justice for that matter, without relying on the normal procedures: incrementalist strategies and popular control. Hochschild sees the best choice in positive leadership by political elites. On the other hand, she is not without fear that significant change is unlikely: “The big danger is that we will *neither* desegregate successfully *nor* pursue other ways to fulfill the promise of *Brown*. In that case, minorities and Anglos alike will suffer the consequences of our political, theoretical, and moral failings” (p. xiv).

The potential criticisms of Hochschild’s type of policy analysis are both apparent and predictable—her work is a nonquantitative secondary analysis, her conclusions are overly subjective, and her policy recommendations are both unrealistic and undemocratic. On the other hand, I wonder where the critics would find a better policy analysis of desegregation—a better policy analysis, or a better use for policy analysis for that matter. Hochschild’s book may be an important vehicle for increased interest in explicitly including normative questions in policy analysis. She provides a blueprint for doing so without sacrificing either existing empirical theory or real-world politics.

JILL CLARK

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The MX Decision: A New Direction in U.S. Weapons Procurement Policy? By Lauren H. Holland and Robert A. Hoover. (Boulder, Colo.: Westview Press, 1985. Pp. xiv + 289. \$22.50.)

The Making of International Agreements: Congress Confronts the Executive. By Loch K. Johnson. (New York: New York University Press, 1984. Pp. xx + 206. \$30.00.)

Legislating Foreign Policy. Edited by Hoyt Purvis and Steven J. Baker. (Boulder, Colo.: Westview Press, 1984. Pp. x + 229. \$22.00.)

Much has been written in the last decade about the changing relationship between Congress and the executive. The literature has argued, almost without exception, that since the early 1970s Congress has been more willing to challenge the executive on a wide range of issues concerning foreign and defense policy than was the case in the earlier post-World War II period. These three books are additions to that literature and each supports this proposition.

While Holland and Hoover devote considerable attention to congressional-executive relations, their book has the broader purpose of testing to see if the complex procurement of the MX missile is consistent with the bureaucratic-politics model of decision-making. In their descriptive chapters the authors discuss a wide range of issues including the window of vulnerability argument and the foreign policy implications of MX. They also provide a detailed review of the MX's history from its inception in 1973 through the Reagan administration's attempts to find an acceptable basing mode. Their chapter, "MXing It Up in the Great Basin," which discusses the political struggle over basing the MX in Utah and Nevada, tends to place too much emphasis on that aspect of the decision, but this probably reflects that both authors are from universities in Utah. As a case study, this book joins the growing and rather substantial literature on the MX and is a worthy addition to it. The troublesome portion of the book is the authors' effort to test the bureaucratic-politics model.

In developing their version of the bureaucratic-politics model, the authors conclude that, among other failings, this approach does not give sufficient attention to the role of Congress and the public. What they overlook is that this model generally is used to examine behavior in the executive branch and, consequently, is not intended to include other aspects of decision making. While they concede that the literature is "ill-defined" (p. 17), they attribute to it more consensus than exists in order to support their criticisms. Also, there is confusion about the role of the president. At times they assign him the role of being a central figure that dominates the process and at other times give him no more than a persuasive role (p. 14). Based on their criticisms of the bureaucratic conflict model, the authors come close to advocating Graham Allison's rational-policy model.

Another problem with their analysis is that they argue that bureaucratic-politics analysts are wrong in maintaining that "strategic values are of secondary significance to bureaucratic participants in procurement decision making," and they believe that "organizational disputes over weapon procurement are partially explained by *real* differences over strategic matters" (p. 29). This interpretation overlooks the literature on bureaucratic politics which recognizes that there are indeed real, not secondary, differences over policy, and that this is the result of each bureaucratic unit seeing the situation from its own perspective. All is to be forgiven, however, when in the last chapter the authors admit that "In setting ourselves the task of testing the validity of the bureaucratic politics perspective . . . we have of course erected a straw man" (p. 245). The reader feels betrayed and must wonder why so much effort went into analysis of the bureaucratic-politics model if it was only to serve as a straw man. They conclude that of 21 propositions they were testing concerning the bureaucratic politics model, 13 were confirmed by their case study, 3 partially confirmed, and 5 rejected. The approach was, apparently, confirmed.

Johnson's book is divided into two distinct parts, with the first making the greater contribution. After classifying international agreements as to subject matter, Johnson also breaks them down by form and administration. The period covered is 1946-1972. It is unfortunate that his statistics do not go beyond 1972. Instead, his data end just when a more assertive Congress is developing. As to the form of international agreements, instead of the usual comparison of only executive agreements and treaties, Johnson includes statutory agreements—a category of agreements that assures congressional action for implementation. Over the five administrations he covers—Truman through part of Nixon's term—86.7% of all international agreements are statutory agreements and 6% are treaties, both of which require congressional review. Only the remaining 7.4% are executive agreements. From this Johnson concludes that Congress has a greater *procedural* involvement in the making of agreements "than conventional wisdom suggests . . . however one cannot conclude necessarily that the Congress plays a large *substantive* role" (p. 26). In a later chapter, "The Hidden Side of Agreement-Making," Johnson makes clear that the exec-

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utive has made a number of important military executive agreements that have bypassed Congress. Johnson also analyzes the countries with which the United States has made agreements and finds that the U.S. has "tended to enter into agreements chiefly with democratic nations" (p. 52), although the more recent the administration, the higher the percentage of agreements with authoritarian regimes.

In the second part of his book, Johnson reviews the fight over the Bricker Amendment and the Case-Zablocki Act, and then discusses the resurgence of Congress in the making of foreign policy. The information and analysis here is not new, but the discussion is reflective of solid research. His conclusions about a more assertive Congress are, because of the limitations of his data in part one, limited to the arguments of part two.

In editing *Legislating Foreign Policy*, Purvis and Baker are less ambitious in their purpose than the authors of the other two books. Most of the chapters are legislative histories of United States relations with another country, including the congressional role in policy toward Turkey, the Panama Canal treaties, arms sales to country X, the sale of nuclear fuel to India, and the SALT treaties before the Senate. The case studies are well-written, but are researched mainly from government documents and only occasionally mention other works. Purvis, who wrote the introduction, served for many years as an aide to Senator Fulbright, and he and Baker (who wrote the conclusions as well as the chapters evaluating Congress, the nuclear fuel case, and arms control) served as advisors to Majority Leader Byrd. Their experiences and access to these central figures in the Senate is reflected in their commitment to a strong role for the Senate in foreign policy, and they give Congress high scores in its foreign policy activities. In their rating system, Congress is given 13 positive ratings, and 6 qualified positive ratings out of 25 categories. The book says little about the capabilities of Congress to make and review foreign policy, however.

No one of these books is a major contribution to the literature on congressional-executive relations, but, with the exception of the discussion of the bureaucratic-politics approach in Holland and Hoover, each has something to offer.

DAVID N. FARNSWORTH

Wichita State University

Party Identification, Political Behavior and the American Electorate. By Sheldon Kamieniecki. (Westport: Greenwood Press, 1985. Pp. xiv + 288. \$35.00.)

Despite its title, Sheldon Kamieniecki's book is narrowly focused on the conceptualization and measurement of an individual's attachments, feelings, and orientations toward parties, partisanship and independence. Ever since John Petrocik's discovery of "intransitivities" in the seven-point party identification scale, questions have been raised over what this scale, in fact, measures. These questions in turn prompted further questions about the conceptualization of an individual's partisan attachments. These efforts led some researchers to hypothesize that an individual's party attachment is multidimensional in character, a possibility that led to the development of new instruments to tap these dimensions and their inclusion in the 1980 National Election Study. Sheldon Kamieniecki uses these new partisan measures to confirm and refine Herbert Weisberg's multidimensional conceptualization of party attachments. Kamieniecki also explores which measure best taps each of these dimensions.

After first reviewing the various controversies surrounding party identification, Kamieniecki replicates Weisberg's factor analysis of these same variables. With only several minor differences he finds the same four-factor solution. However, an attempt to confirm Weisberg's identification of one factor as a party-system support factor failed since this factor was found to be unrelated to Dennis-like party-system support variables. Since the content of this factor could not be identified, Kamieniecki deleted those variables which had only high loadings on this factor and redid the analysis. Three clean factors emerged: (1) a partisan direction factor defined by high loadings for the seven-point party identification scale, the party support and party closeness scales, and a party difference scale derived from subtracting the Democratic party feeling thermometer from the Republican party feeling thermometer; (2) a partisan strength factor defined by the folded party identification, party support and party closeness scales; and (3) an independence factor defined by the strength of the independence scale and the independents feeling thermometer.

To confirm further this three-factor solution, and the utility and interchangeability of the various measures defining each dimension, Kamieniecki, following the logic of criterion-validity analysis, examines the simple correlations between each of the above measures of partisan direction, partisan strength and independence, and a host of criterion variables. The criterion variables examined included demographic variables, parents' party identification, psychological-political involvement, media attentiveness, party system support, political trust and efficacy, attitudes toward groups and issues, ideology, candidate evaluations, participation, vote choice, and volatility of vote choice. For each category of criterion variables Kamieniecki provides a brief summary of previous findings regarding their relationship with party identification, and notes whether the pattern of correlations supports a multidimensional characterization of party attachments, and whether there are statistically significant differences between the correlations for each criterion variable and the partisan measures defining each factor. This analysis comprises about 75 percent of the entire book. The principal conclusions drawn from this mass of correlation coefficients are: (1) party attachments are multidimensional, and in particular partisan strength and independence are separate dimensions of an individual's partisan attachments; (2) the party difference measure is the best measure of partisan direction since it has the strongest relationship with most criterion variables including vote choice; and (3) for the same reason the folded party identification scale and the strength of independence scale are the best single measures of partisan strength and independence, respectively.

Kamieniecki's research is an exhaustive analysis of the meaning and validity of these measures of partisan attachments. As such, it should be emulated in other areas since all too often we use measures whose meaning and validity have not been determined. On the other hand, his analysis does not offer much that is new, and merely confirms what was already known or strongly suspected about these measures. Even at its own level one might want to object to some of his conclusions. For example, the best measure of each dimension may not be any of the individual measures but the factor scores themselves. The

failure to examine this possibility is a significant limitation of the study. Furthermore, the difference between the correlations for a particular criterion variable and the partisan measures tapping a given dimension rarely exceeded .1—a rather frail base on which to select the best measure of a dimension. Finally, the superiority of the party difference measure may be due to the contamination of the party feeling thermometers by short term forces and the fact that its construction is dependent on the traditional party identification scale to distinguish between those who rate the parties equally on the thermometers.

While this book will be of some interest to party identification specialists, its focus and findings are sufficiently limited that its appeal to a broader audience will also be rather limited.

WILLIAM CLAGGETT

Florida State University

Political Equality in a Democratic Society: Women in the United States. By Mary Lou Kendrigan. (Westport, Conn. and London: Greenwood Press, 1984. Pp. ix + 149. \$27.50.)

This study in a "Contributions in Women's Studies" series is a feminist essay, 89 pages in text, critically evaluating equality as an essential, irreducible element on the feminist agenda. In a systematic, scholarly and readable fashion, it performs a service to committed feminists who must come to understand fully the substantive demands of their politics, and to non- or uncommitted others who want simply to understand the nature of the feminist challenge to our political theory and institutions. Ms. Kendrigan dispels the gloomy notion that in the confrontation with traditional democratic theory, feminism is too theoretically dissipated to contribute much. She clears the air about the concept of equality by determining its variable meanings and their policy consequences, and initiates some systematic thinking, long overdue, which might have prevented the more egregious policy fiascoes undertaken in the name of equality. The work is a model of feminist methodology, combining theoretical and empirical analyses of equality, on one level, with programmatic

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advocacy of an equality of rights, on another.

A pragmatic feminist, Ms. Kendrigan adopts a nonabsolutist approach best labeled "Taking Equality Seriously, and Moderately," in which she shows how the cumulative social, political, and economic inequalities found in American society reinforce each other to keep women down. In the women's rights tradition, her remedy is political equality based upon the effective representation of women's interests, through a politics wherein the sex of the participants is irrelevant to power. Her political activism is strewn with bottom-line advice and modest expectations, e.g., a concluding caveat that the best pro-feminist policy attainable may be only marginally successful.

The author's argument is a familiar one. She extensively documents existing political, social, and economic inequalities: a cycle of political oppression, restrictive cultural mandates, and occupational segregation that destroys women's persons and potential. Anticipating the objections of those optimistic about the considerable social change recently, she enlists the sociological concepts of stratification and institutional sexism to explain how system persistence can neutralize if not negate surging social forces. Change-induced differences in society need not represent the increased equalities for which feminists hope.

The analysis of equality represents the strongest part of the essay. Ms. Kendrigan distills the concept into three "essences." One is equal consideration of interest (or, no distinctions except upon proper justification), whose inadequacies are a "negative egalitarianism" and a blindly benign attitude to the status quo. Another is equality of opportunity (or, equal rights and equal rewards—fair rules), likewise inadequate because it works best only in the best case: one of initially equivalent circumstances. The real-life question of equal treatment in the face of differences disarms it. Finally, there is equality of results, the recommended choice for feminists. It seeks, apparently, an equality sufficient to allow equitable participation of both sexes in public life. By stressing similarities and treating differences differently when the common/particular good requires it can decrease inequalities and increase equalities. The ultimate theoretical justification is an irreducible democratic faith in the community of human-

ity; the ultimate empirical justification, the adjustment of feminist political behavior to real politics by a focus on policy outcomes.

The conclusion specifies the political activities requisite to eventual political equality: ideological, self-interested voting by women; electing feminists who produce at least some significant policy changes; and sustaining the women's movement as the organizational basis of the entire endeavor—in effect, the political equivalent of a vertical merger. The author has successfully presented a feminist critique of democratic theory from the viewpoint of the powerless and has clarified a vital segment of feminist politics.

A critique of equality of results deserves another essay. More manageable is a note on the weakness of the post-analytical discussion on political strategy. Substantive vs. procedural, process vs. substance are familiar and well-taken distinctions. Here they lead to disappointingly familiar conclusions: process is important but policy content and results are ultimate proof of quality. For all its timelessness, advice that feminists must be vigilant and must continuously monitor and evaluate policy is anticlimactic. It adds nothing to either the decade or more of labors nearly preempted by the policy-analysis subfield of the discipline, or to the Eastonian systems model introduced over 20 years ago. The essay still deserves a wide reading, certainly wider than its prohibitive price will allow.

MARY R. MATTINGLY

Texas A&I University

American Arms Supermarket. By Michael T. Klare. (Austin: University of Texas Press, 1984. Pp. xii + 250. \$10.95, paper.)

With all the attention paid to arms racing and conflict resolving by scholars who investigate the causes of international competition and war occurrence, the subject of arms transfers is often treated as ancillary to national security analyses whose focus concentrates on nuclear armaments, superpower rivalries, or crisis management. To be sure, arms races, conflict resolution, and war occurrences are in part based on the possession of weapons and their accessories (suppliers, spare parts, training and maintenance, etc.) obtained

by national development or international access, but the literature on arms transfer is only now growing in quality and quantity to bring this critical aspect of world politics and economics into perspective. Readers familiar with Andrew J. Pierre and Anthony Sampson, for example, will welcome Michael T. Klare into this circle.

In *American Arms Supermarket*, Michael T. Klare has produced a tour de force. Beginning with a set of empirical questions and employing a cluster of factors to guide the discussion, he presents a comprehensive history of U.S. foreign policy from the early 1950s to the mid-1980s wherein arms transfers are seen as an increasingly important instrument of American national security with respect to Europe and the Third World (especially the Middle East and Latin America). Klare analyzes the historical, political, economic, and psychological assumptions behind this country's arms policies toward allies and potential allies, and, more importantly, why this instrument does more harm to U.S. interests in the long term than good.

There are three models either explicit or implicit in Klare's study: the bureaucratic politics model, the dependency model, and the interdependency model. The bureaucratic politics model helps us sort out the many political actors involved in American arms policy making (State, Defense, National Security Council, Central Intelligence Agency, Arms Control & Disarmament Agency, Commerce, and Treasury, to name the well-known but not all the important ones such as International Security Affairs, Defense Security Assistance Agency, Military Assistance Advisory Groups in the Defense Department, or Politico-Military Affairs in State, etc.), first in the executive branch, and then in presidential-congressional relations over arms transfers. The dependency model is used to show how the United States in the 1950s and early 1960s once used arms transfers to influence allies and clients in ways generally favorable to American security needs. Today, on the other hand, this policy seems to make Washington dependent on its allies and clients, being careful with these arms transfers not to set off conflagrations in volatile regions like the Middle East or Latin America. The interdependency model assesses two phenomena involving arms transfers: coproduction along with technology infu-

sions from this nation to other nations; and control by first having a new policy agenda, and then working closely with other arms powers to slow down arms transfers that clearly contribute to regional instability or violence. In all, this book is a benchmark of research, analysis, and utility that will be cited in the literature of national security wherever arms transfers are the subject of controversy.

JOSEPH RICHARD GOLDMAN

University of Minnesota

Television's Window on the World. By James F. Larson. (Norwood, N.J.: Ablex Publishing Corp., 1984. Pp. x + 195. \$27.50, cloth; \$17.95, paper.)

With its genesis the author's 1978 Ph.D. thesis, Larson presents a painstaking study of a random sample of 13% (some 1,000 broadcasts) of the coverage of international affairs on the weeknight evening news shows of the three U.S. television networks from 1972 through 1981. He finds that 40% of the evening news—seven out of 17 stories on an average night—falls into the international affairs category (defined as a "news story that mentions a country other than the United States, regardless of its thematic content or dateline" (p. 36). Of these international stories, 42% consisted of the anchorman reading an item, usually from a wire service, with a still picture, map, diagram, etc. in the background. Another 26% were domestic video reports originating live or taped from the White House, State Department, Congress, the Pentagon, or some simulation thereof. Only 32% originated from outside the U.S.; they were normally prepared by network correspondents and transmitted by satellite.

Digging deeper into his data, Larson shows "that the overall pattern of network news attention is highly skewed toward a small number of nations" (p. 55). Right at the top, in fact, with some 60%, is the U.S. itself, followed by the USSR (17%), Israel (14%), and then Britain, South Vietnam, Iran, Egypt, North Vietnam (after 1976 all references to Vietnam were coded North Vietnam), France, and China, with between 10 and 5% each. No other state had more than 5%. The nations appearing most often "were involved in wars

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or major conflicts" (p. 57), and/or were major world powers.

Most coverage originating overseas comes from Western Europe and the Middle East. With the exception of major crises, or when events there involve the U.S. or other developed states, "the U.S. television networks have paid minimal attention to Latin America, Africa, South Asia, Southeast Asia following the Vietnam war, and Eastern Europe if the USSR is excluded" (p. 62). And quite often coverage of the Third World, especially Africa, occurs only when there is a crisis (defined, p. 69, as unrest and dissent; war, terrorism, and violent crime; coups and assassinations; and disasters). Seeking to explain these coverage patterns, Larson discusses the increasing use of relatively cheap satellite communication; the global deployment of network bureaus and correspondents; and the availability to the networks of wire service reports. Of these, network bureau location is the strongest predictor of coverage. Also mentioned, but not considered in any detail, are news values, access (North Korea does not welcome U.S. television reporters), and foreign censorship.

The penultimate chapter is a cursory and too brief attempt by Larson to explore the implications of his data for the foreign-policy process. It does, however, contain the provocative observation, worth pursuing, that "the lack of network news attention to issues of social change and development in the Third World gives U.S. policymakers broad leeway to ignore, minimize, or postpone consideration of such problems" (p. 143). Larson concludes by asking whether television can portray the urgent struggle for health, education and agriculture in developing countries and show the American public how it is involved in such problems.

The book's distinctiveness is that it is a longitudinal study that subsumes (on a sample basis) all coverage of international affairs. The author mines his data with diligence and care. He is able to confirm that the networks' evening news shows are less a window on the world than a shadowy glass. And he shows us the shape and size of the glass.

The work suffers from two major problems: inadequacies of content analysis, and circumspection. First the content: the subject is television news, but there is no analysis of the

visuals—of what viewers actually saw. Worse, there is no discussion of the stories' actual content, of their approach and perspective. We learn little about how the events in question are depicted. Only because of such limitations can Larson report that coverage does not differ from one network to another when in fact a recent book by Dan Nimmo and James Combs (*Nightly Horrors*, University of Tennessee Press, 1985), documents quite dramatic differences.

Perhaps because his data are so limited, Larson seems reluctant to venture beyond them. Thus the key question: "How completely and how well have [the three networks] . . . conveyed information about international affairs to the American public?" (p. 2) is posed, but goes essentially unanswered. Answering it would require the author to consider such topics as what should be covered, how the networks should deploy their resources, and what types of international affairs information would best serve the American public.

DAVID PALETZ

Duke University

Religion and the Presidential Election. By Paul J. Lopatto. (New York: Praeger Publishers, 1985. Pp. xi + 177. \$32.95.)

This book makes a positive contribution to our understanding of the role religion plays in American electoral politics. It attacks the conventional wisdom among political scientists that the voting patterns of American religious groups are not particularly distinctive and attempts "to establish religion as a major explanatory variable in the study of American presidential voting behavior" (p. viii). While this book is not likely by itself to accomplish such a goal, it does demonstrate that when political scientists approach the relationship between religion and voting behavior with the same degree of sensitivity to subject matter and thoroughness in analysis as is generally applied to the study of other relationships, the persistence and importance of religion-based differences in presidential voting patterns become apparent.

Lopatto begins his book with a relatively brief discussion of the role of religion in American political history. This discussion

serves as a context for the development of theoretical propositions that attempt to explain variation in the linkage between religion and presidential voting over time. Lopatto contends that such variation is due to both short-term factors (the particular selection of their presidential candidate by each party) and long-term factors (social, economic, and demographic changes at the mass level).

In the second chapter, Lopatto develops a denominational classification scheme differentiating between and among liberal, moderate, and conservative Protestants and Catholics on the basis of differences in religious beliefs. The remaining five chapters examine presidential voting patterns, and how attitudes toward the presidential candidates, attitudes toward major political issues, and demographic changes relate to such patterns within and across the four denominational categories through an analysis of data from the six national presidential election studies from 1960 through 1980.

Analysis revealed that religion plays a major role in affecting voting behavior by first affecting the development of attitudes toward many of the major issues of the day. While evidence linking religion with presidential voting was quite strong, the direction and impact of religion varied over time. In fact, based upon "normal vote" projections, the impact of religion appeared to be less in the 1976 and 1980 presidential elections than had been the case for the previous four presidential elections.

Does this apparent decline of the influence of religion in the 1976 and 1980 presidential elections suggest a weakening of the hold of religion in American electoral behavior? Not necessarily. Lopatto's analysis suggests that a great deal of variation in the magnitude of the linkage between religion and voting behavior is due to the changing nature of presidential nominees from one election to the next. Thus, the particular impact that religion had, for example, in the 1980 presidential election would likely have been different had Kennedy, rather than Carter, been the nominee of the Democratic party. Moreover, given the Reagan-Mondale pairing in 1984, it is likely that a replication of Lopatto's analysis for the 1984 election would reveal an increase in the religious impact upon presidential voting

—with conservative Protestants voting more Republican and liberal Protestants more Democratic than might be expected on the basis of normal vote projections.

Unfortunately, the book gives no attention to the impact of differential rates of religious involvement for each of the denominational categories. Similarly, denominational classifications tell us little, if anything, about the impact of religion upon presidential voting for those religious groups that may be found within each of the four denominational categories (e.g., evangelicals v. charismatics). Thus, while religion may appear to have had minimal influence on voting patterns in 1980 based upon normal vote projections for the four denominational categories, its influence might have appeared greater had different religious groupings been analyzed. Nevertheless, this book represents an important contribution to the literature on the topic and deserves the attention of all those working in the area of American electoral behavior.

CORWIN SMIDT

Calvin College

The U.S. Anti-Apartheid Movement: Local Activism in Global Politics. By Janice Love. (New York: Praeger Publishers, 1985. Pp. xvii + 296. \$38.95.)

The primary contribution of this study is able description along with systematic and judicious analysis of the roles and effectiveness in American politics as well as governmental and private decision making of domestic groups that favor termination of U.S. investment in and trade with the Republic of South Africa. While the author's sympathies clearly lie with these groups and their objectives, her interpretations are generally balanced and her judgments realistic. This book, moreover, provides considerable useful data not only on the composition, objectives, strategies, and activities of these diverse groups, but also on the contexts of U.S. investment, trade, and other linkages with South Africa which they hope to change. It also includes an excellent bibliography pertinent to the movement, its political milieu, and evaluations of the effectiveness of such social movements in general.

The book begins with an overview of the so-

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defined anti-apartheid movement, its international and domestic environments, its perceptions of South Africa and of changes in U.S. behavior apt to encourage desirable changes there, and its several objectives. A second chapter examines evolution of the sanctions debate both in the U.S. executive and legislative branches and in private life. The author then considers alternative criteria for evaluation and frameworks for analysis of the movement's impacts on policy making and on the American body politic more broadly. She thereafter applies her chosen goal-based criteria and evaluation design to the pertinent legislation which was enacted into law, or attempted, in two states: Connecticut and Michigan. These case studies included careful observation of the behavior of the respective legislatures and executives and interviews with most of the significant actors both in these state legislatures and governments and among the anti-apartheid supporters and their opposing interest groups.

By 1985, Michigan and Connecticut had enacted the strongest legislation regarding South Africa among state governments. However, the principal allies of the groups favoring state-enacted economic sanctions, the institutions to which they were applied, and the types of sanctions varied considerably between these two states. Even there, pro-sanction groups were unable to achieve much divestment or other sanctions applicable to private enterprises that had signed and were making apparently meaningful efforts to comply with the Sullivan Principles for American investment in South Africa. Consensus could be achieved on legislating only marginally beyond those principles, which the anti-apartheid movement regarded as little effective in bringing about significant change in South Africa. The movement succeeded in achieving passage of broader sanctions on investments by state pension and insurance plans by the Connecticut legislature, largely due to the business community's false impression that the legislation would not pass. But once passed, opposing commercial and other interest groups prevailed upon the Democratic governor to veto the legislation. A broadly representative task force appointed by the governor thereafter recommended a more limited bill, the essence of which did become law.

As the author notes, the relevant political

and economic environments vary greatly from state to state. Short of considerably accelerated intensification of the black-white conflict in South Africa (which seemed more probable in August, 1985, than before), it seems doubtful that legislation much stronger or more inclusive than that in Michigan could become law in other states, or that any as effective as that in Michigan could be enacted in more than a few others. As the author implies, such states are apt to be governed by relatively liberal, Democratically controlled legislatures and governors, to be highly industrialized, and to have politically effective black elites, strong trade unions, world-class universities and other educational, research, and cultural institutions.

ALFRED O. HERO, JR.

Belle Chasse, La.

The Personal President: Power Invested, Promise Unfulfilled. By Theodore Lowi. (Ithaca, N.Y.: Cornell University Press, 1985. Pp. vi + 221. \$19.95.)

Theodore Lowi, Professor of American Institutions at Cornell, has added another important book to an impressive collection. In the *Personal President: Power Invested, Promise Unfulfilled*, Lowi takes an historical and institutional approach in developing his summation of the presidency as too reliant on the slippery base of popular impressions. The "plebiscitary presidency" has emerged as political parties have atrophied and Congress has passed along a larger share of government's expanded obligations to the president. What is left is a personal presidency serving as the direct focus of popular demands. Every president, regardless of character, is therefore eventually deceitful in trying to raise expectations in order to win, to hold power, and to "deliver" on public promises. Inevitably expectations are elevated past the point of realization and failure is thereby guaranteed.

How did this state of affairs come about? Until the New Deal, the national government performed essentially patronage functions. However, during the transition period of the 1930s and 40s, the national government turned toward more regulatory and redistributive initiatives. This meant new governmental

obligations, a longer reach for government, a rise of interest groups at the expense of parties, and a shift in program responsibilities from Congress to the president. It also meant a new theory of democracy with the presidency as its centerpiece. The legacy of Franklin D. Roosevelt and the decline of the mediating influence of parties and Congress left recent presidential candidates and incumbents personally responsive to the public as demands on big government multiplied.

While the investment of power in the president's hands democratized government, thereby increasing its concerns and initiatives, the costs have outweighed the benefits. The limits of solvency are quickly reached, for instance, if every program is seen as worthwhile in a large liberal state. Furthermore, the credibility of the presidency suffers as even partial success is defined as failure against an unfair standard of the president's own making—namely, inflated public expectations. Worse yet is the threat of foreign disaster as the president seeks to regain public stature by at least appearing to be bold in international affairs.

How can the problems of the personal presidency be resolved in favor of collective responsibility? Lowi rejects remedies such as the war powers or budgetary initiatives of Congress, a six-year presidential term, a national primary, and a stronger cabinet. He also disposes of several two-party "myths," but he does think a multi-party system would help. It could force the president to include Congress in his or her constituency by threatening to resolve an election in the House. Relatedly, the president would have to develop a party coalition in Congress and would thereby give new life to the role of both party and Congress.

Real parties would help to build down the presidency by affecting elections and the role of Congress and the cabinet. Collective responsibility and a more parliamentary presidency would follow. But the starting point is not structural reform. Ironically, Lowi's solution for the institutional problems of the plebiscitary president begins with an individual president's understanding of the problems and willingness to cope with them.

This is ironic because it assumes presidential behavior eventually controls institutional forms, whereas Lowi's thesis assumes the reverse. In fact, Lowi's analysis discards the

individual president along with any character explanations of presidential behavior. Intuitively, one assumes the deceit inherent in the personal presidency would be made worse by some presidents than by others. Instead, the Lowi model even seems forced in places, with Richard Nixon described as "consistent, logical, and normal" (p. 179).

Lowi also seems to stress the "power invested" subthesis, although more could be said about the complications of fulfilling promises than the seesaw of public aspirations and disappointments. For instance, while he discusses the shift of power from Congress to the presidency, he says less about the atomization of Congress as this, together with what is left of its checks and balances role, affects the president's ability to "deliver."

Criticisms of a work as bold and lively as this one are inevitable. What must be said in its favor is that the analysis is insightful, intelligent, and compelling. Lowi's summation is more than an academic exercise. It is intended to make a difference. What is certain is that it will not be ignored.

JAMES A. DAVIS

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Due Process in the Administrative State. By Jerry L. Mashaw. (New Haven: Yale University Press, 1985. Pp. xiv + 271. \$24.00.)

This admirable book incidentally illustrates the extent to which law professors like its author have mastered the relevant social science materials and now dominate the study of administrative law and regulatory processes. Paradoxically, the rise of behaviorism, with its presumed emphasis on grass-roots activity, resulted in the decline of the study of public administration. Administrative law was typed as formalism. Organizational case studies so beloved of sociologists were perceived as suspect in initial selection and conclusions. Decision-making analysis led many, not merely its progenitor, out of political science and into psychology. Only with the emergence of public choice analysis of regulation—a body of work well-cited by Mashaw—have political scientists come back to these fruitful areas.

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In the interim, administrative law professors—always more pragmatically students of regulatory behavior (as opposed to court doctrinal analysis) than most of their colleagues—have turned to social science, especially economics, to deepen their understanding. Mashaw, who is identified with two of the prime centers of that confluence—Yale and Virginia Law Schools—gives us an analysis of the due process clause holdings of the Supreme Court. But it is also an analysis of the proper domain of courts, legislatures, and administrative agencies. It is a counterpart of roughly similar constitutional analyses by Ely, Choper, and Tribe. Essentially, *Due Process in the Administrative State* is a critique of current Supreme Court doctrine and Mashaw's suggested remedy. The critique is developed through his delineation of three models of due process and adjudication: appropriateness, competence, and dignitary rights.

By appropriateness, Mashaw includes notions derived from legal precedent and past practice, as well as legal reasoning about what is due (appropriate) process in any situation. He suggests this was the prevailing model until the Warren era. By competence, he means an approach which emphasizes interest-group balancing, response of the courts to legislated cues as to authority distribution ("positivism") and instrumental utilitarian assessment. This is traced to the welfare benefits case of *Goldberg v. Kelly* (1970) and defined in *Mathews v. Eldridge* (1976). The latter case sets out yet another of the Supreme Court's myriad three-pronged tests; (1) the private interest; (2) the risk of error and the degree additional procedure would reduce error, and (3) governmental costs.

Mashaw shows that neither of these two models is complete in itself and that even the Supreme Court does not consistently decide within the parameters of its own stated tests, never mind Mashaw's constructs. The missing element is his third model—dignitary rights—admittedly a restatement of natural law, which Mashaw regards as essentially nonsense but terribly useful. The model of dignitary rights helps explain otherwise anomalous decisions and actually channelizes and minimizes judicial discretion. The model of appropriateness provides little guidance when precedents are in conflict or ambiguous. The model of competence permits, even requires the courts

to smuggle in conclusions about policy that it derives by unknown methods, but which somehow are superior to legislative knowledge. In contrast, the model of dignitary rights suggests normal deference to political determination, and exceptions must be asserted in strong justifications limited to certain sectors of rights.

This is not an earthshaking argument. The strength of the book is in its compact summary of much relevant data and reasoning that goes far beyond doctrinal analysis. The writing is generally spritely and lucid, only occasionally lapsing into pretentiousness. There are good summaries of Richard Stewart's delineation of eras of administrative legitimation, the relevance of majority rule analyses for bureaucratic discretion, and Kantian and Rawlsian justifications of individual dignity as a decision rule. The introductory chapter is rich in data on the growth of due process cases and a fair smattering of public choice literature on bureaucracy is adverted to in the later chapters. In short, it is a good introduction to administrative law considerations generally, even though it has the focus of a monograph.

Its major weakness lies in the use of models, an approach law processors have overmined probably because of the pervasive influence of Packer's two models of the criminal justice system. The weakness is inherent in all ideal-type analysis which is largely an excogitated product of the model-builders' imagination. Such projected super-principles are excellent didactic devices and, no doubt, good antidotes to the lawyer's disease of super-particularistic analysis of each little case. Yet judges, by and large, do not operate with synoptic vision; they are themselves transplanted lawyers as a rule, rather than misplaced philosophers. Having created the models at will, the builders generally can manipulate them in almost any way they wish since they know their weakness and flaws as well as their intended strengths. Certainly no justice that I can recall denies that the *Eldridge* test should include "dignitary rights" though his or her emphasis or devotion varies. Mashaw's definitions embed his conclusions, as do Tribe's and Packer's.

It is intriguing to compare Mashaw's reversion to natural law thinking (in spite of his expressed aversion to it) with the revival of similar doctrine in recent English administrative law. In spite of an even stronger tradition

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of parliamentary supremacy and legal positivism than our own, English courts have in this past decade or so found that "natural law" doctrines of previous centuries can be used to constrain parliamentary delegation of absolute discretion. It is, however, the appropriateness model they have usually relied on, though their decisions, even more than ours, blur any such distinction. As Mashaw suggests, any argument ultimately reducible to "what kind of a government is this anyway" fits under due process and also serves to bring into play general notions of decent procedure for the English.

In so careful a book, the carelessness with cited names is jarring. Even Mashaw's collaborator in another volume does not avoid misspelling. But this, like the excessive use of the first-person singular, are minuscule flaws in a very welcome and exceptionally thoughtful work. The volume also has the potential of helping reawaken the current crop of political scientists to the fact that virtual abandonment of the regulatory area as a research focus was a grievous misunderstanding of the nature and working of modern government.

SAMUEL KRISLOV

University of Minnesota

The U.S. Intelligence Community. By Jeffrey T. Richelson. (Cambridge, Mass.: Ballinger Publishing Company, 1985. Pp. xxv + 358. \$39.95, reference; \$16.95, text.)

The United States intelligence community has now been written about from a number of perspectives. Memoirs and exposes still appear with regularity, but they no longer dominate the literature. One current approach focuses on personalities in charting the growth and development of the intelligence community. Figures such as Allen Dulles, Richard Helms, and James Angleton have served as the centerpiece of such studies. A second approach emphasizes the functional tasks of the intelligence community: intelligence estimating, covert action, or counterintelligence. A third approach places the study of intelligence in the broader context of the American political system. Its focus is on such questions as how and why is the intelligence community politicized, and is it under control? A fourth

perspective examines the intelligence community from an organizational perspective asking questions about its structure and the impact of its structure on its performance.

Jeffrey Richelson's book falls into this last category. Where most organizational accounts of the intelligence community operate at a relatively high level of generality, this book delves into the minutiae of organizational charts, lines of responsibility, and institutional change. Moreover, Richelson adopts a broad definition of the intelligence community. His detailed account includes coverage of the military service intelligence organizations, Defense Department intelligence organizations, the intelligence components of the Unified and Specified Commands, and such civilian departments as Agriculture, Energy, and the Drug Enforcement Administration. Richelson lays out in the same painstaking detail the technological and management systems utilized for imagery and signals intelligence, ocean and space surveillance, and nuclear detonation monitoring. The book also includes excellent chapters on cooperative international intelligence agreements, and analysis and estimates.

Richelson has made a major contribution to the study of the U.S. intelligence community by skillfully bringing together in one volume a body of material that is either unavailable to most observers or would require an immense investment of time and effort to obtain. This work should not be seen as a primer on the intelligence community. The nine pages of abbreviations and acronyms following the table of contents alert the reader to the depth of detail that he or she will confront. The brevity of the discussions of intelligence in the first chapter and that of issues in the concluding chapter also indicate that this is a work most useful for those already familiar with the main themes and issues in the literature.

This book is also important for the study of the U.S. intelligence community by revealing just how far we have come over the past decade and how much more still needs to be done. Through the exhaustive use of government documents, the Freedom of Information Act, and personal contacts, Richelson is able to present a detailed if somewhat mechanical account of management practices and standard operating procedures deep within intelligence organizations. The next requirement is to add

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a human element to the narrative describing how the system actually works. The data needed for this is unlikely soon to be available. In discussing national intelligence organizations, Richelson must also often fall back on budgetary and personnel figures cited in Marchetti and Marks, newspapers, and magazines. Not only has the accuracy of these figures been challenged, but making meaningful comparisons is difficult. Typically, the figures are for different years, and little guidance is given as to what is included in the calculations. This lack of current information continues to plague the study of intelligence. To his credit, Richelson acknowledges these problems when they arise and seeks to steer the reader around them.

GLENN P. HASTEDT

James Madison University

Environmental Policy Under Reagan's Executive Order: The Role of Cost-Benefit Analysis. Edited by V. Kerry Smith. (Chapel Hill: University of North Carolina Press, 1984. Pp. xii + 266. \$25.00.)

The great expansion of the regulatory powers and responsibilities of federal agencies in the United States during the 1970s generated increased efforts by presidents to monitor and direct the decisions and actions of these agencies. The Nixon administration focused its attention on the power of the Environmental Protection Agency to impose regulations on industry that resulted in significant compliance costs. The Ford and Carter administrations built upon the Nixon base by encouraging agencies to consider the impact of their regulatory initiatives on inflation and the economic competitiveness of the regulated industries. In 1981, the Reagan administration issued an executive order that expanded the power of the executive office of the president to review all major regulations proposed by executive branch agencies and required that agencies demonstrate that the benefits of their proposed actions exceed the costs imposed. The Office of Management and Budget (OMB) is responsible, under the Reagan order, for reviewing the economic analysis that is to accompany regulations and assuring that the proposals satisfy the cost-benefit standard,

unless agency statutory law prohibits such a consideration. The OMB, however, has no direct authority to rewrite or alter the regulations themselves.

The Reagan order is part of a broader effort of budgetary, personnel, and managerial decisions and actions that seek to redirect and shape administrative activity without rewriting the laws under which agencies function. The efforts have raised much controversy and have been challenged in federal courts, in congressional hearings and proposed legislation, and in interest group and media investigations. Presidential oversight of, and intervention in, regulatory rule-making proceedings have been as controversial as the substantive regulations themselves and have raised important questions of political accountability, the politicization of scientific research and analysis, administrative and managerial efficiency, openness and public participation, and due process and fairness in the regulatory process.

A number of published studies and law review and journal articles have already been published that have focused on the politics of presidential oversight of regulatory agencies. Susan J. Tolchin and Martin Tolchin's *Dismantling America: The Rush to Deregulate* (Boston: Houghton Mifflin, 1983) and Jonathan Lash, Katherine Gillman and David Sheridan's *A Season of Spoils* (New York: Pantheon, 1984), for example, are very critical of the Reagan approach, charging that it has seriously undermined important regulatory programs. Howard Ball's *Controlling Regulatory Sprawl* (Westport, Ct.: Greenwood Press, 1984) and George C. Eads and Michael Fix's *Relief or Reform? Reagan's Regulatory Dilemma* (Washington, D.C.: Urban Institute Press, 1984) find much to criticize in the Reagan effort, but are much more sympathetic to the idea of presidential oversight of administrative decision making.

The collection of essays that V. Kerry Smith has assembled in this volume on the Reagan executive order make a significant contribution to this growing body of literature. The collection includes nine essays, from a variety of perspectives including the OMB's regulatory review office to the Environmental Protection Agency's economic analysis division to academic scholars. The major themes pursued in the essays include the evolution of presidential

oversight of regulatory rule making, the theory of benefit-cost analysis, the quality of analysis that has taken place under the Reagan order, and the overall costs and benefits of imposing benefit-cost analysis in agency decision making through the executive order and OMB review. While the essays do not ignore the political issues that the executive order has spawned, they emphasize and describe with clarity the economic assumptions underlying benefit-cost analysis. Combined with other studies that give primary attention to political considerations such as the tension between congressional and presidential oversight and that between presidential review and the procedural requirements of administrative law, this volume adds much to our understanding of recent efforts of presidents to gain more control over the administrative process, and adds much to an initial and preliminary understanding of the prospects for the use of benefit-cost analysis in regulatory agencies and its effect on the quality of regulatory decision making.

GARY BRYNER

Brigham Young University

Paths of Neighborhood Change: Race and Crime in Urban America. By Richard P. Taub, D. Garth Taylor, and Jan D. Dunham. (Chicago: University of Chicago Press, 1984. Pp. xii + 264. \$25.00.)

Richard P. Taub, D. Garth Taylor, and Jan D. Dunham of the University of Chicago have written an impressive but ultimately troubling book. Their objective is quite ambitious: to modify and refine one the cornerstones of American urban sociology: the invasion/succession model of neighborhood change. As their study's title indicates, they purport to emphasize the connections between crime and neighborhood racial turnover. However, in *Paths of Neighborhood Change's* introductory chapter, which outlines the theoretical issues impinging on the authors' research agenda, the discussion of why they single out crime (as opposed to garbage or governmental unresponsiveness, for instance) as a contributor to neighborhood deterioration is at best sketchy. The remainder of the book sustains this split emphasis. For the most part the authors look

at how neighborhood residents assess their communities, make judgments about whether or not to invest in home upkeep, and react to the initiatives of local government and other "institutional actors." Every several pages a discussion of crime appears, but the authors never make the case as to why this particular issue overshadows other neighborhood problems nor fully integrate the crime and neighborhood investment analyses.

The strength of this volume is its research design and methodology. The authors examine eight Chicago neighborhoods, and through a combination of direct observation, survey data, and secondary material they manage insightfully to detail each community. Following three chapters of neighborhood case studies, multiple regression and formal modeling of the survey results are employed are employed to generate explanations of local investment, neighborhood racial tipping, and crime perception as a component of neighborhood change.

By themselves, the neighborhood case studies highlight the variable paths of neighborhood change. The mathematical analyses in chapters 6 through 8 also produce some interesting insights, most prominently the ways by which housing market conditions can affect the likelihood of neighborhood racial turnover. As is typically the case, the authors make some arbitrary judgments in manipulating and interpreting their data, and these judgments have bearing on their analysis. As both a dependent and independent variable, housing investment is assumed to bode well for a neighborhood, when in reality it is a more ambiguous activity. Homeowners rigging their houses with sophisticated burglar alarms and front door gates, or surrounding their yards with high fences, may not build the confidence of their neighbors. The authors are puzzled at what they interpret as the racial prejudice of blacks directed at other blacks—specifically their fear of neighborhoods becoming totally black—but it does not occur to them that black residents of a city like Chicago associate such segregated neighborhoods with redlining and reduced city services. What the authors view as racial prejudice might alternately be called reasonable apprehension. Finally, Taub, Taylor, and Dunham anchor their discussion of individual investment in theories of collective action, yet they do not connect this

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approach to the functions of community organizations, which succeed—on one level at least—by constraining the free-rider temptation.

The foregoing problems are real, but it should be repeated that they occur within the context of a generally rigorous and creative analytical operation. What troubles this reader most about *Paths of Neighborhood Change* are some of the conclusions the authors draw. On one hand are perfectly sensible correctives to some observers' faith that local organizations, by themselves, can save neighborhoods, or the analogous thinking that the problems of frostbelt cities can be solved through purely local initiatives. Yet the authors also support neighborhood triage, when there is absolutely no reason to suppose that the residents of written-off neighborhoods would receive the job counseling, psychological supports, and financial aid requisite for starting life anew in some other location. Similarly, the authors hold to a faith in the benevolence of institutional actors as agents of neighborhood stability. This flies in the face of the experience of about half their Chicago neighborhoods, in which institutional actors have sought to speed or impede neighborhood change for reasons often quite insensitive to the welfare of local residents. Oddly, in a study that goes so far in describing how distinctive are the courses of neighborhood evolution, the authors themselves fall prey to reifying the concept of neighborhood and forget that people with lives, jobs, expectations, and fears are the neighborhood. Policy should be informed by the needs of these people and not by the idea of neighborhood improvement.

LARRY BENNETT

DePaul University

Democrat Versus Democrat: The National Party's Campaign to Close the Wisconsin Primary. By Gary D. Wekkin. (Columbia: University of Missouri Press, 1984. Pp. x + 253. \$23.00.)

When Democrats circle the wagons, says the old joke, their rifles aim inward. In the wake of

wagons circled—for an internecine shootout that isn't over yet.

Democrat Versus Democrat describes one of the battles in this now eighteen-year war—that between the Democratic National Committee and Wisconsin Democrats over the latter's beloved open primary. Wekkin has captured it with a rigorous eye and a lively pen. The book begins with an important theoretical scorecard, telling the players' positions. Of all the questions raised in this first chapter—federal, factional, legal—perhaps none is more important than the democratic: "the question of whether democratic government means direct, popular participation in decisionmaking—or popular control over those who decide" (p. 12). The first, the "classical" position, "has been the credo of most Progressives and McGovern-era reformers and of the many Wisconsin Democrats who have opposed implementation of the DNC's closed-primary rule." The alternative cudgels are carried by "revisionist democrats such as E. E. Schattschneider and some advocates of 'responsible party government'" (p. 13).

Subsequent chapters illustrate mainly that, alas, players did not often choose their stands on such elevated grounds. For example, some reform Democrats at the national level supported closure largely because George Wallace had done well in Wisconsin's "Democratic" primary by dint of Republican and independent votes. Regulars also supported closure, thinking (wrongly) that crossover voting had robbed Hubert Humphrey of a 1972 victory in the crucial Wisconsin primary. Wisconsin party-organization members, who had aspirations within the national party organization, were more easily resigned to closing the primary than were Wisconsin politicians, who had to face Wisconsin voters. Jimmy Carter saved the state's open primary in 1980, when polls showed he would do well in it. But presidential candidates spurned the primary in 1984, when it fell too late to be vital to them. And so it went, in a dismal parade of calculations about whose ox would be gored, whose ax would be ground.

It is not a criticism of this book, but a tribute to it, to say that there is plenty of room for argument here. For example, in rendering the revisionists' position as one of the conflicting theories of democracy, Wekkin cogently shows that both makers of and participants in

the debacle at Chicago in 1968, Democrats shouted "reform!" and, sure enough, the primary elections have tended to be "an elite few" (p. 14). But he doesn't recall this argument in the final chapter, where he makes the revisionist position his own. There, he truncates the argument after saying that the people have trouble governing in the absence of responsible parties. Of course, *some* people do govern after *the* people default out of primaries, and it would seem logical to say again who they presently are, to add that they are demographically and attitudinally unrepresentative of the party constituency and its interests.

Wekkin does not do this, because, in the end, his animus toward presidential primaries is somewhat tepid. On one hand, he opposes the open primary because it "lessens party responsibility" (p. 182), and he "hopes for more such triumphs for the revisionist view" (p. 189). On the other hand, Wekkin is satisfied that the existing system of primaries and caucuses open to any "self-designated adherent" does assure a "greater degree of internal cohesion and responsibility" (p. 184). That is perhaps too sanguine a conclusion when, for example, over half the self-designated Democrats in the country don't know the difference between the Democratic and Republican positions on the watershed questions of government responsibility for insuring employment or an acceptable standard of living. One can argue that when the Democrats "opened up" the delegate selection process to an electorate unfamiliar with party ideology, they wrought a babel of Madison-Avenue slogans, not a responsible party. And the unhappy irony is that no group has been so disconcerted by this confusion of tongues as working and lower-class Americans—the bedrock of the Democratic party.

But again, this is not a criticism. It is just the kind of question one expects to arise from a good read—the kind provoked by the work of an active mind on an urgent issue.

CALVIN F. EXOO

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Aging and Public Policy: Social Control or Social Justice? By John B. Williamson, Judith Shindul, and Linda Evans. (Springfield, Ill.: Charles C. Thomas Publishing Co., 1985. Pp. 332. \$34.75.)

A consideration of this book can usefully begin by pointing out that essentially the same group of joint authors several years earlier produced a comparable book in the same general field. That work, *The Politics of Aging: Power and Policy* (1982), developed many of the themes that are to be found in the work presently under review, but was fundamentally different in key respects. In *The Politics of Aging* the unifying theme was *political influence*: heavy stress was placed on the hopeful prospects for "senior power" and the potential for "coalition formation" between this country's organized elderly and other constituencies of disadvantaged citizens. While the 1982 book was careful to point out that there could be no absolute assurance of growing political strength for the elderly over the next 50 years, the authors still could voice optimism that the elderly's political resources "will translate into increases in autonomy and political influence," and even could venture the possibility that "the increase [in influence] will be much more substantial than even the most optimistic of today's pro-aging advocates anticipate" (p. 264). While the book did contain some discussion of the less upbeat theme of the *social control* of the elderly, this was only a subsidiary motif.

In the book presently under review the authors not only shift their focus from the elderly's influence and power potential toward the politics of old age programs and policies, they also adopt the social control motif as their dominant theme. Their frame of reference is now firmly neo-Marxian, and their stance toward aging policy in America becomes consistently radical and critical. While many factors presumably contributed to this change in emphasis, one factor clearly was the marked deterioration in the material and psychic circumstances of large numbers of elderly Americans since the period of the 1970s. Thus, the note of pessimism that pervades the present work to some degree reflects the authors' sense of the elderly's recently-diminished prospects—a point that is strongly underscored in their analysis.

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Williamson, Shindul and Evans develop the thesis that the elderly have become a leading example of how "enlightened bureaucratic management" can replace the controls once wielded by the primary group and the coercive powers of the state as sources of social control. They go on to insist that such bureaucratic manipulation, combined with a no less coercive medical and psychiatric model, now largely defines the limits of old people's possibilities and potential. In arguing their case, the authors draw on the findings of other radically-inclined analysts who have earlier written on American policy in aging, especially Carrol Estes, William Graebner, John Myles and Laura Olson. However, whereas the other radical authors have tended to concentrate on one or another *facet* of public policy—for example, age-based social services in the case of Estes, or retirement policy in the case of Graebner—the present authors adopt a more systematic approach, with no major aspect of American aging policy left unexplored. Their approach is essentially encyclopedic, rather than idiographic, a fact reflected in the exceptionally long (50 page) bibliography at the end that is needed to identify properly the works cited in the text.

Among the virtues of the book is its success in combining a cross-disciplinary and multi-author approach with a consistency of tone and focus. Among the three authors listed on the title page, two (Williamson and Evans) are sociologists, and the third (Shindul) is a professor of nursing. Moreover, three additional individuals have each contributed a single chapter to this twelve-chapter book, of whom two are sociologists and one a political scientist. While such diversity of authorship and disciplines might conceivably make for an unevenness of approach, this risk has been avoided; a common thread, based on the "social control" model, links the entire work and gives it coherence.

The book is not without its weaknesses, and to a large degree these are a consequence of an

apparent excess of enthusiasm in developing the authors' main message. Readers who come to this source in hopes of finding a balanced treatment of programs and policies are likely to be disappointed. The tendency to see evidence of social control in every facet of aging policy in this country does not leave much room for any acknowledgement that these same policies in certain ways may actually increase the elderly's autonomy and range of meaningful choice. (This latter possibility is noted in an abstract way in the opening chapter, but the theme is not developed in the main body of the book.) Secondly, the authors' perspective may have caused them to assume something that is very much in need of proof, namely the asserted causal connection between social control and this country's prevailing capitalist system. The question of whether capitalism is the root cause of social control of the elderly would seem to be an empirical matter and not something to be assumed *a priori*. If the authors had adopted a cross-national, rather than a single-country, approach, the question of whether equal (or perhaps even greater) degrees of social control may exist in socialist and other non-capitalist settings might have been meaningfully raised.

These criticisms should not be taken as detracting from the book's essential strengths. For the most part, academic commentary on aging policies in this country has been at least implicitly celebratory, and in this context the recent "radical school" of program and policy analysis is serving as a useful and needed corrective. The present work deserves a prominent place within this school of analysis. While presenting little in the way of material that is in fact original, the book succeeds well in weaving together the many disparate sources that otherwise might remain inaccessible to most students of American public policy.

HENRY J. PRATT

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COMPARATIVE
POLITICS

The French Socialist Experiment. Edited by John S. Ambler. (Philadelphia: Institute for the Study of Human Issues, 1985. Pp. 224. \$27.50.)

Socialism, the State and Public Policy. Edited by Philip G. Cerny and Martin A. Schain. (New York: Methuen, Inc., 1985. Pp. 298. \$29.95, cloth; \$12.95, paper.)

By the time this review appears, the French electorate will have had the opportunity to give its verdict on the French socialist experiment to which both these books are addressed. It began in May-June 1981 with the promise to change life and the society in tones reminiscent of the ideological fervor of the French Resistance. In its first year and a half, the Socialist government undertook sweeping economic and social reforms. By the end of 1982, however, there was a pause followed by a retreat as the central goal of the socialists to revive and modernize the economy while providing, at the same time, full employment and sweeping welfare measures began to elude them. Why was this so? Were the original hopes simply beyond reach? Were the measures used—nationalization and massive government spending—inappropriate? Did the *bête-noire* of the French Left—the business and financial interest—intervene once more to thwart Socialism as they had done in 1935-37 and again in 1945-47? Was it a matter of bad timing? France, solidly implanted in the Common Market and the international economic order over which the U.S. presides, decided to go alone against the current of deflationary economics in West Germany, England, and the U.S.A. Were the socialist families within the Socialist Party unable to put their act together? French Socialism may have been the victim of its inherent "political pluralism" as two authors contend. Over and beyond these questions looms the larger question—and to some of the authors almost a transcendental one. If French socialism is in serious trouble, can socialism continue to be considered a viable alternative to economic liberalism? Can we

still rely on the socialist blueprint to provide equality and abundance and to liberate human initiative and endeavor at the same time?

The reader will find tentative answers or, at least tentative efforts to answer all these questions and many others raised by the editors and authors in these two books. Both are excellent efforts to study the relations between state and economy—a growing preoccupation of students of comparative politics—and to evaluate public policy in France. *The French Socialist Experiment*, edited by John S. Ambler is more directly focused on state-society relations. The editor has written three of the eight essays—the first on the history of the French Left, particularly when the power—1936, 1945-47, and after 1981; on the educational reforms undertaken after 1981, and a concluding chapter on French socialism in comparative perspective—mostly in comparison with Labor Socialism in England. There are essays on Social Security under Mitterrand by Gary Freeman; on agricultural reform by John T. S. Keeler; on Immigrants and Politics by Martin Schain, on economic policy by George Ross and Jane Jensen and on decentralization by Douglas Ashford, which unfortunately is limited to the study of the law introduced by the government and as it was being debated by the legislative assemblies. *Socialism, the State and Public Policy in France*, edited by Cerny and Shain, covers a great deal more but what it gains in coverage is offset by the variety of topics chosen. It is more of a symposium, in which the various authors ran away from whatever plan the editors had in mind. There are, however, some excellent essays: Peter Hall's "Socialism in One Country: Mitterrand and the Struggle to Define a New Economic Policy;" Pierre Birnbaum's "The Socialist Elite 'Les Gros' and the State;" Kesselman's "The French Communist Party;" as well as part three, "Structural Constraints and Policy Arenas" with articles by Philip Cerny, Sally Sokoloff, Martin Schain, and John T.S. Keeler (the last two contributed also to Ambler's volume).

Virtually all essays in both these books—21

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in all—live up to high scholarly standards; the books represent the best and most comprehensive analyses and evaluations of the French Socialist experiment that have appeared to date. Both must be read, and in a sense the haste of the editors to see their books published before the French legislative election of March-April 1986 is both understandable and welcome. The two together give us the most comprehensive account of various facets of the French economy and society, and they both are required reading for understanding the forthcoming elections.

Is there a general conclusion? Even though French socialism is in both books a little bit like the proverbial elephant—each author describes and analyzes what he or she has touched—the overall conclusion is inescapable. It is moribund according to some for lack of humility of the Socialist leaders who promised to change society and life; according to others because of intractable international economic forces; for some because of the internal squabbles of the French “left,” though the term left remains ambiguous—sometimes the authors include the Communists and sometimes they do not. But when all is said, the salient point remains that the French left in general and the Socialist party in particular lacked a strong political base. Though George Ross and Jane Jensen are right in stating that rarely, if ever (in my opinion, the term “never” would have been more appropriate), has there been a Left government endowed with more institutional power than the Mitterrand one. Yet its political base remained shaky. A mere shift of 2–4% accounted for the Presidential and legislative victories of the Socialists in May-June 1981, with full Communist electoral support. After 1982, even this slim electoral advantage began to evaporate, and since the summer of 1984 the Communists have been sulking again in their self-imposed ghetto.

Whatever the economic and other constraints that undermined the socialist experiment, the political base was too thin from the start. And this may well be the major flaw of both books—they neglect the political base of French socialism and of the French socialist, legislative and presidential “majorities.” In their effort to adumbrate the societal and economic forces at work, to dissect the various group pressures and counter-pressures, they ignore the force that binds them together or

causes them to unravel—politics. There is not a single article on public opinion and opinion trends under the Mitterrand government and on the attitude of the opposition parties, including the recrudescence of the extreme right.

ROY C. MACRIDIS

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The Emergence of a New Lebanon: Fantasy or Reality. Edited by Edward E. Azar. (New York: Praeger Publishers, 1984. Pp. x + 292. \$32.95.)

Much ink has been spilled on the Lebanese tragedy and the deadly civil war which has turned Lebanon into a symbol of destruction and disintegration. What distinguishes this book, edited by Edward Azar, a specialist on Lebanese affairs and Director of the Center for International Development at the University of Maryland, from a number of others is the depth of knowledge displayed by its contributors and its focus on the socioeconomic background and historical context of the issues involved.

The authors address several critical questions. Who are the main actors on the Lebanese scene and what are their strengths and weaknesses? Where do foreign powers, mainly the United States, Israel, Syria, and the PLO, stand on the conflict? What difficulties face internal reforms? And, ultimately, what are the chances for the emergence of a new Lebanon?

Since it is difficult to do justice to all contributors in a short review, it might be better to highlight some of their significant ideas and conclusions. Paul Jureidini and R. D. McLaurin note the shift in the political and social balance of forces in the country in the years after the 1975 civil war. Lebanon, they proclaim, has “experienced a social and political revolution that is little understood either domestically or outside” (p. 6). This interpretation is worth underlining, since this “Lebanese revolution” is still in the making and is constantly surprising observers by its ever-changing phases.

Augustus Richard Norton studies a major new factor on the Lebanese scene, the Amal movement, the most conspicuous manifesta-

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tion of the rise of the shi'ah as a power, noting that "whatever the future will hold for this ill fated country, it is indisputable that the Lebanese Republic's plurality, the shi'a, will play a decisive role in shaping it for better or worse" (p. 162). He outlines, among other things, the demographic increase of the shi'a to the status of a plurality; the migration of the shi'a from the south to the north; and their poverty, especially in southern Lebanon.

If the shi'i Amal movement is the main Muslim organization in Lebanon, the primarily Maronite Lebanese Forces is the chief Christian paramilitary organization. A thorough study of this organization is undertaken by Lewis Snider who gives us an account of its rise and growth to the point where it exercises quasi-governmental power in the Christian territory under its control. Its objective is "to liberate Lebanon from Syrian, Palestinian and Israeli occupation" (p. 117).

R. D. McLaurin gives us an original study (perhaps the first of its kind in English) of the Lebanese Army, detailing its historical development; the traditional sectarian basis of its recruitment; its composition—mainly Christian officers and shi'i ranks; and its ups and downs with the Palestinians. The army, according to the author, though it is small and weak, remains solidly respected by the Lebanese population—a fact of significance for the future of Lebanon.

The critical issue of the effect of the war on the Lebanese economy is tackled by Joyce Starr who gives us some interesting but pessimistic reflections on the subject. After discussing the destruction caused by the war on the structure of the Lebanese economy, she comes to the sad conclusion that "the continuing isolation of different parts of the country—in effect, de facto partition—further compounds the stagnation which threatens to become a permanent, rather than temporary, feature of the economy" (p. 77).

If subsequent events on the Lebanese and regional scene have contradicted some of the conclusions in the book, many of the main judgments and observations remain valid. This book deserves to be read by general readers and policy makers alike who are interested in the fate and future of this critical country.

LOUAY BAHRY

University of Tennessee

Interest Groups and Political Development in Turkey. By Robert Bianchi. (Princeton, N.J.: Princeton University Press, 1984. Pp. xiii + 426. \$45.00, cloth; \$17.50, paper.)

Turkey: The Politics of Authority, Democracy, and Development. By Frank Tachau. (New York: Praeger Publishers, 1984. Pp. xiii + 228. \$27.95.)

Bianchi's book describes the emergence of competing means for structuring interest representation as well as tensions in Turkish politics produced by historical shifts in the relative importance of different types of groups. He examines political culture and public policy to account for variance in their influence and strategies. This includes comparative analysis of interest group activity and leadership in different economic sectors.

Essential to Bianchi's analysis is his use of Philippe Schmitter's concepts of corporatist and pluralist group structures later refined to include state corporatism and societal corporatism, with the latter characterized by groups with some autonomy while the former are dependent upon and penetrated by the state. Much of Bianchi's study is devoted to the emergence of a highly unusual and unstable mix of both state and societal corporatist groups produced by state initiatives and political party strategies amidst uneven but rapid economic development. He investigates efforts to reconcile control of tensions and demands through corporatist political strategies with maintenance of a democratic political system.

Bianchi notes that public policy in regard to interest groups has been eclectic and has in various ways—despite a political culture supportive of corporatism—contributed to representational dualism with opportunities for disgruntled group leaders to turn to alternative voluntary associations. With the transition to multi-party competition in the 1950s, large numbers of feeble, competing, and easily manipulated groups appeared. Governments encouraged this in a policy of "debilitating pluralism." From 1960 to 1980 a series of weak governments pursued societal corporatism by extending privileges to certain groups and by fostering dependence on the government while simultaneously imposing restrictions on uncooperative groups. Of particular interest in

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this later period are the respective pluralist and corporatist strategies of the People's Party and the Justice Party. This dualistic interest representation intensified demands for participation and distribution by shifting the demands of those who opposed results in the corporatist arena into an expanded pluralist arena which became the preserve of the political opposition. This important observation provides a significant structural explanation of the intensity and violence of Turkish politics before 1980 and offers a basis for comparison with other political systems—a comparison which Bianchi pursues at the end of his work. The Chilean case, for example, involving competition between antagonistic pluralist and corporate associational channels allied with different political forces bears considerable resemblance to the Turkish experiences with representational dualism and societal corporatism. This is comparative analysis at its best and is an important contribution to both theoretical and practical study of comparative politics. Similarly, Bianchi's comparative analysis of Turkish interest group behavior imposes analytical order on an otherwise confusing array of groups.

Bianchi attributes a degree of planning based on perceived objective interests to ruling elites which ignores the "irrational" dimensions of political behavior. More importantly, despite a disclaimer concerning the "ecological fallacy" and problems in using aggregate data, it appears he finds other kinds of data unacceptable. Not only does he dismiss local community organizations as "politically insignificant" (p. 155), he attributes the "retarded state" of the study of the social bases of Turkish political parties in large measure to the restricted opportunities to conduct public opinion surveys (p. 154, n. 14). Similarly, he pays little attention to the literature on groups in local politics. This literature would have helped balance Bianchi's skewed "macro" view and provided a clearer picture of associational dynamics. For Bianchi the bottom line is aggregate data and survey research even at the community level (pp. 150-51). Political science includes a diversity of perspectives, and Bianchi's view seems dogmatic.

The author's relatively uncritical use of the newspaper *Cumhuriyet* as a source of information is disturbing. For more than a decade, *Cumhuriyet* has been a spokesman for the

radical left and has held a conspiratorial view of Turkish politics. Another, more positive, observation is that this work is of the same genre as *The Turkish Political Elite* by Frederick Frey. It relies heavily on statistical analysis, and is likely to be as important to interest group analysis in Turkey and elsewhere as Frey's work is to elite analysis.

Like Bianchi, Tachau is concerned with political change in Turkey. He studies this and modernization in terms of the concepts of authority, democracy, and development. Although the work is not intended to be comprehensive, it provides a wealth of information, a skillful synthesis of much of the best literature on Turkish politics, and an insightful analysis of important aspects of the Turkish political experience including constitutional changes, political participation, economic issues, and foreign policy. Tachau's intimate knowledge of Turkey is apparent throughout, and although he remains alert to general trends and issues, he studiously describes and provides examples of important details of Turkish political life. He is at his best in bringing together and analyzing the case study literature on rural Turkey—literature to which Bianchi gives little attention. His description of the critical position of provincial towns and the rich and varied dynamics of their group politics provides the reader of both of these books with a useful reminder of the perils of aggregate analysis and the temptation to reify categories amenable to statistical analysis. His instructive study of economic issues addresses income distribution, standard of living, and social welfare and their relationship to Turkish political culture and particular political events.

In Tachau's treatment of foreign policy, little attention is paid to the importance of Turkish "guest workers" in Western Europe. Turkey's improving relations with the Muslim world are discussed but the interesting related issue of Turkish-Israeli relations is largely ignored. His emphasis on the external determinants of Turkish foreign policy leads him to overlook the foreign-policy implications of the ideological struggles caused by the social and economic transformation he describes so well. Anti-American sentiment, for example, may have been exacerbated by particular American policies, but it was hardly created by these policies. This said, Tachau's excellent description of the Turkish role in the Cuban missile

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crisis must be noted. His discussion of Turkish perceptions is a perspective rarely found in studies of that crisis. Students of comparative politics will find this work a thoughtful overview of Turkish political development and specialists on Turkey have been given a stimulating synthesis and interpretation of much of the literature on Turkish politics.

ARNOLD LEDER

Southwest Texas State University

The Consensual Democracies? The Government and Politics of the Scandinavian States. By Neil Elder, Alastair H. Thomas, and David Arter. (Oxford: Martin Robertson, 1982. Pp. xii + 244. \$49.95, cloth; \$11.95, paper.)

The authors' expressed purpose in *The Consensual Democracies?* is to provide students with a contemporary study of Nordic politics in English. It replaces the woefully dated five-country comparisons of Nils Andren and John H. Wuorinen written in the 1960s (Nils Andren, *Government and Politics in the Nordic Countries: Denmark, Finland, Iceland, Norway, and Sweden* [Stockholm: Almqvist & Wiksell, 1964]; and John H. Wuorinen, *Scandinavia* [Englewood Cliffs, N.J.: Prentice-Hall, 1965]). It makes more useful for instruction collections of essays such as Erik Allardt, editor, *Nordic Democracy* (Copenhagen: Det Danske Selskab, 1981) which is a smorgasbord of topics of varying difficulty and which lacks an organized perspective for studying the region.

A brief political history of the Nordic countries is presented in the introduction. Chapter 1 raises the question of the origins and viability of Scandinavian consensual politics. A three-dimensional definition of consensus is offered: (1) the degree of opposition to the framework of rules and regulations for conflict resolution; (2) the level of conflict or the intensity of societal cleavages and the exercise of political power; and (3) "the degree of concertation in the gestation of public policy" (p. 11). Chapters 2 and 3 outline the structuring of mass politics in the region and the patterns of party conflict from 1870 to the late 1970s in the tradition of Stein Rokkan. A "nuts-and-bolts" description of political institutions is

found in chapter 4. Chapter 5 addresses parts 2 and 3 of the consensus definition for Denmark, Finland, and Iceland as separate cases and comparatively for Norway and Sweden. Chapter 6 adds an international dimension by outlining consensus and cooperation in international security matters and regional cooperation through the Nordic Council. And the epilogue ponders political trends and prospects.

The authors do a commendable job in juxtaposing the structure and development of five, "five-party" systems in chapters 2 and 3. There is a nice mix of historical narrative, tables, and a sensitivity to dominant paradigms of Scandinavian political development. There is detailed description of the political conflicts of the 1970s and their resolution (chap. 4). Noteworthy is the thorough discussion of Iceland which distinguishes this book from most Scandinavian comparative studies. Students interested in pursuing further work on the area will find the primarily English bibliography helpful, particularly with respect to Sweden. More Norwegian citations would have been welcome.

The description of the institutional framework and international politics is less well balanced because more information could have been added. The conflict management role of the courts and a limited power of judicial review, especially in Denmark, Iceland, and Norway, is omitted. The reader obtains substantial information on the structure of institutions, electoral rules, timing of institutional innovation, etc., but little on the political culture within the institutions. Missing is the nuance found in Johan P. Olsen's articles compiled in *Organized Democracy* (Oslo: Universitetsforlaget, 1983) on Norwegian elite behavior and their management of political conflict. No mention is made of the restructuring of local government and the experiments to promote consensus by decentralizing decision making and strengthening grass-roots democracy. The resurgence of conservative political forces is underestimated.

Trying to compare five countries is a difficult task, especially in a limited space and on a multitude of subtle dimensions. Thus, one should not be overly critical of the omissions. The book is primarily a description of a set of political events and structures in Scandinavia coherently presented around the question of

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political consensus. The book is well written. And it fills a significant gap in instructional materials on Western Europe and orients scholars to a rich body of data and studies on the Nordic democracies.

ROBERT B. KVAVIK

University of Minnesota

Islam and Politics. By John L. Esposito.
(Syracuse, N.Y.: Syracuse University Press,
1984. Pp. xviii + 273. \$12.95.)

This study of the role of Islam in politics should prove useful to the layperson or the professor seeking one short book that recounts and explains the major religio-political events that rocked the Islamic world in the late 1970s and early 1980s—for example, the rise and evolution of the Islamic Republic of Iran, the seizure of the Grand Mosque in Mecca, and the assassination of President Sadat.

Esposito limits the geographic scope of his study to the Middle East (from Morocco to Pakistan, excluding Afghanistan), and presents it in the form of a chronological narrative. Chapter 1-3 provide the historical background or context of contemporary Islamic politics. They deal successively with the relationship of Islam to politics in early Islamic history, the nature and legacy of the various movements of Islamic revival and reform in the eighteenth and nineteenth centuries, and the role of Islam in the anti-colonial independence movements and nationalist ideologies of the twentieth century. Chapters 4 and 5, comprising half the text, are more current. Chapter 4 discusses the diverse state- and nation-building paths followed by newly independent Muslim countries in the quarter century after World War II. Using case studies of Turkey, Saudi Arabia, Pakistan, Egypt, and Iran, it analyzes the nature and influence of two major Islamic movements, the Muslim Brotherhood and the Jamaat-i-Islami. Chapter 5 explores the sources and expressions of the Islamic resurgence in the 1970s and 1980s through an examination of the use of Islam by both incumbent governments and opposition movements in Libya, Pakistan, Iran, and Egypt. The study concludes with a short chapter that analyzes the problems and

prospects of contemporary attempts to establish modern Islamic states and societies.

Esposito is a knowledgeable and sympathetic student of Islamic politics. He is sensitive to the staggering socioeconomic problems that have helped to trigger the Islamic resurgence—problems shared by most less-developed, modernizing countries—and he takes seriously and is respectful of the efforts of Muslim thinkers and activists to find culturally appropriate solutions to these problems. These matters are presented from the perspective of the developing countries involved, their histories, and the history of Islam—not from the perspective of international politics and the concerns of American foreign policy. The presentation is straightforward and quite free of jargon.

This book is a survey, however, and not a scholarly monograph. It contains little by way of conceptual rigor or theoretical insight to interest the specialist. Well-known facts and unexceptional explanations are frequently repeated, sometimes word for word and even on successive pages, rather than clarified or elaborated. Moreover, the text contains confusing conceptual and factual slips. The repeatedly made distinction between "Islamic" and "Muslim" states is never really explained. Is there a useful distinction to be made? If so, what is it? On one page the reader is told that in 1949 there were 500,000 members in the Muslim Brotherhood and on the next page it is put at 2,000,000. Which is it?

Finally, this book cries out for editing. Confusions like those cited above should have been caught and sorted out. Careful editing would have eliminated the unnecessary repetitiveness, a luxury that short books on big topics cannot afford. Most annoying, many sentences make no sense apparently because words were deleted or substituted and punctuation marks were omitted or misplaced as this work went from the typed to the printed page. The study is disappointing in part because of Esposito's qualifications. Chairperson of the Department of Religious Studies, College of the Holy Cross, he has in recent years authored or edited at least four other volumes on various aspects of Islam and the modern world.

ROBERT BURROWES

New York University

Political Elite Recruitment in the Soviet Union. By Bohdan Harasymiw. (New York: St. Martin's Press, 1984. Pp. xxii + 277. \$27.50.)

Elite Studies and Communist Politics: Essays in Memory of Carl Beck. Edited by Ronald H. Linden and Bert A. Rockman. (Pittsburgh: University of Pittsburgh Center for International Studies, 1984. Pp. xii + 352. \$34.95.)

As Sovietologists ponder the strategic moves of Mikhail Gorbachev, it becomes increasingly apparent that his success will rest upon his ability to confront Brezhnev's legacy of elite immobilism. To grasp fully the significance of this challenge and to translate it meaningfully for the western public and policy makers alike, Sovietologists would be well advised to examine afresh their assumptions and models of political elite recruitment in Communist systems. An excellent place to start is Linden and Rockman's *Festschrift* to Carl Beck, and Bohdan Harasymiw's thorough empirical analysis of political elite recruitment in the USSR.

The readings in Linden and Rockman's volume will go a long way toward advancing research in this area down the path of rich theory building and useful empirical analysis—two objectives which Carl Beck would have surely found an appropriate dedication to his scholarly efforts. The eleven papers in this volume are remarkably consistent in their conceptual breadth and clarity of presentation. While each chapter is enlightening, arguably the most seminal chapters in the volume are those of Valerie Bunce ("Of Power Policy and Paradigms: The Logic of Elite Studies"), and William A. Welch ("Political Elites and Public Policy"). Both chapters capture the general points developed throughout the volume: The need to view the environmental context within which political elites function, and the need to conceptualize the interaction between the environment and the structural constraints confronting the political elite. Bunce turns our attention specifically to the dynamic of the political economy, while Welch explicates the utility of combining different levels of analysis in our study of the linkage between communist leadership preferences and policy outputs.

Despite the volume's overall richness, one

grows increasingly uneasy moving through these papers. It becomes apparent that on the surface the frameworks for analysis promise much for future research, but not far beneath the surface it is equally apparent that the actual prospects remain limited. Indeed, it is interesting to note that the only empirical treatment in the volume is directed toward Yugoslavia.

If one comes away from Linden and Rockman with some doubts as to just how far we can go toward putting our designs to work for us, then turning immediately to Harasymiw's study provides the reassurance that our conceptualization can lead to fruitful empirical verification. Harasymiw's principal task is to address the question: How can the Soviet Union remain so apparently stable while confronted by the turbulence in Poland and the inevitable pressures which bureaucratic modernization and industrial development create at home? To answer this question, Harasymiw examines the means of adaptation practiced by the Communist Party of the Soviet Union (CPSU). Specifically, he leads the reader through careful discussions of the cadre selection process, the changing role of the CPSU in a modern bureaucratic state, and the party's adaptation relative to the rate of societal change coloring the Soviet Union.

The volume provides a rich collection of social scientific research with variables carefully operationalized and hypotheses and alternative hypotheses logically presented and considered. However, the most basic shortcoming of the volume is his concluding section. After a thorough exploration of the political recruitment process from Khrushchev to Brezhnev, the reader is left hanging when the author offers very little speculation about the challenges confronting the post-Brezhnev political elite. His data confirm that while the Brezhnev era was a period of party "proletarianization," the process was not only imbalanced, favoring certain worker occupation groups (i.e., skilled workers and white-collar employees) but reflective of a gender preference as well. Furthermore, during Brezhnev's long tenure, the recruitment into the party activist positions took on an affirmative action character. This has meant that the technical intelligentsia have been largely bypassed relative to workers and peasants. Further restricting the technical intelligentsia's recruitment into active leadership

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positions has been the slowing of the cooptation process which allows non-activists to bypass normal career paths. Combined with a decline in the growth of party recruitment generally, the Brezhnev years have left Gorbachev with a legacy of "proletarians" occupying disproportionately political activist roles in the CPSU, while the intelligentsia remain largely closed out and presumably anxious for their chance at leadership. Here the reader is left wondering what it all means. This is unfortunate, for Harasymiw's data and presentation go much further than most studies toward explaining what exactly Brezhnev's legacy of the "stability of cadres" process is and how it works, and why it was built. It would have substantially strengthened the volume if the author had offered a few more thoughts on what the institutionalized elite recruitment process means for Gorbachev and his generation.

Taken together, these two volumes provide the discipline with some of the most thoughtful and illuminating material on communist political elite recruitment presently available in the literature. Both should be of much value to Sovietologists and comparativists engaged in sorting out the policies of Gorbachev and the post-Brezhnev Soviet political elite. Perhaps more importantly, both volumes are a tribute to the maturation and sophistication of Soviet studies, helping to dispel the subdiscipline's reputation of nescience.

JOHN D. ROBERTSON

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Iraq: Eastern Flank of the Arab World. By Christine Moss Helms. (Washington, D.C.: Brookings Institution, 1984. Pp. 215. \$28.95, cloth; \$9.95, paper.)

Since September 1980, Iraq has been engaged in a dangerous and costly war with neighboring Iran, the outcome of which may affect the energy economy of the West and the prospects for political stability in the Persian Gulf. It is through the Persian Gulf that about two-thirds of the world's exportable oil flows via the principal sealanes. Iraq holds oil reserves second only to those of Saudi Arabia

in the Middle East. The concentration of oil resources in the Persian Gulf, and the turmoil that affects the Middle East have created a major policy problem for energy-consuming industrialized nations.

In light of the importance of the Persian Gulf region, and of Iraq in particular, Helms examines the fundamental determinants of Iraqi policy—influences steeped in history and the geographic configuration of the country. She focuses on the forces that influence Iraqi policy formulation and evokes the perspectives from which the Iraqi government views its problems and priorities.

The book is in three parts and seven chapters. The parts are on domestic politics, ideological considerations, and conflict with neighbor. The seven chapters describe and assess the key elements in Iraqi policy. They deal with land and people; the emergent state; the years of growth; party organization; the Iran-Iraq war; a prognosis on the war; and the future of Iraq.

In her introduction Ms. Helms regrets that "it is ironic that Iraq, a key player in the drama unfolding in the Gulf, receded from the attention of senior officials in Washington after the overthrow of the last openly pro-Western government in Baghdad in 1958" (p. 1). She is right that because of lack of understanding and knowledge of the Middle East region the American government invites more trouble for its policy in the area.

Part three is the central aspect of the study which analyzes the Iraq-Iran war and offers a prognosis. It is neither a chronological account nor a military assessment of the war, but rather a description of the sources of conflict and of the decisions the Iraqi government made in light of its presumed objectives. Helms undertook a formidable task to examine the causes and foreseeable consequences of the war. Although she was able to visit Iraq twice to interview some of the officials, her "knowledge of Iraqi decisionmaking in the war has been sharply limited by the secretiveness of the Iraqi military" (p. 135). Yet she compiled much of the latest information on the war and corroborated the fact that unless the war ends in disaster for Iraq, that country will remain a vital force in the Middle East. Her treatment of the Iraqi position on the war is adequate.

The concluding chapter points out the size of Iraq's oil reserves and the potential for diversi-

fyng its oil-based economy. The war has sparked renewed concern about the possibility of closing the Strait of Hormuz by Iraq's enemy—Iran. Helms outlines Iraq's policy regarding the war quite well, but she fails to provide enough information in her observations about the possibility of closing the strait and its consequences on the world economy.

This slight criticism of the otherwise well-researched and documented study made possible by grants from the Rockefeller Foundation and the U.S. Department of Energy should not detract from the contribution Helms intended to make in the ever-growing literature on Middle Eastern affairs.

Helms, a research associate at the Brookings Institution, uses a simplified form of transliteration of Arabic words, chosen for the general readers rather than specialists. This makes the study of the volume much easier for the Western readers. All in all, this book is an extremely useful one for those interested in a study of Iraq.

SHEIKH R. ALI

North Carolina Central University

Theatre and Cultural Struggle in South Africa.

By Robert Mshengu Kavanagh. (London: Zed Books Ltd., 1985. Pp. xv + 330. \$26.25, cloth; \$10.25, paper.)

Constitutional Reform and Apartheid: Legitimacy, Consociationalism and Control in South Africa. By Laurence J. Boule. (New York: St. Martin's Press, 1984. Pp. xii + 270. \$27.50.)

Book length studies of South African playwrights and their works are rare, so one welcomes any good study of the South African theatre scene, regardless of its obvious biases. Kavanagh was born and raised in South Africa, but it was at Oxford that his politics moved Left, as far as Mao in the Third World, he says. And he became convinced "that theater should be used in South Africa as a means of mass organization, mobilization and conscientization" (p. x). He returned home to teach at the University of Witwatersrand, and

became very active in the local avant-garde (multi-racial) theater scene in the 1970s. It is with this background that he shares his insights on off-off-Broadway theater there and the people engaged in it.

Kavanagh states in the introduction that he follows the ideas of Marx and Engels that art must "be evaluated from the position of the revolutionary class," and of Lenin: "... for the socialist proletariat, literature can not be a means of enriching individuals or groups; it can not, in fact, be an individual undertaking, independent of the common cause of the proletariat. . . . Literature must become *part* of the common cause of the proletariat" (p. xiii). From this perspective he undertakes an analysis of theater in the Johannesburg area up to 1976. (Many interested in South African playwrights, especially the internationally successful Athol Fugard, will wish the study had been made more current, as too many excellent works are thus eliminated.)

Kavanagh focuses his analysis on four plays: *No Good Friday* by Fugard; *King Kong*, which was produced by the Union of South African Artists; *Too Late* by Gibson Kente, and *Shanti* by Mthuli Shezi. In each instance he relates the history and class origins before synopsising it, and analyzes it from the angles of "culture and ideology," politics, aesthetics, and language. It is in this analysis that the author is at his best, quoting liberally from the plays to provide excellent contemporary literary analysis.

On the other hand, Kavanagh clearly feels that a significant progression is made in expressing class consciousness from the beginnings in Fugard's multi-racial production (based on liberal, middle class values) to Shezi's *Shanti*, which is an attempt at a specifically political use of the theater by a playwright so devoted to the Black Consciousness movement that he becomes vice-president of the Black People's Convention. The two plays in the middle show an evolution toward the attempt of blacks to establish hegemony over their own culture rather than accept being coopted into the liberal, middle-class white theater. Despite occasions when the author lets his class-based analysis get in the way of reasonable interpretation, the book contains a great deal of compelling analysis of both South African theater and the society from which it springs.

Boule's book is described by its author as "a

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monograph on South Africa's constitution in the light of consociational theory (p. vi). Indeed, it will appeal to some students of the southern African situation because the second half of the book is a constitutional/legal analysis of the 1977 and 1983 constitutions. The latter is contained in its entirety in the appendix. Boule is an associate Professor of Law in the University of Natal. His study is clearly legalistic and has limited political realism. For example in his examination of the positions of political groups active in South Africa on the constitution issue, he fails to include the ANC or the UDF.

Boule attempts to weigh the merits of consociationalism as a solution to the constitutional problems of South Africa. In doing so he draws first on a long list of political theorists right out of every Intro. to Political Science syllabus. His analysis of liberal democratic majoritarianism concludes that its basis is electoral competition in which a minority can always hope to gain a majority vote. But in a plural society where a group such as South Africa's white population is out-numbered by six to one, majority rule, he claims, would doom the whites to a position in a permanent minority. Thus, adversarial democracy as we know it can never work in South Africa. (Of course this analysis assumes that political division will be invariably along racial lines, an oversimplification to say the least, and a position that does not correspond to the political history of South Africa or with current realities.)

Thus, advocates of consociationalism challenge the identification of democracy solely with majoritarianism, preferring to substitute their own position. Using the consociational model of Dutch theorist, Arend Lijphart, Boule explains that this approach has four elements: (1) a grand coalition of prudent, pragmatic elites representing all major groups, (2) mutual veto, (3) proportionality in governing institutions, and (4) segmental autonomy. While even Boule recognizes the historical failures of some of these principles, he finds it to be the only feasible alternative that could be acceptable to all sides. He then backs away from that position, however, and ultimately seems skeptical that it could work in South Africa. As a beginning, he is not even sure that one could put together a core of leaders for a grand coalition who could carry their consti-

tuents with them. In any case, he notes that the 1983 constitution appears to have moved away from consociationalism rather than toward it.

JOHN J. GROTPETER

St. Louis College of Pharmacy

Choosing an Electoral System: Issues and Alternatives. Edited by Arend Lijphart and Bernard Grofman. (New York: Praeger Publishers, 1984. Pp. xii + 273. \$32.95.)

In 23 short chapters, separately written by 22 authors (including the editors), Lijphart and Grofman seek to respond to a renewed interest in electoral reform. They regard their volume as unique in that it brings together "all of the basic issues and all of the key alternatives for electoral choice, presented by scholars with widely varying electoral system preferences" (p. 3). In this respect, the book is distinguishable from *Democracy and Elections: Electoral Systems and Their Political Consequences*, edited by Vernon Bogdanor and David Butler (Cambridge: Cambridge University Press, 1983), which avoided advocacy and presented excellent specialized analyses of particular national experiences around a principal theme stressing the determining role of political circumstances. True, some overlap occurs. A few chapters in the Lijphart-Grofman volume also usefully review national experiences, and two authors, David Butler and Richard Rose, contribute here as well as to the earlier work.

Choosing an Electoral System is striking because of its variety of viewpoints. It includes chapters by long-time critics of proportional representation (PR), notably Ferdinand A. Hermens; by established champions of PR, like Enid Lakeman and George H. Hallett, Jr.; by proponents of systems differing from both PR and single-member, simple-plurality election, exemplified by Steven J. Brams and Peter C. Fishburn; and by scholars who treat the electoral system as relatively unimportant, as do Richard Rose and William Riker. Almost every author begins with a reconceptualization of electoral systems and their roles in democratic government. Repetitious as this becomes after a few chapters, it is no doubt necessary for the development of each argument. At any rate,

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the chapters are so brief that a reader's interest is unlikely to disappear even if one shares the view that electoral systems are not major causal factors in the success or failure of democratic regimes. Such a reader will, incidentally, be happily confirmed in that view by Rose and Riker. In his customarily trenchant style, Riker puts the point starkly: "Constitution writers might as well adopt the electoral method that is most comfortable for the people who will use the constitution they produce, plurality or PR as the case may be" (p. 110).

From the record, the "most comfortable" method for most nations appears to be the one adopted about a half century ago. As Dieter Nohlen's analysis (pp. 217-24) shows, basic changes have not often taken place in the last fifty years. France was an exception even when the book was written, for the Fifth Republic's single-member, double-ballot Assembly election represented a return from the Fourth Republic's PR to the method customary in the Third Republic. Now that the Socialist majority seeks to cut its future electoral losses by moving again toward PR, the French exception is still more conspicuous. But for the very reason that France has so often changed methods, its return to PR would strike most of us as less portentous than the adoption of PR by Britain. Yet it is that bastion of single-member, simple-plurality election where change has lately been most widely and seriously discussed. Furthermore, so detached a scholar as David Butler believes it unlikely that "first-past-the-post voting for the Westminster Parliament will last to the end of the century" (p. 229).

Among the numerous advocacies in this volume, one of the most curious is the suggestion by both Maurice Duverger (pp. 36-37) and R.J. Johnston (p. 69) that the U.S. House of Representatives be elected by PR. Presumably, third and fourth parties would then be much more likely to elect members, but it is not clear that the result would be greater diversity in representation of opinion or interests than is now achieved through our loose and porous Republican and Democratic parties. It may be granted that multipartism in an American legislative body does not, as in a parliamentary system, make for a multiparty executive coalition; hence, one cannot argue that the decreased chance of a legislative majority party, under PR, has possibly baneful effects on execu-

utive stability. Nonetheless, the absence of a majority party in the House of Representatives could make it significantly more difficult to conduct legislative business in a constitutional system where the Congress is expected to exercise great authority of its own. The looseness of each of our major congressional parties, while accommodating diverse policy preferences, does not preclude effectiveness in organizing each house and determining its procedures.

LEON D. EPSTEIN

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Studies of the Structure of National Elite Groups. Edited by Gwen Moore. Research in Politics and Society: A Research Annual. Volume 1. (Greenwich, Conn. and London: JAI Press, 1985. Pp. x + 283. \$23.75.)

The eight papers in this volume are for the most part based on elite studies done in the early 1970s and already reported on by their authors in various other publications. All but one focus on a particular country and offer a descriptive analysis of what is referred to as the "structural integration" of its national elites—broadly conceived as social homogeneity and/or political agreement among the incumbents of leading positions in state and society. While the theoretical perspectives and methodological approaches employed by the different authors vary a good deal, social homogeneity is generally deduced from demographic data and elite consensus from opinion surveys.

The country chapters on elites in the United States (Barton), West Germany (Hoffmann-Lange and others), Australia (Highley and Deacon), Franco Spain (Baena del Alcazar and Pizarro), Israel (Lisak), and Colombia (Ocampo-Zamorano) will mostly interest area specialists who are not familiar with the original studies. Unfortunately they bear little or no relationship to each other, though the editor and most of the contributors collaborated for many years in the International Opinion Makers Project. Here one wishes for a more precise common analytical framework, for less conceptual confusion, and, above all, for some sort of comprehensive introductory or concluding chapter by the editor.

In the only systematic cross-national con-

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tribution to the volume, Field and Highley claim to put forward a novel elite theory with far-reaching policy implications for the United States and other Western countries. In modern "bureaucratic" societies political stability—defined negatively as the absence of violent seizures of governmental power—is said to rest on structural integration and a consensus on proper political procedures among the occupants of top "strategic positions" in "powerful organizations." Their admittedly superficial survey of national elites and political stability in some eighty countries in recent times persuades the two authors that what they consider the persistence of stable political institutions under all sorts of regimes is first and foremost a function of enduring and stable elite integration. Since they find the latter condition to be the normal state of the elites in practically all countries, Field and Highley conclude that there is little that Western countries can do to promote stable representative institutions around the world. Such policies are termed "unrealistic" and more likely to do harm than good because they allegedly concentrate on nonelite conditions to bring about desired changes that, in the last analysis, are said to depend on the disintegration of both elite structures and elite consensus.

This reviewer finds these arguments neither particularly original nor especially persuasive. Others, such as Paul Keczkemeti in *The Unexpected Revolution* (Stanford University Press, 1961, p. 149), have similarly argued that regime stability depends on the unity of the ruling elites. And the evidence marshalled by Field and Highley here seems far too tenuous to support their rather emphatic policy conclusions.

LEWIS J. EDINGER

Columbia University

Latin American Democracies: Colombia, Costa Rica, Venezuela. By John A. Peeler. (Chapel Hill: University of North Carolina Press, 1985. Pp. xiii + 193. \$24.00.)

Not too many years ago a book on democracy in Latin America would have met with little interest but, with the recent democratization of the region, we can expect that this

volume by John Peeler is a harbinger of a wave of new studies that will inundate the field.

If anything this is a provocative work. Peeler claims to be representative of the "post-behavioral" trend in political science that rejects the notion that research can or should be value free. He unabashedly promotes liberal democracy as "a good thing" while at the same time condemns it as a sham that stymies the search for true social, economic, and political justice. This position would of course be contradictory if not placed against the background of Latin America's most recent history. Liberal democracy with all its warts, according to Peeler, is preferable to regimes that torture, imprison, and murder their citizens. However, in attempting to go further by suggesting how, once liberal democracies are firmly implanted, Latin American polities can further democratize—creating truly equalitarian, participative democracies—Peeler fails to find a formula for this transformation and is left with the lesser of two evils.

Rare to analysis of Latin American politics, Peeler commendably begins with a discussion of liberal democratic theory and, following the works of C.B. Macpherson, concludes that "liberal democracy is essentially a liberal political system legitimated by the appearance of democracy" (p. 6). In essence, although he believes that liberal political freedoms and guarantees are essential to democratic political life, liberalism—through its defense of property and the inequality resulting therefrom—makes real democracy impossible. This said, however, it seems to be Peeler's belief that liberal democracy is at least a starting point for the creation of real democracy in Latin America. The bulk of the work then is an attempt to show how stable liberal democratic regimes, few that they are, emerge in Latin America, how they might be maintained, and how they may be transcended to create real participative democracies.

Peeler tackles each of these questions with varying success. Briefly examining and rejecting (most often without explanation) the theories of Samuel Huntington, Guillermo O'Donnell, Howard Wiarda, and Goran Therborn in which socioeconomic factors predominate, Peeler finds through a survey of the political histories of Colombia, Costa Rica, and Venezuela that liberal democracy comes about primarily as a result of elite accommoda-

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tion. Elites, for one reason or the other (usually it seems because they are threatened with annihilation from below if they continue to fight among themselves) decide to live and let live. This "political act" of accommodation, while limited and conditioned by both internal socioeconomic and external geopolitical and economic forces, is not determined by them. We are thus left with chance, luck, and fortune. Economic, political, or social conflicts that violently split elites dissolve simply because they will it!

Once liberal democratic regimes emerge, the task then is to preserve them. For Peeler, this is accomplished by a combination of the same elite accommodation and the very form of liberal democracy. The consensus reached by the elites creates a hegemony around which the political debate within the country revolves. Alternatives to this hegemony are eliminated or marginalized while the veneer of democracy—free, honest elections, the right to organize, etc.—evidenced in sometimes strenuous competition for office by elite fractions, masks the domination of the system by the ruling class. With both the right and left politically marginalized, extreme economic, social, and political inequality can be maintained by leaving the voters no alternative but what is constituted as the center—the status quo interest of the elites: that is, the defense of their property and privilege.

Because in Peeler's opinion any kind of attack on liberal democracy (especially attacks from the left) would most likely lead to right-wing military coups, he urges the left to "work within the system"—that is, accept liberal democracy. This of course means that it should abandon any attempt to transform fundamentally the system of inequality and, following that, any transformation to real participative democracy. This produces the conundrum in which Peeler enmeshes himself. On the one hand, he argues that "we should work towards those ends [referring to justice and revolution]" (p. 154), but on the other hand, we cannot rock the boat for fear of a repressive right-wing coup. Although he presents several scenarios for openings to real democratic transformation, none are realistic given his commitment to the maintenance of liberal democracy.

DOUGLAS FRIEDMAN

College of Charleston

Human Rights in the Soviet Union. By Albert Szymanski. (London: Zed Books, 1984. Pp. viii + 338. \$30.95, cloth; \$12.95, paper.)

Szymanski describes himself as a Marxist writing on socialism vs. capitalism. Socialism wins. Szymanski has obvious sympathy for, and appreciation of, the domestic policies and achievements of the Soviet Union. He accentuates the positive.

Szymanski uses the idea of human rights, but makes no attempt to demonstrate that concern for them influences Soviet policy. To demonstrate this would be difficult. The evidence is that the Soviet leaders concern themselves with what they think of as interests of the people, claiming to know those interests better than the people do themselves. In promoting them, the leaders incidentally promote the enjoyment of some rights, and thus they make Szymanski proud. He cites the provision of the constitution indicating that when rights conflict with the official conception of interests, rights must give way, but he pays no attention to the implication that individuals (and whole classes) may be sacrificed when the interests of the people can thus be served.

Szymanski gives good marks to the Soviet Union in each of the areas with which he deals. In doing so he maintains a tone of scholarly reasonableness and musters substantial supporting evidence. Friends and critics of the Soviet Union should all be aware of his arguments. He does least well in assessing Soviet policies on emigration. It is enough for him that those policies are more liberal than the policies of most states have been through history. He does not mention the fact that the Soviet government has explicitly committed itself to the right of everyone to leave any country, including his own.

Szymanski's general theme with respect to minority nationalities is that Soviet policies are benign, respectful of distinctive cultures and contributing to both cultural and material advancement. The consequence is that separatist or anti-Soviet feeling is limited, and where it exists it is based on nationalist traditions rather than on any contemporary oppression by the Russians. The theme with respect to women is that the Soviet government has advanced them into at least as good a status as they enjoy in any other country. With respect to the economy the theme is that despite problems, especially in agriculture, substantial

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growth in production has occurred and standards of living have been rising.

Szymanski approaches the problem of toleration and repression somewhat differently. In effect he grants that the Soviet record is spotty, and he seeks to explain the fact "in materialist terms" (p. 205). His argument is that in certain kinds of circumstances governments of all sorts become intolerant and repressive, and to the extent that this happened in the Soviet Union it was on the basis of a kind of universal law and not because of anything for which a party, a person, or Marxism itself can be blamed. Anyway, he claims the number of deaths and ruined lives associated with collectivization, the purges, and so on, is usually greatly exaggerated. Since the mid-1950s "the range of political tolerance has continually increased" (p. 270). There are some dissidents, but they are few in number and have been able to generate little sympathy or support among the people.

A chapter on "The Land of the Free" goes through the history of denials of civil liberties, especially free speech, in the United States. The record is troubling, but it is important that the United States champions free speech in both its law and its ideology. The Soviet Union does not, giving priority to alleged interests of the people and to the cause of socialism as interpreted by the Communist Party. Crediting the Communist Party with serving the interests of the people, Szymanski pays little attention to the question whether it is responsive to their will; and he is silent on the question of the extent to which the party renders itself accountable to them.

It is interesting to contrast Szymanski's book with Richard Pipes, *Survival Is Not Enough* (New York: Simon and Schuster, 1984). The two authors disagree more in their selection of subject matter and in their interpretations than in their facts. Pipes accentuates the negative. Those teaching courses on the Soviet Union might find it interesting to assign both books, for they provide antidotes to each other and illustrate that scholarship is not always uninfluenced by the ideological predispositions of the scholar.

VERNON VAN DYKE

University of Iowa

Big Structures, Large Processes, Huge Comparisons. By Charles Tilly. (New York: Russell Sage Foundation, 1984. Pp. xii + 176. \$14.50.)

Tilly asks how social scientists can improve their understanding of those large-scale structures (e.g., systems of states) and processes (e.g., capitalism and the formation of powerful national states) that transformed the world of the nineteenth century and continue doing so today. On the negative side, he answers that social scientists must abandon eight pernicious myths of nineteenth century thought that still encumber twentieth century social science. On the positive side, he proposes two classificatory schemata, one for distinguishing the analysis of structures and processes at four historical levels and another for comparing big structures and large processes.

The eight pernicious myths of nineteenth century folk wisdom are: (1) *Society* is a distinct entity; (2) society affects individual minds which, in turn, produce social behavior; (3) there is a general phenomenon known as social change; (4) large-scale social change takes distinct societies through evolutionary stages; (5) the mainspring of large-scale social change is *differentiation*; (6) *social order* reflects a balance between differentiation and *integration* (or control); disorder results from rapid or excessive differentiation; (7) the strains of excessive social change produce crime, suicide, rebellion, and the like; and (8) legitimate forms of conflict, coercion, and expropriation flow from the processes of integration and control—illegitimate forms flow from change and disorder.

If we grant that the term society is a reification of multitudes of structures and processes that are seldom, if ever, bounded in time, space, and personnel, then most of Tilly's other myths fall as well. Thus, if society is a fictitious entity or communal ghost, it cannot be the ultimate source of social behavior. Social change must be disaggregated into such processes as urbanization, industrialization, and bureaucratization, and there can be no general "stages" of a society's evolution. Similarly, if there is no coherent, aggregated process called social change, then differentiation cannot be the mainspring of social change and the juxtaposition of differentiation and integration fails accordingly. Finally, the notion

that certain social facts (e.g., suicides and crime) result from the strains of social change fails because there no longer is a coherent process of social change, and the distinction between legitimate and illegitimate forms of force becomes impractical and obfuscating.

Tilly complements his exposure of the eight pernicious myths with a call for concrete (real times, places, and people) and historical (limited in scope to eras bounded by the playing out of well-defined processes) analyses of big structures and large processes. He cites four historical levels of analysis: (1) the world-historical, comparing the different major eras of human history; (2) the world-systemic, analyzing the contours and operations of each major era; (3) the macrohistorical, accounting for big structures and large processes and their alternate forms within a given era; and (4) the microhistorical, tracing the encounters between individual and groups and these big structures and large processes. Tilly's preferences lie with the macrohistorical and microhistorical.

Tilly also examines four methods of comparing big structures and large processes. He distinguishes "share of all instances" (getting the characteristics of one or many cases historically right) and "multiplicity of forms" (whether a phenomenon has one or many forms). From these two dimensions derive four forms of comparative inquiry: (1) individualizing, where each case is treated as unique; (2) universalizing, where properties common to all instances of the phenomenon are identified; (3) variation-finding, or the search for a pattern of variation through systematic examination of differences among all instances of a phenomenon; and (4) encompassing, or explaining different instances in terms of their location within and relation to the system as a whole. The four forms of classification stem not from the logic of comparison, per se, but from the relation of observation and theory or the questions being asked. Which of the four approaches are better for discovering "truth" depends, according to Tilly, on the actual (even if unknown) structure of the world being explained.

This book deserves the careful attention of political scientists. To be sure, as Tilly admits, he exaggerates the unity of nineteenth century thought and by so doing exaggerates its stranglehold on modern social science. Still, a

core truth remains, as anyone troubled by the careless use of abstract concepts (e.g., society, differentiation, and social change) would agree. Tilly's preference for the macrohistorical and microhistorical surely is his choice, just as preferences for world-historical or world-systemic may be the legitimate choices of other scholars. More disconcerting, however, is the notion that history occupies an epistemological plane such that "sociologists, anthropologists, political scientists, and an occasional economist have begun to work at getting the history right before generalizing, in order to be able to generalize soundly" (p. 79). Alas, I find "right" and "wrong" history as limited as the distinction between legitimate and illegitimate force.

I perhaps am most troubled by the typology of comparative inquiry. Tilly readily admits that the classification schema depends neither upon the internal logic of comparison nor upon the existential referents being compared. The typology stems instead from what various researchers are attempting to do. This caveat will of course not prevent some from inappropriately using the typology as a guide to comparative inquiry. More immediate, however, is the bet (p. 147) that all four varieties will have their place in comparative inquiry and that encompassing comparison will come into its own. Here Tilly too appears to use the schema as a logic. We may well face the challenge of integrating big structures, large processes, and huge comparisons into history, but our success ultimately will rest on an equally systematic attention to a philosophy of the social sciences.

STEVEN THOMAS SEITZ

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First Among Equals: Prime Ministers in Westminster Systems. By Patrick Weller. (Sydney: George Allen & Unwin Australia, 1985. Pp. ix + 228. \$25.00.)

Patrick Weller seeks to contribute to the sizeable collection of studies of executive power with a comparative study within the general institutional framework of cabinet government. Considering the United Kingdom, Canada, Australia, and New Zealand, he makes his overall question the power of the prime minister, defined "as the ability of prime

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ministers to have an impact on the intended actions of governments" (p. 180).

Wisely following the lead of Anthony King, Weller breaks down the global question into several different factors and analyzes the institutional constraints on prime ministers in each of his selected nations. Thus he describes the relationships of parties and party leaders; their selection and vulnerability; their patronage; cabinet relationships; personal advisory supports; parliamentary relationships; and roles as public figures.

Weller hopes that by comparing these verbal descriptions—and intelligent estimates of who has power and what are the constraints—one can distill out useful advice on the operation of the office. For example, if Tony Benn urges that cabinets be elected by the parliamentary party, one can look at the experience of the Australian and New Zealand Labor/our Parties and find "evidence" concerning these arrangements. Incidentally, he decides it does not make that much difference, as PMs must see various factions represented in Cabinet, cannot avoid certain leaders, and will neutralize enemies with responsibility.

The studies are intelligent and compact institutional descriptions and verbal assessments, with limited generalizations cautiously drawn from the mass of activity which these four countries represent. One can learn of party organization and rules, cabinet practice and traditions, and personal reactions of observers and participants in each of his nations.

In the end, Weller attempts the impossible and does it as well as anyone could: he rates prime ministerial power in each country and each major party on seven representative and important variables. Very tentatively, he suggests British Conservative leaders are most powerful, and the Labor/our Party leaders of Australia and New Zealand least. He immediately qualifies, and notes that no one set rates highest or lowest on all variables, and that certain individuals could make better use of their available leverage than others. After reviewing the institutions, he refuses to attempt to draft a prime minister's "job description," but argues that some institutional relationships aid a prime minister better than others.

Weller's judgments must necessarily be subjective and controversial, although he qualifies and states with intelligent caution. He tends to come down against the Crossman thesis on

British ministers and notes wryly that each nation's observers appear to believe their prime ministers most powerful. While he notes the enormity of the task of ferreting out the many variables in four similar but different sets of institutions, he glosses over the fundamental problem of all literature dealing with power comparisons: that power is, as he says, transactional. Generalization and quantification must be, in any precise sense, impossible. As a student of comparative politics, this reviewer likes comparative analyses, but he must also admit it compounds the problem geometrically. But the questions are there, and Weller does a pithy job of dealing with them. He will be cited on this subject for some time to come.

One final note: Weller sets out looking for, primarily, the institutional constraints of his subjects' powers. One wonders whether his answers would be the same if he had started with cabinets, or individual departmental ministers, and listed the constraints on them.

JOHN H. MILLETT

Wichita State University

State Politics in Contemporary India: Crisis or Continuity? Edited by John R. Wood. (Boulder: Westview, 1985. Pp. xiii + 257. \$15.00.)

The domestic politics of India, both central and state, engaged the devoted attention of a cohort of able, young, North American political scientists in the 1960s. The focus of the discipline has shifted since then. The political development framework they used has come under attack by political economists, Public Law 480 funds have dried up, and access to India has been severely reduced by the restrictive visa policies of its government. Significantly, there are no case studies of the Punjab, Assam, or Kashmir in this volume. Yet some of those who dedicated their professional lives to this specialty have persisted with increasing sophistication and the present work is therefore the best on the topic in almost 20 years. Whether it will receive the attention it deserves from fellow political scientists, however, is doubtful because the authors assume substantial background knowledge of India in their readers, as was appropriate for

the Asian Studies meeting to which the papers were originally addressed, but which will inhibit comparisons of India with other democratic federal systems.

Unlike many such collections, *State Politics* does nevertheless have a theme and a set of questions each contributor is asked to answer. The theme is the growing importance, yet also increasing diversity, of state politics in all-India governance. The main question is whether the 1980 election (and, despite Indira Gandhi's departure from the scene, one might now add the 1985 poll) marks a return to normality in the "Congress system" or whether political decay in the form of de-institutionalization has gone too far to avoid a revolutionary crisis. All contributors opt against the latter possibility, at least in the short run.

All of them (Paul Brass on Uttar Pradesh, Harry Blair on Bihar, Atul Kohli on West Bengal, Ouseph Varkey on Kerala, James Manor on Karnataka, Jayant Lele on Maharashtra, John Wood on Gujarat, and Roderick Church as discussant) also focus on the struggle of the "backward" lower (but not untouchable) castes against the "forward" upper castes for their share of the political and status spoils of the system. Each has to struggle with how to assign the significant sub-castes or *jati* to both the Hindu ritual hierarchy of the *varnas* or castes and to economic classes since, except for the top (Brahmins) and the bottom (Untouchables), these do not cut across regional boundaries. In general, their "Forwards" are not Brahmins or big landlords, but dominant middle-peasant sub-castes, ominously termed by Brass and Blair as Kulaks, farmers who employ landless laborers. The "Backwards," who now demand reserved quotas like those already accorded the Untouchables and Tribals, are poor peasants or artisan sub-castes. Most of the authors recognize the poor fit between caste and class because of large variations in economic status within each *jati* and consequent overlap. Kohli is the weakest in this respect as he relies on the Bengali category of the Bhadrak (respectable people) which is an educational and occupational term, without explaining its sub-caste composition. It is hard to accept his assertion that caste is weakest in Bengal. Lele resorts to the term elites without making it clear whether these are positionally or ascriptively defined.

Blair is the only contributor who gives the

Political Economy approach any more than token application, and his work is the most statistically sophisticated. Varkey has the least economic background data. But all the writers share an unthinking liberal use of the terms "progressive" and "reactionary." The question is only whether growing politicization and participation by the Backwards in an economy of scarcity has increased corruption and violence to the point where democracy is in jeopardy. Implicitly their sympathies are with the downtrodden, except for Brass who gives a favorable account of Charan Singh's agricultural program for the rich and middle peasants. There is short shrift for the relative deprivation of the "Twiceborn" *varnas* whose natural resistance to downward mobility has been increasingly violent and manipulative at the price of noblesse oblige. None of the studies except the editor's on Gujarat deals with "Sanskritization," the ritual mobility of lower sub-castes through adopting the values and lifestyle of the upper. He shows how Rajputs in Gujarat extended their Kshatriya (warrior) status downwards in order to increase their vote banks.

THEODORE P. WRIGHT, JR.

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Albany*

Industrial Policy: Business and Politics in the United States and France. Edited by Sharon Zukin. (New York: Praeger Publishers, 1985. Pp. xi + 256. \$36.95.)

This volume is based primarily on papers given at a conference on state policy and business strategy in the United States and France in October, 1983. The result is 21 separate chapters, all of them relatively short, and each one gives a substantive overview on some aspect of the topic. The chapter authors are a fairly heterogeneous lot, about half being full-time academics while the rest, some of them authors, are active in organizations directly involved in industrial planning for the two countries.

To the editor, the core of industrial policy is reindustrialization which involves a state-led effort to reconstruct a nation's industrial base, something which cannot be done by market forces alone. This process demands the chan-

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neling of investment capital to industries that have realistic prospects for expansion given a country's resources. On the other hand, industries on the decline demand rationalization, deinvestment, retraining of workers, and the introduction of new industries into regions most hard hit by the presence of declining industries. The state is to lead the way with subsidies, tax expenditures, and regional and export programs that will bring about the necessary national economic transformation.

Does a comparison of business and politics in the United States and France shed any light on what is effective industrial policy? The editor argues yes because state-centered France represents a proper contrast with the market-oriented United States. The contrast, however, turns out to be more one of rhetoric; that is, from the American side there is a debunking of the tools of French industrial policy. Yet when the United States takes hesitant and small steps toward an industrial policy, the basic shape of the implementing ideas is virtually identical.

There are many tools of French industrial policy, but among the most interesting is the idea of a *filière* or production channel. Christian Stoffaes, director of the Office for Computer and Data-Processing Industries in the French Ministry of Research and Industry, explains this as a strategy of a nation to control the chain of production within an industry from the basic raw materials to the final consumer product. Such a strategy recognizes the synergies among firms in an industry especially between parts suppliers and industrial consumers and among the main manufacturers themselves. In the French case, the *filière électronique* or electronics complex seems to be especially successful in the 1980s.

While this volume is very useful to beginning students of industrial policy and those who want to discover the basic facts and current trends of industrial policy in the United States and France, there is little new for

political scientists with a rudimentary background in these areas. The main reason for this is that the authors cover the conventional wisdom in a general way. There is no new research reported on here and 9 of the chapters have neither notes nor bibliographic references.

Substantively, a noteworthy gap exists in both the literature and this volume that concerns the interaction of the state and business over the public policy development for industry—that is, the role of business interest associations (BIA's). For all too many authors and participants in this policy process the significant players are individual business firms and the state. The BIA's are ignored, an unfortunate omission since there is good reason to believe that they can and do provide effective leadership in industrial policy. For example, industry-wide associations can be very useful in research and development by eliminating duplication of efforts by firms, by obtaining economies of scale, and by quickly communicating this knowledge to individual firms in an industry.

Finally, there are several ways the volume could have been improved. The introductory chapter should have been the basis of a concluding one which built upon the essays of the contributing authors. The chapters should have been fewer in number, longer, and written by academic scholars. These chapters could have been introduced by the editor who then could explain to the reader why the chapter topics and authors were chosen. Conferences can result in useful anthologies such as this one, but with revisions, paring down of contributors, and extensive editorial work, such volumes may become enduring aids to academic scholars.

HENRY J. JACEK

McMaster University

INTERNATIONAL
RELATIONS

Regionalism and Global Security. Edited by Gavin Boyd. (Lexington, Mass.: Lexington Books, 1984. Pp. vii + 194. \$25.00.)

Gavin Boyd has edited and contributed much to a fascinating and timely book. There is little doubt that the present world is quite different from and more complex than the raw power-centered world many expected (and hoped!) would occur after 1945. Political science practitioners have made major progress in catching up to reality and now talk the language of interdependency. Today many see international relations in more complex interactive terms. There is an inclination to recognize the need to discuss issues not as politics or economics, but as political economics, which is a helpful step forward. Boyd and those whom he has assembled move in a related profitable direction by hooking political and economic cooperation to a lessening of international tensions.

One of the pleasant aspects of the book is that Boyd has orchestrated a unified, yet edited, discussion. All of the essays treat common themes: the possibilities for cooperation within a variety of interdependent relationships, and, perhaps more importantly, the constraints that continue to inhibit such potentials. There is an evenness in tone and argument across each of the essays. This reviewer found them to be basically well argued and thoughtful, if not always finally persuasive. The following roster makes a worthwhile read: "Foreign Economic and Security Policies," by Gavin Boyd; "Atlantic Relations," by Phil Williams, William Wallace, and Stephen Woolcock; "African Community Building," by Sam C. Sarkesian; "Pacific Community Building," by Gavin Boyd; "Latin American Regionalism," by Gavin Boyd and Yale Ferguson; "Middle East Community Building," by Mary Ann Tetreault; and "Regional Design," by Gavin Boyd.

While I believe that the several authors are surely pointed in the proper direction, there are several points with which I have some rather deep reservations. These disagreements stem from what I perceive to be a general

nonrecognition of the deep-seated structural reasons why some of the constraints function as profoundly as they do to inhibit, indeed one should say prohibit, meaningful planning for cooperation. For example, the fundamental tensions and inconsistencies present in liberal political economics at the domestic and international levels are overlooked. The liberal separation of political and economic agenda building and implementation stand directly in the way of making progress in the way that the authors desire. There is much more gravity than the several authors argue here in the liberal system of economically driven political decision making.

This characteristic of stopping short of the more convincing case is again evident in the authors' elaboration of the crucial roles played in each region by the United States and other dominant players. They document clearly that the dominant nations in the various regions generally act to constrain cooperative actions rather than to enhance the possibilities for them to occur. But the authors do not go on to the next step which would be to examine why in terms of decision-making structures what they observed occurs. This book provokes thought. The themes are relevant and I hope it helps generate the sort of meaningful discussion which the topics of regionalism and regional cooperation in the context of global security deserve.

CARL PINKELE

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National Security Strategy: Choices and Limits. Edited by Stephen J. Cimbala. (New York: Praeger Publishers, 1984. Pp. xi + 371. \$34.95.)

Cimbala's text is the latest volume of the Foreign Policy Issues Series produced by the Foreign Policy Research Institute (FPRI). This is a collection of essays loosely organized around various concepts in national security strategy, which Cimbala defines as "a process for making decisions, a way of relating the

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national organism to its environment" (p. 1). In his brief introduction to the essays, Cimbala posits strategy as purposive, manipulative, systematic, dialectical, and political. His principal criticism of U.S. national security is that we have such little strategic concept in our national strategy. Instead, we "have substituted technology and slogans for strategically conscious policies. Our strategies have been reactive to Soviet objectives and criticisms . . . rather (than) to our own requirements" (p. 6).

The book offers no panacea. Cimbala believes that only "marginal improvements are possible" in national security policy making. The essays themselves certainly operate in the marginalia, offering few significant proposals for change—except for the five essays which address strategic forces. Those essays are provocative and controversial; they are certain to upset many readers with their frank discussion of nuclear war-fighting strategies and capabilities.

The writers are principally political scientists who, like the prevailing theme at FPRI, hold somewhat conservative views of international politics. Most hold dual credentials, in that they have served both in government and in academia. Thus, their essays reflect considerable pragmatism and are sensitive to the political/bureaucratic realities of transforming national security policies.

Nevertheless, the nuclear strategy writers are clearly dissatisfied with current nuclear strategies, even in the Reagan era; they propose the enhancement of strategy, if not of force levels. Colin S. Gray suggests that U.S. nuclear strategy is confused and that our objectives are irrelevant. Successful war fighting requires a commitment to victory, however defined, in order for deterrence to succeed. Gray advocates more active and passive defenses, survivable command and control systems, and more flexible targeting.

In a similar vein, David Tarr suggests that the U.S. shift from a policy of nuclear deterrence to one of nuclear defense. Unlike the current administration's defense policy of "more of everything," Tarr believes that more nuclear defensive systems would substitute for some nuclear offensive (deterrent) systems. Thomas A. Fabyanic also takes issue with the Reagan administration, as much for its method of making strategic decisions as for its strategy.

Like other writers here, Fabyanic urges more study of the history and nature of warfare, rather than strategic analysis based upon statistical theory. This administration, like others, has used a decision-making process that "has relied less on the conceptual aspects of warfare and more on data outputs of various quantification techniques" (p. 259).

Political scientists in general probably will enjoy most the three short concluding chapters on the nature of strategic policy making within the executive branch. These are high quality analyses easily adaptable to textbooks on American government, written by seasoned scholars: Vincent Davis, Sam Sarkesian, and John Allen Williams.

The text suffers to a very small extent from the glitches that usually plague national security studies: occasional items of misinformation, dated material in a highly-perishable field of study, insufficient explanation of tables drawn from government studies without accompanying narratives, or non-standard (and unexplained) abbreviations. These are minor problems in an otherwise well-prepared text.

The greater problem is conceptual. Cimbala suggests that we have begun a new great debate on national security policies because the strategic environment has changed dramatically in recent years. He offers "a conceptual perspective" of strategy, which is then largely ignored by the writers. This is not to fault the essays, for they are generally excellent. The organizing theme, however, disappears from view and the reader is left with a 1982 snapshot of selected portions of the "new great debate." It is difficult to assemble a collection of policy essays which remain consistent to a central framework for analysis, especially when written by political scientists who are justly proud of their iconoclasm.

ABBOTT A. BRAYTON

East Tennessee State University

Citizens and Soldiers: The Dilemmas of Military Service. By Eliot A. Cohen. (Ithaca, N.Y.: Cornell University Press, 1985. Pp. vii + 227. \$22.50.)

This is an outstanding analysis of America's difficulties in settling on a durable system of

peacetime military service. *Citizens and Soldiers* certainly belongs on the reading list of any course on national security policy.

Cohen's book is distinctive because it excels in three areas: historical development, comparative analysis, and philosophical dimensions of public policy. Most authors would have been satisfied with highlighting the history of military service in the United States. But Cohen compares and contrasts America's experiences with those of other major as well as small nations. He also discusses military service in terms of liberalism and egalitarianism. Cohen analyzes "the difficulties that stand in the way of a sound American manpower policy: the strategic problem of having to prepare to fight two very different kinds of war; the philosophical problem of reconciling opposing notions of just obligation; and, overarching both, the practical difficulties imposed by the demands of war and normal political life" (p. 152).

One of the most interesting sections of the book is the discussion of the all-volunteer force. The author argues that the assumptions of the all-volunteer force reflected the doctrines of liberal (free-market) economists. That, combined with an unpopular war, discontent with the draft, and a short-term increase in the pool of available manpower, made the all-volunteer force politically irresistible.

What has occurred, according to Cohen, is "a determined and desperate effort on the part of Democratic and Republican administrations to prove that the all-volunteer force was working" (p. 169). The four types of criticisms of the all-volunteer force (unrepresentativeness, difficulties in maintaining numerical goals, declining quality, and problems in overall combat efficiency) are difficult to dismiss—especially since it really has not been tested in a major war.

Cohen's suggestions for improving America's manpower policy are not fully satisfactory; but, admittedly, there are no facile solutions. He favors a "universal militia draft" (p. 187). He believes that a successful military manpower policy must "(1) . . . be consistent with both liberalism and egalitarianism. In other words, service should be brief and limited, but all or almost all young men should serve. The program must have a clear military rationale. (2) The system must pro-

duce effective forces for both total and small wars: this means that the standing army must consist of volunteers. (3) The system must not be excessively costly, nor may it place excessive organizational and manpower burdens on the armed forces" (p. 186).

Cohen dismisses two alternatives to this policy: maintaining a fragile all-volunteer force or reducing it and transforming it into a professional force. However, this latter alternative seems more likely than Cohen admits—even if it would require major withdrawals from Europe and a substantial alteration of U.S. foreign policy interests.

ROBERT H. PUCKETT

Indiana State University

Power in Numbers: The Political Strategy of Revolt and Rebellion. By James DeNardo. (Princeton: Princeton University Press, 1985. Pp. xiii + 267. \$25.00.)

For the past 20 years political scientists have tried to understand why people would choose, on a rational basis, to revolt against the existing political order. Early attempts to build rational choice theories of protest and rebellion encountered what seemed to be an insurmountable obstacle: the free-rider problem. Individuals bear the costs of protest, in terms of time and, frequently, government repression. Yet a single individual's participation will not noticeably affect the probability that the protest will succeed in effecting desired changes. Since a person will reap the rewards of any changes that might occur if the protest is successful, whether he participates or not, why should he participate?

In recent years, several people have tried to get around this obstacle by introducing assumptions that would mitigate or eliminate the free-rider problem. DeNardo's book is the most ambitious of these attempts. In this book the free-rider problem is eliminated by assumption. DeNardo assumes that people don't discount the benefits of a successful protest by the probability that their individual contribution changes the outcome, but rather simply consider whether their preferred policies are closer to those enacted by the government or those advocated by the protestors. In the first part of the book (dealing with peaceful protests and

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no repression) this is the only consideration that determines whether an individual protests or not. In later chapters, DeNardo adds to this basic mechanism the costs of government repression and the use of violence by protestors. On the government side, DeNardo assumes that leaders have a (unitary) preferred policy position but are willing to compromise on this in order to reduce either the number of people who are protesting or the level of violence.

With these basic assumptions, DeNardo then analyzes the strategy choices of protest leaders. His analysis focuses on two levels. First, what strategy will yield policies closest to those preferred by protest leaders? Secondly, if protest leaders differ over the policies they most prefer or the value of a compromise, how will factions and coalitions among protest leaders develop? DeNardo reaches interesting conclusions on both levels. He offers a persuasive argument as to why leaders who are unable to attract enough support to succeed in changing a policy to one that meets their ideal may actually secure larger concessions from the government if they either moderate their demands or, alternatively, engage in violent protests. He is also persuasive in explaining why general conclusions on the impact of moderation will not hold under more complicated scenarios that include government repression. These scenarios are important because optimal strategy becomes indeterminate, not as a matter of principle, but rather because the amount of information required to make a determination is staggering and generally unavailable both to protest leaders and outside observers. This indeterminacy is also relevant to DeNardo's analysis of factions and coalitions among different protest leaders. Although DeNardo's analysis of factions rests largely on the preferences of leaders and the responsiveness of government, one could argue that a third factor—information estimates—may also enter the picture, probably to reinforce the prior inclinations of protest leaders.

To some extent, the strengths of this book are also its weaknesses. As one example, DeNardo ignores the free-rider problem and as a result can carry out a complex analysis of rational strategy choices. On the other hand, although free-rider problems are not as pervasive as expected utility analysis would make

them out to be, it does not seem reasonable that rational actors should evaluate public goods as though they were private goods. As a second example, DeNardo often adds a variety of substantive assumptions about preferences. This "fleshes out" the theory and ties it in with other work in the field. On the other hand, conclusions may then be very close to assumptions and not very different from arguments that are already well known as, for example, that repression may either increase or decrease participation because it both generates anger and imposes costs. Finally, the amount of indeterminacy in strategic choices is interesting and may serve to explain why it is so difficult to predict mobilization or outcomes of protest movements. This indeterminacy, however, is also an obstacle to empirical tests. When all is said and done, the most important contribution of this book is that it extends a rational analysis of protest beyond that accomplished by previous works. If protestors are rational (and I assume they are) that is an important contribution.

BARBARA SALERT

Washington University

A Continent Apart: The United States and Canada in World Politics. By William T. R. Fox. (Toronto: University of Toronto Press, 1985. Pp. xv + 188. \$10.95.)

Canada and the United States: Enduring Friendship, Persistent Stress. Edited by Charles F. Doran and John H. Sigler. (Englewood Cliff, N.J.: Prentice-Hall, 1985. Pp. vi + 256. \$15.95, cloth; \$7.95, paper.)

Prompted in part by controversial economic, military, and environmental policies which strained ties between Washington and Ottawa in the 1980-83 period, the North American academic community has shown a renewed interest in contemporary Canada-U.S. relations. These volumes are but the latest in a series of publications which explore the nature of the relationship between the two neighboring nations. (See also John W. Holmes, *Life With Uncle: The Canadian-American Relationship* [Toronto: University of Toronto Press, 1981]; Stephen Clarkson, *Canada and the Reagan Challenge* [Toronto:

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James Lorimer, 1982]; Kenneth M. Curtis and John E. Carroll, *Canadian-American Relations: The Promise and the Challenge* [Lexington, Mass.: Lexington Books, 1983]; Edelgard E. Mahant and Graeme S. Mount, *An Introduction to Canadian-American Relations* [Toronto: Methuen, 1984]; and Charles F. Doran, *Forgotten Partnership: U.S.-Canada Relations Today* [Baltimore: Johns Hopkins University Press, 1984].)

Americans basically take Canada for granted. In most surveys, Americans perceive Canada as being the nation which is the "most friendly" to the United States. On the other hand, they rarely reflect on how important Canada is to the United States, both strategically and economically. For example, even two of the past four U.S. presidents have publicly referred to Japan as being America's leading trade partner, when in fact trade flows between the U.S. and Canada are much larger, and the U.S. actually exports more to the province of Ontario alone than it does to Japan.

The Fox book is derived from a series of lectures which the professor emeritus from Columbia presented at the University of Toronto in 1982-83. Fox unabashedly considers North America north of the Rio Grande as a "continent apart" (pp. 6 and 144). Two of his seven chapters (chap. 1 and 4) examine the special characteristics of a "two-country North America" (p. xiv), two (chap. 2 and 3) explore within a broad historical context the involvement of other states in North American affairs, and three (chap. 5-7) look specifically at the evolution of Canada-U.S. relations and the range of policy choices available to each nation in the contemporary international arena.

The scope of the book is too limited and the discussion of current bilateral and multilateral issues too brief for this volume to serve as the principal text in college courses devoted to U.S.-Canada relations. Moreover, the author would have served his audience well by adding another chapter on the lessons to be learned from how the U.S. has interacted with Canada in comparison to how it has pursued relations with the rest of North America north of the Isthmus of Panama. On the other hand, Fox's detailed discussion of the "miraculous non-event" (p. 33), the fact that the U.S. did not formally annex Canada, and the complex strategic challenges currently facing each

nation (chap. 6) would be good supplementary material for both U.S.-Canada and basic U.S. foreign policy courses.

The Doran and Sigler edited volume was prepared under the auspices of the American Assembly and the Council on Foreign Relations. Twenty years earlier, the American Assembly had sponsored its last volume on Canada-U.S. relations under the able editorship of John Sloan Dickey. Because of the vital importance which should be attached to this bilateral relationship, and the multiplicity and complexity of outstanding issues, one hopes that we will not have to wait another two decades for the next volume in the series.

This academic endeavor is truly binational, with the U.S. academic, Doran, and his Canadian counterpart, Sigler, jointly providing a brief but insightful introduction, and a more lengthy concluding essay on change and continuity in Canada-U.S. relations over the past 20 years. Canadian university representatives Gordon Robertson, J. L. Granatstein, Richard G. Lipsey, and Albert Legault examine the bilateral relationship respectively from the vantage points of federalism, post-World War II diplomatic history, economics, and defense. U.S. professors Seymour Martin Lipset and Lynton K. Caldwell tackle the cultural and environmental issues.

On the whole, this is a very informative volume and is highly recommended as a supplementary text in courses devoted to the bilateral relationship. One minor weakness is that the authors are not certain about the nature of their audience. Several rightfully assume that the reader may not be conversant with the current literature and issues, so they provide good introductory and background material. A few of the authors, however, apparently took for granted that their audience has followed developments in the bilateral relationship on a regular basis. This is particularly the case with the Caldwell chapter and his discussion of Poplar River, the Garrison Diversion, and Richelieu-Champlain, and to a lesser extent with Lipset's cataloging of authors and publications, and Legault's periodic lapses into jargon. All three of these pieces, however, do provide some invaluable insights and lay readers should take the time to digest the material.

Lipsey refers to an "asymmetry of knowledge" (p. 69) and surveys have indeed shown

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conclusively that Canadians know much more about the United States than U.S. residents know about Canada. Nonetheless, it is disturbing to find that so many Canadian scholars and government officials have such generalized and sweeping notions about the policy-making process in the U.S. (see pp. 66-68, 83). Apparently, few make an honest effort to grasp the complex nature of relations between the executive and legislative branches in Washington, or the impact which federalism, bureaucratic in-fighting, and electoral pressures can have on the resolution of such pressing issues as acid rain and liberalized trade. When this tendency among certain key officials and the attentive public north of the border is combined with the general ignorance and lack of awareness about Canada which is so pervasive south of the 49th parallel, one worries at times about the ability of the two governments to cope with a very extensive and extremely complex bilateral policy agenda.

Fortunately, more attention is now being accorded to various dimensions of Canada-U.S. relations by the academic community in both countries, and the Doran-Sigler book is a noteworthy addition to the available literature.

EARL H. FRY

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Detente and Confrontation: American-Soviet Relations From Nixon to Reagan. By Raymond L. Garthoff. (Washington: The Brookings Institution, 1985. Pp. xviii + 1147. \$39.95, cloth; \$16.95, paper.)

As the size of this book testifies, the causes of complex events are rarely quickly—or briefly—told. Raymond L. Garthoff, a distinguished Soviet specialist with both academic and foreign service experience, now a senior fellow at the Brookings Institution, has undertaken the ambitious task of chronicling the rise and—at much greater length—the collapse of detente. The book is huge, unwieldy, awkwardly organized, and ultimately, profoundly depressing. It is also a remarkable scholarly achievement: quite possibly the most important study of Soviet-American relations

to have been published during the past decade.

Garthoff's sources for so recent a topic are of course limited. He depends heavily, but by no means uncritically, upon the memoirs of such key participants as Nixon, Kissinger, Ford, Carter, Vance, Brzezinski, and Haig. He has mastered the relevant secondary literature by Western scholars, as well as a surprisingly extensive array of Soviet writing on detente, not all of it as predictable as one might think. Garthoff's long career within the State Department (most recently as Ambassador to Bulgaria), has also provided him with a fair amount of inside information, which he puts to good use. And he has even taken advantage of a quirk of fate—the 1979 seizure of the American embassy in Teheran, and the Iranians' diligence in pasting back together what came out of its paper shredder—to draw upon once-classified documents detailing events prior to the Soviet invasion of Afghanistan. The result is a sophisticated multi-dimensional analysis, unmatched in breadth, depth, and thoroughness by any other treatment of the subject now in print.

It is also certain to be controversial, because of Garthoff's insistence that the United States must bear primary responsibility for the demise of detente. There was, he argues, no deliberate or sustained conspiracy to bring this about, other than the one orchestrated by the late Senator Henry Jackson, who does not come off at all well in this book. What happened, rather, was a triumph of expediency over strategy: an accumulation of shortsighted and mostly even well-intentioned blunders that had the cumulative effect of arousing suspicion, ill-will, and ultimately a deep sense of insecurity in the Kremlin. Washington's errors, Garthoff suggests, were of two kinds: good strategies wretchedly executed, and wretched strategies wretchedly executed.

Richard Nixon and Henry Kissinger fall into the first category. Garthoff has no real quarrel with what they were trying to do, but he argues—with all the enthusiasm one might expect from someone who witnessed the process from a largely-bypassed State Department—that they did it very badly. Excessive secrecy and centralization, however purposeful they may have seemed at the time, in fact caused confusion, an inability to draw upon expert advice, and ultimately such widespread cynicism as to preclude building

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any effective domestic base of support for detente. Garthoff accepts Kissinger's thesis that Watergate weakened the administration's hand at a critical moment, but he points out that this was an entirely self-inflicted wound. The Russians did nothing to cause it, and even regretted its effects.

But the Republicans did at least have a strategy—the Democrats, Garthoff asserts, lacked even that. Carter himself was an earnest lightweight; his secretary of state was passive and ineffectual; and his national security adviser was an irresponsible, militantly anti-Soviet alarmist, given to outbursts of paranoia about imaginary "arcs of crisis." The Carter administration botched the SALT negotiations by appearing to renege on previous commitments; it irked the Russians by stressing human rights; it chose to complete Kissinger's rapprochement with China without Kissinger's caution that such a move must never appear to be directed against the Soviet Union; it allowed itself to be distracted by unnecessary flaps over the neutron bomb, the deployment of theater nuclear forces in Europe, and the Soviet combat brigade in Cuba; and, finally, it grossly misread the sense of reluctance and defensiveness with which the Soviets moved into Afghanistan. Garthoff maintains that detente was dead by the time the Reagan administration took office. Indeed, his brief epilogue treats the current administration with surprising respect, perhaps because he expects little from it. The Russians, he notes in passing, have been similarly respectful.

Whatever one thinks of this overall argument, it deserves to be taken seriously. Garthoff is a responsible and well-informed student of Soviet-American relations. His book is carefully, even elaborately, documented. It will certainly be difficult, after reading his account, to hold to the opposite viewpoint: that it was the Russians, and the Russians alone, who torpedoed detente.

But does the evidence sustain the thesis of primary American responsibility? Garthoff himself does not wholly exculpate the Russians. They have, he points out, meddled to no very good purpose in Latin American affairs; they have sought to encourage anti-U.S. sentiment in the Middle East; they certainly did not explain very effectively their SS-20 deployment in Europe; and they badly misjudged the impression their invasion of Afghanistan

would make. He also demonstrates, though, how a succession of quite unrelated American initiatives—the reconciliation with Sadat, the modernization of American strategic forces, and recognition of the People's Republic of China—might have appeared, from Moscow's perspective, as a plot to undermine detente. He even constructs a plausible explanation for Afghanistan on the grounds that the Kremlin feared growing American influence there.

Unfortunately, Garthoff makes no comparable effort to accord legitimacy to American fears, and herein lies the principal weakness of his book. International relations, after all, involves a mixture of both the rational and the irrational. No government can be entirely successful in separating one from the other; many do not even try. Garthoff blames American officials for setting off irrational concerns in the minds of Kremlin leaders, but he seems unable to acknowledge that the process could have worked the other way as well. Indeed, the American political system, with its constitutionally-mandated division of responsibility for foreign affairs, may well be more susceptible to such dislocations than its more highly-centralized Soviet counterpart. And yet, it is the United States that is faulted for not being sensitive to Russian concerns.

In a policy-related sense, it is useful to have this argument made. There is always a gap between one's intentions and the effects of one's actions, and Garthoff's book provides convincing illustrations of how this can happen. But as a balanced historical account of the collapse of detente, written with equal attention to both rational and irrational perceptions on each side, this volume, for all its impressive qualities, falls short.

JOHN LEWIS GADDIS

Ohio University

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Dilemmas of Government: Britain and the European Community. By F. E. C. Gregory. (Oxford: Martin Robertson, 1983. Pp. 265. \$24.95.)

Fiscal Harmonization in the European Communities: National Politics and International Cooperation. By Donald J. Puchala. (London: Frances Pinter, 1984. Pp. xii + 158. \$25.00.)

Professor Gregory's topic, the effects of European Community (EC) membership on the British system of government, covers an issue of particular importance for those who want to understand how the political systems of nation-states change when they join integration-oriented, supranational organizations. Unfortunately, the analysis includes some grammatical errors (p. 185) and is so poorly executed that it hinders the reader's ability to understand Gregory's point of view. His treatment of the topic from a structural and legal perspective focuses on constitutional issues (chapter 2); the further erosion of parliamentary power after entry into the EC in 1973 (chapter 3); the attempts of the executive branch (both civil servants and ministers) to cope with the EC as a major source of policy initiative (chapter 4, written by Freida Stack); the role British officials and politicians can play in EC institutions (chapter 5); and the impact of EC policies on Britain (chapter 6).

The author's potentially valuable contribution is marred by his failure to follow an adequate research design. Gregory's analysis of how Parliament has been affected by British entry into the EC illustrates this oversight. He begins with vague hypotheses regarding both the post-entry call for domestic electoral reform and the development of select committees to increase parliamentary control over EC policy. He then presents evidence which cannot test the hypotheses and must conclude that the results are still "open issues" (p. 120). Throughout the book, Gregory's remedy is to stray from the stated purpose of his research. Conclusions are replaced by prescriptions: the reader is told that ministers need to be good at bargaining in order to maximize British interests (p. 205), but is not told how British interests and ministerial behavior have been influenced by EC membership. Unsubstantiated views are aired: all governments since

1973 have adopted a negative and low-keyed attitude to EC membership (p. 2). Irrelevant themes are introduced: Gregory discusses the "mechanics of a possible withdrawal" in the concluding chapter (pp. 206-208). Furthermore, the analysis is based on sources so readily available to the public that the only utility of the book is in the author's having compiled material which others will be forced to analyze.

In sharp contrast to Gregory's work, Donald Puchala's study of the European Community's Value Added Tax (VAT) is skillfully executed because the author does adhere to an appropriate research design. Puchala presents a nonquantitative, empirical study of how the linkage between domestic politics and foreign policy affects the ability of governments to cooperate with one another internationally. He uses primary documents and open-ended interviews with participants in policy making to sketch the efforts of EC member-states and Community authorities in Brussels to harmonize turnover taxes across member-countries between 1958 and 1979. One of the more worthwhile aspects of Professor Puchala's analysis is that he has accomplished the limited purpose for which he wrote the book. According to the author, the study does not make a "systematic theoretical contribution" (p. 10). Its significance lies, instead, in his effort to give empirical meaning to a limited number of analytical concepts: politicization, political will, political capacity, and political reverberation.

In the first chapter Puchala illustrates, through an abstract discussion of the politics of European integration, the Lasswellian assumptions about politics that guided his research and that are embodied in the four concepts around which he organizes the data. The author suggests that this conceptual framework reveals a policy process which is actually a source of strength in the promotion of cooperation and integration rather than, as is generally assumed, a factor which retards unification. The remainder of the chapter clearly summarizes the objectives and themes of the book. Chapter two takes up the notion of fiscal harmonization (Puchala actually delves only into tax policies), including both a broad discussion of how it has contributed to economic integration and a brief historical and technical account of the VAT. Chapters three

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through six provide the empirical substance of the book. Case studies of the West German, Dutch, Italian and British experiences with the VAT during a 21 year period are succinctly and lucidly presented. The concluding chapter is an attempt by the author to summarize what lessons can be learned about international cooperation for the future of European Integration and, more importantly in the author's mind, for relations among western democracies.

Despite the high caliber of Puchala's contribution to the study of integration and cooperation, readers should be aware that his work presumes the utility of both state-centrism and liberal theory in studying the politics of western democracies. The author's acceptance of the state-centric approach means that his analysis of domestic influence on foreign policy eliminates possible linkages between the private sector and government which other approaches consider. Puchala's willingness to endorse liberal theory correlates with his acceptance of the state-centric paradigm. His praise of Western democracy for being "the most humanistically benign form of government ever devised" (p. 145) and his desire to protect and preserve it denote an uncritical acceptance of liberal theory which belies Puchala's claim that he did not intend to make a theoretical contribution to the field. There are other drawbacks in the book. First, the author does not provide enough evidence to justify conceptually distinguishing political will from political capacity. The British reaction to the Sixth VAT Directive is the most pertinent case to use in making this distinction. Yet Puchala's analysis (pp. 121-26) fails to establish when British opposition to the Directive was illustrative of political incapacity, and when it was a consequence of failed will. Puchala also does not relate his findings to the growing body of literature on domestic politics and foreign policy of which his study is a part. This failure is even more critical since his position on the circumstances under which domestic politics takes precedence over international consensus (p. 146) appears to contradict the conclusions of others whose work he cites. Finally, Puchala's attempt to extrapolate from the European case to all Western democracies (pp. 143-52) falls far short because differences in the political-economic processes of these nations preclude

generalizing from the European case. These limits do not, however, detract from what Puchala has accomplished: a solid piece of empirical work which advances our understanding of international cooperation and is so clearly and provocatively written that it can and should be read by both students and scholars in the field.

M. MARK AMEN

University of South Florida

The Dividing Discipline: Hegemony and Diversity in International Theory. By K. J. Holsti. (Boston: Allen & Unwin, Inc., 1985. Pp. x + 165. \$22.50.)

This essay, written by one of international relations' leading contemporary scholars, is a spirited and seductive defense of the centrality and viability of the *classical paradigm* in international theory. Starting from the indisputable premise that international theory "is in a state of disarray" (p. 1), Holsti asks whether this paradigm continues to dominate research and teaching in the field and, secondarily, whether a genuine global community of scholars has yet emerged in the discipline. He answers his principal question with an emphatic affirmative. The classical approach continues to provide "a core for both descriptive and theoretical efforts" which "can incorporate some of the insights emerging from the challenging paradigms" (p. 144). As regards the second, Holsti concludes that Anglo-American scholarship continues to exercise hegemony in the discipline, and that the parochialism among Western scholars is even greater than in years past.

For Holsti, the classical paradigm—the inheritance of somewhat disparate traditions of Hobbes, Rousseau, Kant, and Grotius—is characterized by a focus on war and peace, sovereign states as actors, and an anarchic global system. A review of the literature, he suggests, reveals that, despite extensive criticism of this paradigm, most contemporary theory remains within this tradition. There has been a good deal of "meddling" but only two genuine alternatives occur—a "Global Society" and a "Neo-Marxist" paradigm. The first, he asserts, greatly extends the agenda of scholarly concerns, de-emphasizes nation-

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states, and emphasizes global interdependence; the second is relatively unconcerned with the problem of war and focuses upon class-based actors. Neither of these Holsti sees as posing a significant challenge to the dominant paradigm.

In fact, Holsti has stacked the deck. His definition of the classical paradigm is so vague as to accommodate virtually any body of research that encompasses nation-states or their governments and/or that views war as a continuing problem of mankind. This allows him to dismiss much of the international relations scholarship that emerged from the ferment of the 1970s as "marginal additions or deletions" (p. 11). Yet in order to serve as a paradigm, the cluster of assumptions that scholars make about the world they are studying must be sufficiently precise to guide research and permit cumulation of knowledge. Realism, as stipulated by scholars like Morgenthau, may have performed this function in the postwar years, but it was a highly codified version of what Holsti terms the classical paradigm; in any event, its grip on the discipline weakened significantly in the 1970s.

Holsti is certainly correct in arguing that the "behavioral revolution" did not itself entail a paradigm shift; all too often concern with theory was sacrificed to methodological preoccupations during this revolution. He is also correct in asserting that the meddlers do not provide an alternative paradigm. Nevertheless, it requires a suspension of disbelief to accept his claim that scholars in areas as diverse as crisis decision making, integration, issues, regimes, and bureaucratic politics (to name a few) are merely laboring in the same old vineyards even though they are unaware of this. They may agree that war, government authorities, and the absence of system-wide authority are important factors, but the levels of analysis at which they work, the variables they consider, and the agenda of problems that preoccupy them are vastly different and more complex than those of their predecessors. To the degree that there was a classical paradigm, it implied a parsimonious model of a single set of dominant actors (black-boxed states) whose behavior was determined by system-level characteristics, notably the distribution of power. It assumed, moreover, that these states acted rationally in response to shifts in the distribution of power (national interest). In

contrast, contemporary scholars have thrown open the black box, and are seeking the causes of war in the psychology of leaders and groups, societal characteristics, and so forth. In many cases, they are preoccupied by political violence *sui generis* (not merely interstate war), and their image of the "state" has only a nominal relationship with that of their classical predecessors. To suggest that they remain in the classical tradition because of their interest in war would be akin to claiming that shamen and modern physicians follow the same paradigm because of their mutual interest in disease.

To be sure, there is no definitive answer to just *how* similar scholars must be in order to be regarded as paradigm brothers, but Holsti's argument masks the degree to which the field has already flown "apart into ghettos of specialization" (p. 144). His cross-national examination of textbook references to reinforce his claims regarding theoretical continuity and the absence of a global community of scholars is unsatisfying for several reasons. Textbooks inevitably lag behind intellectual currents and must be written in order to appeal to a generation of teachers reared in the older tradition. In addition, Holsti neither clarifies the criteria for his sample nor does he weight the references in his texts.

Although his advocacy of the classical tradition is unpersuasive, the essay is an important one because Holsti performs the critical task of pointing up the centrality of norms and values in theory construction. This is a major contribution and suggests that the debate about which he writes is less over competing paradigms in the Kuhnian sense than it is over competing norms and their impact on the way we view the world around us.

RICHARD W. MANSBACH

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Superpower: Comparing American and Soviet Foreign Policy. By Christer Jonsson. (New York and London: St. Martin's Press, Inc., 1984. Pp. vii + 248. \$25.00.)

There has long been a growing cottage industry among specialists and generalists alike, to compare various aspects of power and

performance between the United States and the Soviet Union (or, in Tocqueville's case, Czarist Russia). The annual *Military Balance* of the International Institute of Strategic Studies and the various studies of the Stockholm Peace Research Institute are two of many sources of comparison that are read with great care and interest.

Thus *Superpower* cannot be considered as breaking new ground, or even providing startling new insights about old ground. What it purports to do is to discuss elements of *condominium* in the two superpowers' behavior toward each other, drawing upon three cases: aid relations with the Third World; crisis management in the Middle East; and nuclear nonproliferation. The emphasis is on foreign policy behavior, although obviously the definition must of necessity be broad rather than restrictive in order for the author to make useful analytic conclusions.

Jonsson's main thesis is that the superpowers engage in mutually reactive policies, with the Soviet Union being more reactive to the United States than the other way around; although the United States also engages in "mirror-imaging" in its foreign-policy behavior. Where this is least the case is in aid relations; where this is most the case is in nuclear nonproliferation; where the issue is more muddled is in the Middle East. I doubt that most persons informed in these matters would disagree.

The author makes a distinctive point by suggesting that the superpowers' emulative behavior is primarily due to their perceptions of themselves as superpowers, and their strong desire to preserve that perception primarily in the eyes of the other—and then for the benefit of the rest of the world. Jonsson goes on to observe:

The USSR—the junior superpower—has been more interested in condominium-like arrangements than the United States which has generally preferred unilateral action. While both superpowers may be likely to resort to condominium-like arrangements to avoid direct confrontation in a crisis, this is seen by the United States as a provisional device, by the Soviet Union as a step toward a more permanent structure. (p. 223)

The interaction of the superpowers in various combinations of cooperation/conflict is well known and understood. The desire of the Soviet Union to be treated as equal in this global co-existence is also well known. The

frustrations both superpowers have encountered as they have attempted to translate their varying forms and degrees of national power into unequivocal national advantages (preferably but not exclusively at the other's expense) is only too evident. What has not emerged, as Jonsson points out, is any reincarnation of what was viewed once, referring to the nineteenth century, as a "club" of powers, not only in *realpolitik* terms more equal than the other players of international politics, but also in the sense of these powers being acknowledged, as by "right," to be the big players in the game.

In other words, the contemporary international system has not brought any conferment of legitimacy to the two superpowers' desires to be considered not only more equal than everyone else, but more equal by right as well as by might. In fact, as time goes by, neither superpower can claim any reinforcement of their ideological claims for this acknowledgement, which leaves both of them searching for ways to assert some sort of normative justification for what otherwise appears increasingly to be simply good old-fashioned national pride, ambition, perhaps even hubris, and certainly opportunism. Jonsson observes: "Although their influence may be declining, the superpowers are not likely to drastically alter their global role. Each forces the other to maintain its 'superpower role'" (p. 225).

In conclusion, it is not likely that the notion of condominium will ever have much appeal to either of the superpowers over the long haul, or to any of the other 150-odd states composing the community of nations that exists today. This is because calculations of national power, and the linkage of such calculations to prospects of national advantage, are simply too complex. A glance at the dilemma facing the United States in trying to maintain a coherent and credible policy toward South Africa, and the dilemma facing the Soviet Union in trying to maintain a coherent and credible policy toward Poland are but two examples. Whether "friends and allies," client states, or vassals, other national units simply are not willing to acknowledge total preeminence for either superpower.

This book makes good reading, laced as it is with relevant quotes and clear historical examples. Its length is also attractive and makes it useful as part of a graduate course syllabus

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as well as general background reading for the scholar and practitioner of foreign affairs.

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World Trade Issues: Regime, Structure, and Policy. By Young Whan Kihl and James M. Lutz. (New York: Praeger Publishers, 1985. Pp. xvi + 274. \$25.95.)

Kihl and Lutz explain in their introduction that they do not attempt a comprehensive discussion of world trade, but rather seek, "to specify certain interaction patterns in the world trading system, their important consequences, and the political and economic policy responses made by the various participating countries in world trade" (p. 2). The authors also admit that their work may raise more questions than it answers, and it certainly does.

Following the introductory chapter are 11 chapters organized in four parts. "Part I: Theory and Politics of Reform" discusses, in chapter 2, what is meant by regime and five stereotypes of world trade. The authors indicate their attachments to free trade liberalism but never clarify that "real world" policies are more complex and mixed than their five stereotypes. The third chapter outlines the content and last 15 years' debates on the New International Economic Order (NIEO). Kihl and Lutz fail to integrate these two chapters, add nothing new to the literature, and ignore North-South politics so ably described elsewhere, as in Robert I. Rothstein's *Global Bargaining: UNCTAD and the Quest for a New International Economic Order*, (Princeton: Princeton University Press, 1979). Since both chapters are ahistorical and incongruous, one questions the purpose they serve.

The four chapters of "Part II: Changing Commodity Structures" do not elucidate the connections among chapters. Chapters 4 and 5 discuss exports of manufactures from developed and newly-independent countries, respectively. While the authors provide interesting statistical analysis of changes in export volume for groups of manufactures, they use 1968-78 data. Instead of tracing interacting trade patterns, Kihl and Lutz use exports to

represent all trade (Wouldn't that help the U.S. deficit?) and country pairs, without sufficient discussion of interacting patterns. This omission becomes particularly burdensome in the third part of the book, in which the authors argue for regionalism in trade without reference to the overwhelming share of world trade occurring among Organization of Economic Cooperation and Development (OECD) states. Chapter 5 repeats the illusion that reference to a stereotype, in this case NICs, is an adequate discussion of policies. Why, then, is Mexico's trade position so different from that of Singapore? Chapter 6 on primary commodity exports and chapter 7 on world food exports continue the use of categories and descriptive quantitative information on exports without either asking or answering why, how, and who benefits?

"Part III: Regional Patterns and Trends" is discontinuous with Part II. The authors seem to have divided the world geographically without recalling their documentation of North-North trade in manufactures and without mention of contemporary economic manifestations of colonialism. Thus, chapter 8 covers the U.S. and the western hemisphere; chapter 9, Western Europe and Africa; chapter 10, Japan and the Asia-Pacific Basin; and chapter 11, the USSR and "centrally planned economies." The data base for Part III does not provide support for the conclusions reached. The authors rationalize using noncomparable time periods, 1961-1971 and 1971-1976, on the basis of data availability; yet, they rely on only two sources. The data base is particularly problematic in the tenth chapter, in which Kihl and Lutz want to argue the emergence of Japan in the Pacific Basin. While Japan's role in the last 10 years is obvious, that is not the time period the authors use, in spite of the publication of the book in 1985. Chapter 11 is a puzzle, also. The authors cite bilateralism and nonconvertible currencies as "two distinct trading practices" (p. 205) without acknowledging the Soviet role in the definition of the Bloc trade regime.

The authors promise that the conclusion, Part IV which is chapter 12, will highlight major points and speculate on the future of world trade. In spite of some interesting dyadic data and discussions of regionalism, the authors fail to present a convincing picture of world trade as it exists. The two middle parts

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of the book would make sense as separate works, each with analysis of contemporary data. As the book stands, it raises questions already and better answered elsewhere.

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The Quest for a Just World Order. By Samuel S. Kim. (Boulder, Colo.: Westview Press, 1984. Pp. xix + 440. \$40.00, cloth; \$16.50, paper.)

The Quest for a Just World Order was written under the auspices of the Center of International Studies at Princeton University as part of an on-going effort of the World Order Models Project (WOMP). It is a powerful statement in what is becoming a political literary genre. Professor Kim sets forth an ambitious scheme. Not only does he call for world "order" but this state is also to be "just." Like other works in the WOMP series, a normative path is followed on a "global" scale as opposed to an international or even a world perspective. Kim spurns the value-free world in which the traditional social scientist conducts research. On the contrary, Kim argues that if the scientist is task oriented, then values are necessary in order to render solutions applicable to those global problems under observation. In this way, values become the essential basis of systemic transformation. The value-laden observation is further connected to an assumption of the existence of "human nature" which may be then modified to better the human condition and ultimately add to the development of a world order.

The general purpose of the author's proposal is aimed at "generating the political consciousness and mobilization required to challenge and transform an unjust social order rooted in violence" (p. 335). The book is divided into three neat parts. Part one provides an examination of the current state of international relations theory focusing on methodology. The particular focus is on a normative theory of world order. Part two looks at four specific problems affecting the globe. Part three contains Professor Kim's prescription for a more humane world order system that he believes is feasible.

The social organization of the world, Kim

holds, has undergone systemic change over time. Man, as a social being, first focused on small groups ultimately developing to the currently acceptable order in which interdependence exists on a global scale. Since human social organization has essentially expanded to the global limits, world-order theory is not set for the benefit of any one sub-global group. Hence, on one level the need for change is evident by the pervasiveness of global problems. However, the policies enacted by many governments tend to perpetuate many of these same problems. For example, weapons development continues to escalate. There is, moreover, a growing economic disparity between the industrialized and non-industrialized sectors of the world. Also discussed is the basic threat to the world's ecological balance that recognizes no national boundary. There are, Kim argues, alternative means of achieving world order and stability.

As might be expected, the alternative world order is a cooperative venture. Together all are encouraged "to shift the course of the human journey in a more humane and just direction" (p. 308). It is hoped that as the number of individuals and groups who realize the precariousness of the global condition grow, an even larger coalition will develop to re-create the world.

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La politique étrangère. By Marcel Merle. (Paris: Presses Universitaires de France, 1984. Pp. 218. FF130.)

Since Talleyrand and the Congress of Vienna, French diplomacy has acquired a reputation of being able to compensate for many internal weaknesses of the French state and society by the skill and high motivation of its diplomatic personnel, and the talent of its ministerial leaders. In France, and even more so abroad, this valuable piece of make-believe has produced an image of a stable French foreign policy detached from the various vagaries of French domestic problems.

How is it now under the conditions of global interdependence, not only in France, but all other countries? Many interesting answers are offered by a French scholar, Marcel Merle, in

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his most recent book entitled *Foreign Policy*. In it he sees foreign policy as only one of several segments of international relations (diagram on p. 169) that represent more than a sum total of various nations' foreign policies. The author is professor of international politics at the Sorbonne; he is also author of *Sociologie des relations internationales*, 3rd ed. (Paris: Dalloz, 1982), today the most widely used text in France; it is now also available in English.

Merle's new study is not a comparison of the contents of various national foreign policies, but a focus on, and analysis of the concept of foreign policy and the state as such. Although Merle finds the principal and traditional justification of nation state still in the duality—and difference—between foreign and domestic policy, he naturally finds that, under the conditions of interdependence, "the dikes which used to be state frontiers have been fissured and that the locks—separating the various water levels and formerly under the careful control by the diplomats—are today out of kilter" (p. 197). Under such conditions the very principle of the nation state is at stake.

Foreign policy which has expanded in terms of issues has, according to Merle, contracted in terms of its former specificity as achieved and refined in the eighteenth and nineteenth centuries. Despite the often confusing intermingling of domestic and foreign policy considerations and various non-state actors, Merle rejects the "zero-sum" concept of domestic v. foreign policy. These do not expand at the expense of each other; they intermingle. The result is not an utter confusion, but an interplay of various autonomies without the production of a homogenous bloc (which, in some cases, could indeed invite authoritarian remedies).

Merle notes (p. 93) the increasing difficulty of the central governments to know about, much less control, all the important trans-border activities initiated by noncentral governments and private actors. Even the Quai d'Orsay, now renamed by Mitterand "Ministry of External Relations," (instead of "Foreign Affairs"), once a model of tightly centralized diplomacy, had to create (May 23, 1983, p. 93) an office to monitor and coordinate the "mini-diplomacies" initiated by various regions and cities of now partly decentralized France. The author also notes the con-

temporary microdiplomacy of Quebec and other Canadian provinces as well as that of some Italian regions (Sardinia and Friuli Venezia) and the Walloon and Flemish communities of Belgium. Surprisingly, Merle does not take note of the mushrooming foreign activities on the part of the U.S. states (by 1985, for example, 28 of the U.S. states have established 54 permanent foreign missions in 17 foreign countries).

While Merle's book is truly comprehensive and knowledgeable about contemporary American analytical concepts, this reviewer believes that his general analysis could be usefully illuminated by references to Nye/Keohane's world paradigm and complex interdependence studies as well as Krasner's and Haas' work on international regimes.

"Is then the nation state close to its death?" (p. 201) asks Marcel Merle, noting the blurred line between domestic and foreign policy. It is, of course, an old question which had been posed, often with impatience, by liberal economists, Marxists, Saint-Simonians, utopians, and romantics. "Not at all," is his answer. Nation states—a "necessary evil" (p. 206)—will keep on managing segmentally their now globalized economies ("economies mondalisées") to make international anarchy moderately tolerable. With sad irony Merle adds: "We can count indeed on the nation state's resistance to any true international integration" (p. 119)—which logical reasoning seems to recommend so convincingly. Quoting Spinoza (p. 200), Merle warns against the dreams of a golden age or other chimeras in politics in which men will base their behavior solely on logical reasoning.

Merle's new book is a wise book whose lessons, drawn from different historical and academic environments, will usefully contribute to the current American debate on the concept of the "State."

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The Age of Vulnerability: Threats to the Nuclear Stalemate. By Michael Nacht. (Washington, D.C.: The Brookings Institution, 1985. Pp. 209. \$26.95, cloth; \$9.95, paper.)

Usually books from the Brookings Institution can be relied upon to help us identify the shadows on the wall of the cave more readily and to see the order with which they pass more clearly. This book is no exception. It is well written, it contains some interesting information and its conclusions remain well within conventional wisdom.

Nacht identifies what he considers the central problems of the nuclear age which, because they are ultimately unresolvable, "provide the boundaries within which effective policies should be formed and implemented" (p. 7). These problems, which are both political and technical, include: Russian national character, American national character, the new high-accuracy weapons systems, American strategic doctrine, arms control, alliances, and nuclear proliferation. Each problem is considered in a separate chapter and several of them have appeared earlier in different form as articles in various journals.

The chapters on national character are interesting but subject to the same criticisms that have been relevant to this type of analysis at least since Montesquieu. While readers will not be surprised to discover that the Russians suffer from extreme insecurity, they might be puzzled to discover that Americans are essentially schizophrenic, desiring on the one hand armaments second to none and on the other a commitment to a world "permanently at peace." While the chapter on weapons development is clear and understandable to the layperson, the one on strategic policy seems to be particularly fuzzy. We learn for instance that the first American strategic doctrine was containment (p. 85), but since that policy did not discuss nuclear weapons it was not until massive retaliation in 1954 that strategic doctrine as we know it first emerged. One is tempted to ask whether containment and massive retaliation are even comparable. Did not we have both at the same time, and in fact, is not containment still a long term goal of American foreign policy?

The final chapter is most troubling because the conclusions Nacht draws offer very little

hope to those who believe that the continued development and deployment of nuclear weapons is not inevitable. When reading some of Nacht's observations one understands the feelings of Nietzsche's Zarathustra who is amazed at the saint who still talks about God, apparently not having yet heard that God has died. What is one to make of a sentence which suggests that nuclear weapons "are technologically sweet . . . and they protect Americans and their allies from communist (essentially Soviet) aggression" (p. 196)? Does anyone still seriously believe that? That is a sentence one might expect to hear from Robert Oppenheimer in the late 1940s. Do nuclear weapons protect America from anything? Are nuclear weapons really weapons in any meaningful sense of the word or are they simply enormous explosions that can be linked to no rational purpose at all?

The book ends on a confusing note. The subtitle and much of the argument lead the reader to believe that some of the modern developments discussed may indeed be threats to the nuclear stalemate. After all the book looks at some of the things that it does in order to evaluate the extent to which changes were threats to the status quo. However the concluding sentence says: "It will take truly revolutionary technological innovation or a massive exercise of human stupidity before this stalemate is seriously threatened" (p. 201). If that is true, then what is the purpose of the book? If indeed there are no serious threats to the stalemate on the horizon, what accounts for the book's subtitle and the choice of subjects examined? The concluding sentence suggests that there is nothing for us to worry about, and that we can go back to sleep, provided only that neither major technological innovations nor massive stupidity occurs. In either case there is little for the intelligent layperson to do.

In short, the book seems to be an invitation to the maintenance of the status quo under the assumption that deterrence has worked in the past, will continue to work in the future, and no matter how many nuclear weapons we assemble there is nothing to fear because the stalemate is intact and nothing will go wrong. Where does this kind of naive optimism come from? Are not we long past the stage where we can leave these issues to the experts and return to our reveries? Is not the existence of enor-

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mous vertical proliferation by the two major powers the dominant fact of our day about which something must be done, even by average people, before it is too late? Considering what we now know about the fate of the earth, is it any longer enough to be reassured that the enormous arsenals on both sides "will *probably* [italics mine] prevent nuclear doom" (p. 197)? Reading this book will offer no hope out of the miasma of business as usual that characterizes so much of conventional, "sensible" thinking on this subject. If this kind of work is well-balanced and even-handed, perhaps the time has come for someone who is not well balanced to point out that the condition we are living in is absolutely insane.

PHILIP W. DYER

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Nuclear Forces in Europe: Enduring Dilemmas, Present Prospects. By Leon V. Sigal. (Washington, D.C.: The Brookings Institution, 1984. Pp. x + 181. \$22.95, cloth; \$8.95, paper.)

Nuclear Weapons and European Security. Edited by Robert Nurick. (New York: St. Martin's Press, 1984. Pp. ix + 142. \$22.50.)

During the last decades, as the Soviet Union developed nuclear weapons capable of inflicting unacceptable damage on the United States, the American commitment to initiate nuclear war on behalf of its West European allies became increasingly incredible. This weakening of the principle of "extended deterrence," and the corresponding ambiguities in NATO's strategic doctrine of "flexible response," have led to divisive debates among NATO allies within their respective electorates and, in some cases, within their governments. Moreover, the close connection between security issues and fundamental political questions—about the nature of the Soviet threat, the future of the Western alliance, and the shape of a desirable European political and economic order—has led all parties to use the technical language of arms and arms control to express what are, at bottom, political intentions and political anxieties.

This meshing of military-strategic and political purposes was at the core of NATO's so-called "double-track" decision, the subject of Professor Sigal's book. In December, 1979, following the deployment of an increasing number of intermediate-range Soviet nuclear missiles within range of Western Europe, NATO ministers decided—this was the first track of their double-track decision—to deploy new American nuclear-armed cruise and ballistic missiles capable of striking deeply and accurately into the Soviet Union from bases in Great Britain, West Germany, Italy, Belgium, and the Netherlands. The second track of the decision was to seek arms control negotiations with the Soviet Union which would, if successful, make the first track unnecessary or reversible. The failure of the second track led to the partial implementation of the first track, leaving in its wake inflamed U.S.-Soviet relations, tensions within the alliance, and a divided public opinion in many West European countries.

In terms of extracting analytical meaning and political lessons, Professor Sigal makes the most of this apparently prototypical case of alliance mismanagement. But what makes this book eminently successful and lends plausibility to the author's political judgments is that he treats the double-track decision not as a narrowly focused case study, but places it in a much broader political, conceptual, psychological, and chronological context. Professor Sigal begins with a discussion of the paradoxes in the logic of deterrence, and the contradictions between political and strategic logic and stability, coining and applying the term "nuclear inter-dependence" as an alternative to the term "strategic parity," which he considers misleading since its connotation of numerical equality is not essential for mutual vulnerability. This leads to a discussion of NATO's public, as well as unstated rationales for augmenting its euromissile arsenal, and of the technical characteristics of ground-launched cruise missiles and Pershing II missiles which Professor Sigal regards (in my view correctly) as fitting these rationales only loosely. The core of the book, Chapter 4, examines the political dimensions of the euromissile issue. Here, as in other chapters, Professor Sigal insistently demonstrates that beyond the numbers game of calibrating the military balance on a variety of levels and rungs on the

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ladder of escalation, there is a central political purpose that the contending parties often leave unarticulated and in some cases even unexamined. The book concludes with a tentative and necessarily inconclusive discussion of the problems and prospects of arms control negotiations and their implications for East-West relations and the coherence of the Western alliance. Professor Sigal has given us an excellent account of the euromissile issue, and one hopes that he will turn, in the future, to a similar analysis of the Reagan administration's Strategic Defense Initiative—an issue that is even more divisive and will concern us for years to come.

The volume edited by Robert Nurick consists of chapters on the future of Britain's deterrence force (by Peter Nailor and Jonathan Alford); nuclear weapons in Europe (by Gregory Traverton); extended deterrence and U.S. strategic forces (by A. H. Cordesman); a call for "inflexible response" on the part of NATO (by Francois de Rose); and TNF modernization (by Christopher Makins). These are good, but dated and rather technical discussions for the think-tank initiates who presumably read these essays when they first appeared in various journals several years back.

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The United States and Israel: Influence in the Special Relationship. By Bernard Reich. (New York: Praeger Publishers, 1984. Pp. xiii + 236. \$29.95, cloth; \$13.95, paper.)

The relationship between Israel and the U.S. is particularly complex and marked by contradictory objectives and perspectives. On the one hand, every U.S. President since Truman has affirmed a commitment and moral obligation to the independence and survival of Israel. Simultaneously, however, the U.S. has sought to minimize Soviet influence among the Arab states, many of which have had difficulty accepting Israel's presence. As a result, U.S.

policy vis-a-vis Israel has often seemed to be inconsistent and unpredictable.

Largely descriptive, this volume provides a thorough review of U.S.-Israeli relations during the Carter and Reagan administrations. The negotiations between the U.S. and Israel which, no less than those between Egypt and Israel, culminated in the Camp David Accords, are chronicled in detail. In addition, the efforts of the Reagan administration in this area, including the promulgation of the Reagan Plan and sponsorship of the abortive Israeli-Lebanese negotiations in 1983, are traced.

This volume is included in a series entitled, "Studies of Influence in International Relations," and this series provides the analytic framework for the study. Influence is defined as the ability of one state to bring about changes in the policies or actions of the other. This is usually brought about by a combination of threats and inducements and is asymmetrically dominated by the stronger state. In this case, however, what appears to be almost complete Israeli dependence on the U.S. has often not enabled the "patron" to manipulate or control the "client." Occasional differences with regard to policy have led to U.S. arms embargoes and policy reassessments, but such actions have not produced desired changes in Israeli policies. "Israel . . . took action . . . where it believed its national security interest was at stake, even when it understood this would lead to clashes with the United States. The U.S. could react, but could not alter the Israeli decisions. On less central issues . . . Israel deferred to U.S. entreaties" (p. 138). For example, various U.S. embargoes on weapons sales and deliveries had little impact on Israeli policy, but did contribute to the Israeli effort to develop an indigenous arms industry. Similarly, pressure from Washington during the Carter period "had a counterproductive effect as support for Begin grew at home and abroad, partly in response to this perceived strategy" (p. 49).

Only when this strategy was changed, and inducements were substituted for threats, was Carter able to convince Israel to take the enormous risks necessary for the successful conclusion of the Camp David negotiations. In general, over the past three decades, the U.S. has been most successful in influencing Israel "through entreaties and reassurances." Israel,

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in turn, has been able to influence U.S. policies not through bluster and angry defiance, but by "persuasion and argument."

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In Search of Namibian Independence: The Limitations of the United Nations. By Geisa Maria Rocha. (Boulder, Colo.: Westview Press, 1984. Pp. xi + 192. \$16.95, paper.)

In terms of longevity, if not of complexity, the issue of the international status of Namibia—called South West Africa by the United Nations until 1968—has been on the agenda of the United Nations longer than any other single issue. As of 1981, consideration of its status generated 130 resolutions, 18 of which were passed by the United Nations Security Council and the remaining 112 by the General Assembly. The political, legal, and now military battle for its independence from South Africa has continued unabated for 40 years. This territory has attracted substantial international attention and has provided ample opportunity for social scientists, journalists, international legal experts, military strategists, humanitarians, and political activists of most every stripe to exercise their talents. But to what avail?

Geisa M. Rocha, now a doctoral student in political science at the Graduate School at the City University of New York, had six years experience as a civil servant working with the late Marcio Rego-Monteiro, the Secretary of the United Nations Council for Namibia. This U.N. background is reflected not so much in explicitly personal judgments as in the brevity and crispness of her treatment of the main elements of the issue. The Namibian affair, because of its multiplex nature, can become a data swamp for the unwary. But not for her.

As the subtitle of her book suggests, her inquiry deals with the inability of the United Nations organization to effect the independence of Namibia. Thus she has written a case study

of United Nations behavior. In doing so, she has provided cold comfort to those who entertain great, and unwarranted, expectations about how effective this international body can be in terms of conflict management. In the introductory chapter, she furnishes a thumbnail sketch of the origins and early practices of the United Nations with particular reference to conflicts which engage the attention and material interests of one or more of the great powers. She is not especially sanguine about U.N. resolution of conflicts that involve the vested global interests of one or more major powers.

She contends that the United Nations is not an international actor and that Western pressure upon South Africa to relinquish the territory, rather than U.N. action, is the way to achieve Namibian independence. Although she does not assess the utility of the guerrilla war there in terms of displacing the South African regime, presumably she would find it less effective than Western coercion. Yet most of her criticism is directed at non-Western states. Like other scholars, she contends that the U.N. should not have transformed an essentially political into a legal issue by frequently consulting the World Court. In addition, she avers that the General Assembly majority lost touch with reality and added nothing by passing impractical resolution after resolution, thus lowering its credibility. She sees the utility of including both the Angolan MPLA and UNITA in an independence package and comments adversely on the spendthrift nature of the "missions of consultation" undertaken by the U.N. Council for Namibia.

What is especially intriguing are her views on the spillover effects of the "loss" of Namibia on the white South African power structure. Had she chosen to do a parallel analysis of the last few years of the international politics of Zimbabwean independence, it would have added a great deal to the breadth of her study on what the U.N. is ill-equipped to do, especially in violent decolonization. The publisher would have been well advised to have required the author to have included maps and a chronology for the novice reader as well as to have proofread it more carefully. The select bibliography had no pagination for journal articles and was marred by typographical errors. Nevertheless, this is a useful contribution that challenges conventional

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wisdom, political inertia, and bureaucratic boondoggling.

RICHARD DALE

*Southern Illinois University
Carbondale*

Anti-Americanism in the Third World: Implications for U.S. Foreign Policy. Edited by Alvin Z. Rubinstein and Donald E. Smith. (New York: Praeger Publishers, 1985. Pp. xii + 267. \$37.95.)

The essential myth of American foreign policy has been that the United States is a force for good in world affairs. This self-perception has sometimes made it difficult for Americans who expect gratitude but often receive hostility to appreciate the sources and implications of anti-Americanism. Popular explanations have pointed either to Communist influence or American malevolence and stupidity as the cause of anti-Americanism.

As a first attempt to subject anti-Americanism to scholarly investigation, this volume establishes a new level of realistic discourse on the subject. The first of 12 essays originally prepared for a March, 1984 conference is the editors' suggested approach to the subject. The next eight chapters examine anti-Americanism in specific countries and regions. The final three essays address "functional" questions: What is the relationship between multinational corporations and anti-Americanism? Does a particularly troubling type of anti-Americanism prevail within the United Nations Organization? What are the broad implications of anti-Americanism for U.S. foreign policy? Most, but unfortunately not all, of the individual studies clearly follow the editors' suggested approach.

Two major themes inform their approach. The first consists of aspects of Third World societies—ethnicity, elite values, and political systems. The second is disagreement between the U.S. and Third World states about general orientations toward world affairs and specific issues. Both imply that anti-Americanism is a highly variable concept that depends on the particular country in question and its relations with the U.S., e.g. the attitudes of Latin American intellectual elites are quite different

from those of radical Moslems. Therefore, the most satisfying contributions are those with the most specific focus, such as George Grayson's essay on Mexico, Robert C. Horn's on Malaysia, and Ainslee Embree's on South Asia. Broader selections such as those on Latin America, Africa, and the Afro-Arab countries, while interesting and instructive, carry fewer useful implications for policy.

Despite the difficulty of generalizing about anti-Americanism throughout the Third World, several interesting points do emerge from this collection. First, as described in Ahmet Evin's chapter on Turkey and Harvey Glickman's discussion on Africa, the United States has contributed to its tarnished image by failing to meet the expectations raised by American can-do rhetoric, for example in the area of economic assistance. The Soviet Union may suffer less criticism because it has avoided raising such expectations.

Secondly, anti-Americanism is often found concentrated within elites and is frequently based on areas of disagreement or conflicts of interest or on the "Ugly American" attitudes and behavior of diplomats and others. Thirdly, anti-Americanism has great instrumental value. Governments often find that the United States is a useful scapegoat to absorb the blame for their economic, political, diplomatic, or military failures.

Anti-Americanism can play a useful role for a nation trying to define its identity and place in the international order. The image of a secular, consumerist America with a foreign policy of global intervention is much like the image of decadent, war-ridden, monarchical Europe that helped the young American republic to confirm its identity and its determination to become what Europe was not. It is also a useful unifying theme. It can provide a common frame of reference for discussion among intellectuals, and it can supply the basis for a minimum level of consensus among the members of the non-aligned movement.

Finally, in discussing the implications of anti-Americanism for U.S. foreign policy, Richard E. Bissell argues that, although anti-Americanism cannot be eliminated, reasonable improvements in the quality of American diplomacy and foreign policy could dampen the intensity of anti-American opinion and reinforce the reservoir of pro-American opinion. Therefore, policy makers should be

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more aware of the impact of U.S. policy on foreign elite and mass opinion.

The editors correctly call this volume a first cut. Several of the authors confess that their contributions are impressionistic reflections on anti-Americanism in various parts of the Third World. What is needed now is for analysts to make even more careful and rigorous investigations of anti-Americanism in specific countries.

BRIAN E. KLUNK

Union College

Foreign Policy Decision Making: Perception, Cognition, and Artificial Intelligence. Edited by Donald A. Sylvan and Steve Chan. (New York: Praeger Publishers, 1984. Pp. xi + 347. \$28.95.)

This book is the first volume in an annual series, "New Dimensions in International Studies," which will attempt to document the evolution of the field by publishing collections of writings composed largely of papers presented at the annual conventions of the International Studies Association (Sylvan and Chan were co-chairs of the 1983 ISA convention). Describing in a coherent way the development of a disjointed, multifaceted, and relatively non-cumulative field like international relations would be difficult enough under most conditions, but to try to do so with the rough and tentative work represented in most convention papers seems next to impossible.

The volume does not adequately cover the central dimensions of its topic, foreign policy decision making. Even accepting the focus on recent writings with a psychological orientation, would the three main categories be "Perception and Personality," "Cognition and Learning," and "Artificial Intelligence" (the three parts of the book)? The co-editors provide a nice overview of the field in the first chapter, but it does not logically lead to the book's organization; indeed one of the four key substantive elements in Sylvan and Chan's foreign policy decision-making matrix (p. 4)—management—is overlooked. Crucial decision-making distortions in foreign policy—such as groupthink, disjointed incrementalism, and satisficing—similarly receive little treatment,

and the crisis decision-making literature is barely mentioned.

As with many edited collections, the chapters are of uneven quality. Several provide innovative and well-grounded insights on narrow slices of foreign policy decision making. For example, Richard Herrmann presents a convincing case as to why there should be more attention paid to perceived opportunity (rather than perceived threat) as a foreign policy motivation. Margaret Hermann takes an intriguing look at how leaders' sensitivity to their environment and interest in foreign affairs affect their states' foreign-policy behavior. Philip Tetlock compares American senators and British parliamentarians to show that extreme ideologues possess lower belief complexity than do moderates. And, by exploring the relationship between public beliefs and foreign policy behavior during the Carter era, Jerel Rosati finds that the correlation is highest when consensus prevails among the top foreign policy makers. But Susan Kiesel's chapter on voice stress analysis is too preliminary and descriptive, and John Lovell's chapter on bureaucratic learning contains poorly formulated hypotheses. Furthermore, the entire section on artificial intelligence is weak. Two of its chapters, by James Bennett and Akihiro Tanaka, seem to discuss complex computer models rather than artificial intelligence, which unlike simulation requires that the computer itself (rather than human programmers) generate and "learn" rules of behavior through trial and error.

The isolated analytical strands within this collection cry out for integration and synthesis both with each other and with others in the foreign policy-making literature. Implied controversies run rampant throughout this volume. Can foreign policy wisdom be learned? Do leaders' beliefs have a greater impact on foreign policy behavior than this behavior has on these beliefs? Although any collection on foreign policy decision making might raise more questions than it answers, these controversies need more open-minded identification and examination. Despite its flaws, this book has accomplished perhaps its greatest service by opening up more cans of worms.

ROBERT MANDEL

Lewis and Clark College

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Cloning and the Constitution

AN INQUIRY INTO GOVERNMENT POLICYMAKING
AND GENETIC EXPERIMENTATION

Ira H. Carmen

Recombinant DNA, one of the most significant scientific breakthroughs of recent years, has raised important social and constitutional questions. This first in-depth examination of genetic engineering from the point of view of social science breaks new ground in political science and Constitutional law studies and will be of lively interest to political scientists, attorneys, judges, ethicists, decisionmakers in government, administrative officers of universities, biological scientists, and concerned readers generally.

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Among the features of the book are a discussion of science and the First Amendment, a summary and analysis of the debate involving recombinant DNA, and a consideration of how science, politics, and law influence that debate. For all who are concerned with constitutional aspects of genetic engineering, *Cloning and the Constitution* will come as an enlightening and thought-provoking contribution to one of the most important issues of modern science and society.

Ira H. Carmen, a member of the Political Science department of the University of Illinois, is author of two books on constitutional subjects – *Movies, Censorship, and the Law* (University of Michigan Press, 1966) and *Power and Balance* (Harcourt Brace Jovanovich, 1978) – and numerous articles in law reviews and other professional journals.

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ARTICLES

MARKET JUSTICE, POLITICAL JUSTICE

ROBERT E. LANE

Yale University

The defense of capitalism in America is rooted in a preference for the market's justice of earned deserts over the justices of equality and need associated with the polity. These preferences have structural roots in the way governments and markets serve different values and purposes, satisfy wants, focus on fairness or justice, enlist causal attributions, distribute or redistribute income, are limited by rights, and seem to offer either harmony or conflict of interest. Some of these "structural" differences, however, are themselves perceptual, and corrected by changed perceptions of the productivity of government and of our historic predecessors, and by a community point of view involving changed accounting systems, as well as by policies of full employment rather than guaranteed incomes. With few institutional changes, these altered perceptions may partially restore political justice to favor.

"Without some very considerable surge of moral anger . . . [political and social] changes do not occur" (Moore, 1978, p. 459). To account for "moral anger" is to contribute to our understanding of popular turbulence—or, more likely, to our understanding of "the cankers of a calm world and a long peace."

Our concern is with the American sense of justice and injustice regarding the distribution of valued goods by markets and democratic politics—how they compare with each other, and what structural and ideological factors explain the apparent preference for market justice over political justice. Inasmuch as the following analysis makes certain assumptions on contested issues, we turn to these first. For our purposes a standard definition of the market will do: it is the device for coordinating an economy by prices largely determined by relatively free, competitive transactions among firms guided by anticipated profits (Lane, 1978a, p. 4; Lindblom, 1977; Weber, 1947, pp. 179–83). Similarly, democratic politics embraces

standard elements: competitive elections, a legitimate opposition, a relatively free (officially uncensored) press, and protection of minority rights. The relation between these two institutions offers more difficulty. While for certain purposes it is desirable to emphasize their close interrelation, for the purposes at hand it is desirable, and equally legitimate, to regard them as separable and alternative methods of coordinating production and distributing goods; neither is merely the disguised servant of the other. The public regularly makes this distinction: functions belong either to "business" or to "government," albeit with some confusion and overlap (Lipset and Schneider, 1983; McClosky and Zaller, 1985).

We assume that the perceptions and values we encounter and their institutional and material bases have reciprocal influences. Whereas the sociology of knowledge, like its Marxist origins, closely ties beliefs and values to their parent institutions and social locations (Mannheim, 1949), we follow a Parsonian (1951) and Weberian (1947) analysis, loosening

these ties. Thus, in the analysis of structural roots we show the influence of social structures on perceptions, while in discussing perceptual changes we rely on the independent effects of perceptions on the interpretation of structures. We return to this problem later.

Popular perceptions of justice are related to, but distinct from, philosophical conceptions of justice. They are related in the sense that most philosophical treatments regard people's sense of justice as relevant, but certain treatments—such as Ross's (1939, p. 2) reliance on "a large body of beliefs and convictions," or J. S. Mill's (1910, p. 49) use of "the social feeling," or Edmund Cahn's (1964, p. 25) reliance on the "pre-disposition (of) . . . the human animal . . . to fight injustice"—are misleading, for they fail to preserve sufficiently the distinction between philosophical justice and the public's sense of injustice. In rejecting that tradition we take the further step of assuming, like others (e.g., Deutsch, 1975; Harman, 1983; Kaufman, 1973; Sidorsky, 1983), that conceptions of justice are partially dependent upon other goals simultaneously pursued, and upon institutional settings. There is evidence that the public follows this course (Barrett-Howard and Tyler, forthcoming; Hochschild, 1981), denying the universal applicability of such formulations of justice as those recently put forward by Rawls (1971), Nozick (1974), and Ackerman (1980).

Our aim is not to clarify philosophical justice; we do attempt to illuminate concepts of the legitimacy of markets and politics, to contribute to an understanding of the forces of social change, and above all, to help explain the tenacious hold of market capitalism on the public mind. There is an irony in the public's justice-based defense of the market, for that is the very point where many commentators have found it weakest (e.g., Rawls, 1971). It is uncertain whether this defense repre-

sents a social lag, a "contradiction" between institutions and perceptions, or, as we shall argue and then modify, a structurally induced perception based on the functioning of the market and its political alternatives.

Finally, the analysis helps to explain a historical anomaly, as well as a current situation. In his study, *The Pursuit of Equality in American History*, J. R. Pole (1978, p. xi) puzzles over the focus on political equality and the lack of attention to "such questions as the redistribution of wealth or an effective re-examination of the criteria by which economic rewards are allocated." The puzzle is made greater by the increasing economic inequality over most of this period (Williamson and Lindert, 1980). Two points are in order: (1) American concepts of justice were never predominantly directed toward equality of condition (Tocqueville, 1945, vol. 1, p. 51, but see vol. 2, p. 94; Potter, 1954); hence, sentiments of justice took other forms; and (2) for reasons we shall explain, Americans tend to prefer market methods to political methods.

Origins and Elements of a Sense of Injustice

Certain perceptions and beliefs favor the translation of deprivation into an "injustice." The minimum condition seems to be that people want something they do not have, and believe they deserve it (Crosby, 1982, chs. 1-2; Runciman, 1966). It is the genius of the market to stimulate wants without at the same time stimulating a sense of deserving more than one gets. Comparing oneself or one's group with some better-off relevant person or group, especially a member of an "in-group," makes deserving more salient, but in the market comparisons of this kind encourage effort and not an acute sense of injustice. Expectations are important, for both past expectations that by now one would possess some good and

1986 Market Justice, Political Justice

expectations that one will in the future possess that good affect justice sentiments. For the lay person, as for the philosopher, the sense of feasibility implied by these expectations influences justice sentiments (Galston, 1980, p. 110; Moore, 1978, ch. 14). As we shall see, the belief that the market follows natural laws makes market outcomes seem more inevitable than political outcomes. Finally, if one believes that outcomes are attributable to one's own acts—that the self is to be credited or blamed for one's own fate—one does not invoke justice sentiments. Again, this sense of controlling one's own destiny in the market, but not in politics, leads to more of a sense of political injustice than of market injustice.

Conceptually, the problems of a sense of distributive justice may be expressed in a question:

Who distributes what to whom, in virtue of what criterial characteristics, by what procedures, with what distributive outcomes?

Employing this conceptual formula, we turn to a brief analysis of these six terms, comparing their market and political expressions.

American Perceptions of Market and Political Justice

Agent (who distributes). Although in the 1980s about three-quarters of the public agreed both that government favored "a few big interests" at the expense of the "benefit of all the people" and that business pursued "profits" at the expense of "the interests of the public" (surveys reported in Lipset and Schneider, 1983, pp. 17, 183), there is a difference in the legitimacy attached to these two perceived biases. As we shall see, it is more legitimate, and publicly beneficial, for business to pursue profits than for government to cater to the interests of "a few big interests." That difference is brought out in answers to questions on the fairness of the two systems. Answering a series

of questions about the fairness of the American free enterprise system, very large majorities of national samples in 1958 and 1977 held that the free enterprise system is "fair and wise" (82%), "gives everyone a fair chance" (65%), and that it is a "fair and efficient system" (63%) (McClosky and Zaller, 1985, ch. 5). In contrast, in 1980 large majorities believed that "the government is pretty much run by a few big interests looking out for themselves" (70%), and that "you cannot trust the government to do what is right" most of the time (73%) (Center for Political Studies, in Lipset and Schneider, 1983, p. 17). The market is thought to be "fair and wise," but not the government.

Goods. People tend to exclude collective goods when thinking about their standard of living; thus, a large category of governmental distributions is omitted from justice calculations (Compton Advertising, Inc., 1975). Furthermore, although people seek and demand certain kinds of government payments, especially when they are in need, they prefer market income to transfer payments. In spite of Rawls's (1971, p. 440) belief that self-esteem, "perhaps the most important primary good," is distributed by government in the form of political rights, in fact it is the market (and small primary groups) that confers self-esteem (Lane, 1982). Finally, the government, through its police and courts, is seen as an agent of punishment as much as a distributor of goods, even though in concrete forms the services of the welfare state are often appreciated. Thinking concretely, the public appreciates these services; thinking generally, the public tends to derogate them as inefficient and wasteful (Katz, Gutek, Kahn, and Barton, 1975; Lipset and Schneider, 1983). Where benefits are thus discounted (at least in general) and punishments counted, any distributor of "goods" will suffer perceptions of injustice.

To whom. In assessing the market, the public tends to think only of those who are in the market, and more particularly of the full-time employed, forgetting the unemployed and the part-time employed. Reporting on this tendency, Lee Rainwater (1974, p. 166) suggests that "unemployment is the major factor responsible for a divergence in [actual] relative income shares from the shares our respondents judge to be fair." But the unemployed and the poor who are not in the labor market are justice problems for the government, and are perceived to be such (Hochschild, 1981; Rainwater, 1974). Beyond this, other data suggest that the American public tends, more than other publics, to define disadvantages in racial terms—therefore more suitable for political action—than in economic terms, where market justice would seem to be more relevant (Jennings, 1983). Defining the unemployed out of the market and injustice more as a matter of race than income tends to politicize the sense of injustice.

Criteria. As Hochschild (1981) has pointed out, people tend to apply the patently inegalitarian criteria of earned deserts to the market, and, with some anguish and ambivalence, the criteria of equality and need to the polity. Except for political resources, "the idea of essentially equal distribution of resources does not seem attractive to most people" (Rainwater, 1974, p. 168; see also Lane, 1959). One reason for this is the belief that the criteria of earned deserts enhances productivity, whereas the criteria of equality and need do not (see below). Preferring the market's criteria of earned deserts over the criteria of equality and need associated with the polity, the public tends to favor market justice over political justice.

Procedures. For the market to be considered fair, two related procedural requirements must be met: there must be

perceived openness, if not equality, of opportunity, and the market must be considered responsive to effort—to hard work. For the polity to be considered procedurally fair, there must be "adherence to a general norm of political equality," at least among groups if not among individuals (Verba and Orren, 1985, p. 214), and one must believe that the government is responsive to one's influence. By and large, the market passes the test but the polity does not. Over the 1970s, about 65% of the public believed that "hard work is most important in getting ahead," while most of the time fewer than 10% held that "luck or help from others is most important" (NORC, 1981). In 1984 more than 85% of the public believed that: "America has an open society. What one achieves in life no longer depends on one's family background, but on the abilities one has and the education one acquires" (NORC, 1984). In contrast, a cross section of leaders rejected the view that there is equality of influence in the United States (Verba and Orren, 1985, ch. 9), and about half of the public has believed for over a decade that "public officials don't care much about what people like me think" (Miller, Miller, and Schneider, 1980, Table 4.7; Lipset and Schneider, 1983, p. 22). These interpretations are confirmed by direct questions on procedural fairness: Only about a quarter of a sample of 300 adult Chicagoans thought that the procedures used to decide tax levels were fair, and only two-fifths thought that procedures used to allocate benefits were fair (Tyler, Rasinski, and McGraw, forthcoming).

Outcomes. The crucial question, of course, is whether, in the end, people think that they get what they deserve. As we mentioned, a sense of deservedness is central to the concept of justice, at least among the lay public, if not always among philosophers (e.g., Rawls, 1971, p. 311; but see Rescher, 1966, p. 48). What is

striking about the survey material is the high proportion of people who believe they do get about what they deserve. For example, in a small study comparing British and American adults in London and New Haven, 62% of the Americans stated that they "personally have the standard of living that [they] feel they deserve" (Perkins and Bell, 1980, p. 81). In a larger study of 400 employed men and women (including housewives), both men and women in high- as well as low-prestige jobs (almost equally in both cases) scored high on a "deservedness scale"—i.e., "they feel that they are receiving approximately what they deserve and what they expected to receive" (Crosby, 1982, p. 64). The unemployed are different only in a modest degree (Schlozman and Verba, 1979, p. 141). These judgments of the fairness of the market do not extend to the way others are treated (e.g., women referring to other women, the public referring to "most people"), but since correcting injustice to others implies political means, at that point we enter the political arena.

While similar questions on the fairness of the benefits one receives from government and the taxes one pays do not reveal "moral outrage," they do reveal a much lower sense of fairness than these studies of market fairness show. For example, compared with almost two-thirds of the New Haven sample reporting they received the incomes they deserved, only a little over a third of the Chicago sample thought that the taxes they paid and the benefits they received from economic policies were fair (Tyler et al., forthcoming). Similarly, in a comparison of leaders' assessment of income and assessment of political influence, political justice is considered less fair than market justice. When asked to construct a preferred income distribution among occupations, leaders of various social groups (labor, business, blacks, feminists, etc.) rank occupations by income in the same

order in which these occupations are currently paid, and place themselves at approximately the same ordinal position as the one they now occupy. However, when they are asked to order a preferred distribution of political influence they radically revise what they perceive to be the current distribution in such a way that their own group's position is much improved. That is, people generally are seen to get about the relative magnitude of income they deserve in the market, but, given the norm of equal (group) influence mentioned above, other groups, but not one's own, are seen to have more influence than they deserve (Verba and Orren, 1985, chs. 8-9).

On balance, therefore, it seems that the public tends to believe that the market system is a more fair agent than the political system. People tend to include the problem cases in the political domain and exclude them from the market. They ignore many of the public benefits and, with certain exceptions, prefer market goods to political goods. They prefer the market's criteria of earned deserts to the polity's criteria of equality and need, and believe that market procedures are more fair than political procedures. They are satisfied that they receive what they deserve in the market, but much less satisfied with what they receive in the polity. By a different measure, they are much more satisfied with the general income distribution among occupations than with the distribution of influence among social groups in the polity.

There is a "deep structure" to these assessments residing in the very way these institutions distribute their goods, but, as we shall see, this "deep structure" is also partly a matter of perception.

Market and Political Invitations to Justice Complaints

Because their functions and responsibilities differ, our two institutions invite

justice complaints in different ways. They differ in four ways. The first difference is in the way they enlist and frustrate individual beliefs, preferences, and levels of assessment. Among the many such beliefs and preferences—e.g., the goodness of human nature, the sources of authority, the reliability of various kinds of knowledge, and the unity or diversity of mankind (Lane, 1972, ch. 10)—we focus on four: dominant values, satisfaction of wants, preferred justice criteria, and level of assessment (process or outcome). Second, the perceived causes of agency allocations differ, relying on external circumstances in some cases and on the dispositions and values of the allocators in others. Here we focus on a systematic difference: firms are thought to be bound by circumstantial necessities, but governments are thought to be free to follow political discretion. Third, the justice process is rarely free from constraints, such as lack of resources or authority, legal limits, force majeure, and so forth. We will focus on two such constraints: the disadvantage of coming second in a distributive sequence, and the constitutional constraints imposed by rights. Finally, the perceived harmony or conflict of interests involved in market and political distributions alters perceptions of justice; we explicate these perceptions in terms of class conflict, the uses of profits, and competition.

Structured Differences in Individual Preferences and Levels of Assessment

Purposes and values. As mentioned, both philosophical discussion and research evidence point to the selection of justice formulae in order to serve other values (production, solidarity, personal development, and so forth). Thus the perceived purposes of our two institutions and the values they enlist affect the selection of justice principles. There are times, of course, when the nonmaterial purposes of government are ascendant, as in the case

of war or when freedom is threatened, but for most people most of the time earning a living and related material values take precedence. In assessing public policy, for example, three times as many Americans give priority to policies promoting income stability and growth compared to policies promoting freedom, "more say in government," or other values (Inglehart, 1981). In that limited sense there are three times as many materialists as "postmaterialists" in America. Justice concerns may be important, but, as mentioned, people favor the justice of earned deserts—partly because that form of justice is thought to promote higher productivity and income, that is, material values. Furthermore, the market is conceived to be the agent of productivity, while politics is thought to be a burden on productivity, indeed, government is blamed more than the market for failures of the productive mechanism (Katona and Strumpel, 1978, p. 43). A justice system that encourages productivity and that produces income is therefore better than one that, at best, merely redistributes it.

Satisfaction of wants: individual or collective. Both markets and politics are want-satisfying mechanisms, the distinction being that markets satisfy wants individually, while politics satisfy collective wants, or, at least, wants grouped by categories (whose members, of course, individually may want what is offered). While the equal treatment associated with public goods appeals to some senses of justice, in general people prefer to tailor the satisfaction of their wants to their own particular tastes, something they can do in the market but not easily in their dealings with government. If they have the money they can buy the kinds of cars they want, but they cannot order the kind of road system they want. Therefore, the political method of collective satisfaction of wants creates more claims of injustice than does the market method of individual satisfaction of wants.

The distinction may be clarified by a contrast between two concepts of equality suggested by Douglas Rae (1981, ch. 5). *Lot-regarding* equality is achieved for a group whose members have identical goods such that each is indifferent to whose bundle he may have. *Person-regarding* equality is achieved when each person in a group has equal fungible resources to begin with, and with these acquires goods which leave him as well satisfied as every other person, tested by the availability of exchange among group members to the point where no further trades are desired. Notice that both equalities are "fair" in the sense that in both each person is treated identically, but the second is more satisfying to the group members.

Lot-regarding equality may be achieved by the public provision of, say, (1) a rule against smoking applying equally to smokers and non-smokers; (2) the provision of *collective goods*, e.g., public parks equally available to the well and the bed-ridden; or (3) equal allotments of *goods in kind*, e.g., food stamps for the urban hungry and for the farmer growing his own food. While such rules and offerings are fair in one sense, the smoker, the bed-ridden, and the farmer may complain that in another, important, sense they are unjust. The public method of allotment contrasts with the market's method of monetary rewards and transactions that permit individuated satisfaction of tastes.

The flaw in this analysis, of course, is that the market generates enormous inequalities of income, permitting some to satisfy a wide variety of individuated tastes and others to satisfy almost none. This argument will count heavily with egalitarians, but most members of the public "do not find gross inequity in the way income is [now] distributed" (Rainwater, 1974, p. 166). For many, including many of the relatively poor, market processes without equality will seem

fairer than government-induced equality without market processes.

Justice criteria: the polity's reversal of earned deserts. Most people exhibit very considerable sympathy with those who are poor, handicapped, or unemployed through no fault of their own. But how can people be sure that such government beneficiaries are, indeed, without fault of their own? The "just world" syndrome (a tendency to justify all distributions) leads people to "blame the victim" (Lerner, 1980), and that corresponds with the favored justice of earned deserts. The confluence of sentiments of sympathy with sentiments favoring the justice of deserts is painful. Lee Rainwater (1974, p. 180), on the basis of extensive interviews, finds this pain stemming from the opposition of two principles: "Opposed to the principle that no one should live on so little money is the principle that everyone should have to work for what he gets." Transfer payments to relieve poverty present the public with the most acute difficulties in assessing justice, difficulties reflected in the lower evaluation of political justice than market justice.

The justice of taxes, especially progressive income taxes, is even harder to assess. For purposes of contrast we will compare four tax-related experiences and perceptions with partially comparable market experiences and perceptions. (1) People are often uncertain whether others similarly situated are paying the same amount of taxes as they do—the greatest single source of tax cheating (Lewis, 1982, ch. 8). At the same time, people either assume that others in the market pay the same price, or, if they pay less, that they are model consumers rather than cheaters ("I can get it for you wholesale"). (2) Tax payments are compulsory, market payments are regarded as voluntary. (3) The benefits of paying taxes are delayed, often obscure, and sometimes regarded with

hostility (as when a pacifist pays for the defense appropriations he abhors). But market payments for goods and services are inevitably for something one wants, the yield in utility of which is visible and usually immediate. Finally, and most importantly in this analysis, (4) payment of taxes proportional to what is regarded as one's contribution to productivity reverses the normal interpretation of the justice of earned deserts: while in the market one is rewarded for such contributions, in the polity one is penalized. This seems unfair.

Level of assessment: fairness versus justice. Philosophers distinguish between *fairness*, as a method of, or criterion for, allocation, and *justice*, as the outcome of the allocation process (Feinberg, 1973, p. 117; Rescher, 1966, p. 90; but see Hart, 1961, p. 154). But there are times when justice may be defined by the outcome of fair procedures. Thus Rawls's (1971, p. 86) "pure procedural justice" refers, it seems, to market justice, where "there is a correct or fair procedure such that the outcome is likewise correct or fair, whatever it is, provided that the procedure has been properly followed." Rawls grounds his general theory in the rationality of persons in the original position, but there is another interpretation.

The psychologist Philip Brickman asked his subjects to allocate certain scarce goods fairly, but after they had done so they were dissatisfied with the shape of the resulting distribution, finding it too inequalitarian. In their own views, fairness did not produce justice. As Brickman and his associates say, "Even if all groups, as well as individuals, receive what their aggregate merits appear to deserve, the distribution of rewards in a system as a whole can be judged unfair" (Brickman, Folger, Goode, and Schul, 1981, pp. 174-75). Brickman also tested Rawls's hypothesis regarding decisions on distributional arrangements by persons in

the original position. He found that people who did not know their own abilities in a certain situation—one in which those abilities would later earn them benefits—were more egalitarian in their preferences for distributional methods than those who knew they had great abilities. But beyond that, Brickman also found that both those who knew their own abilities and those who did not were more egalitarian when asked to plan a distributional system than when they were asked to decide on fair methods of allocation. Thus, while confirming Rawls's hypothesis regarding the greater egalitarianism of those behind a veil of ignorance, Brickman suggests that part of the egalitarian effect of the original position stems from the difference between looking at outcomes in contrast to looking at distributional criteria (Brickman, 1977).

The relevance to market and political justice both of this distinction between fairness and justice and of Brickman's experiments and hypothesis is plain. The market leads people to think about processes of allocation and deserts, whereas politics leads people to think about outcomes of allocation—the overall shape of a distribution where concepts of equality are more salient. This is true in part because the market has no machinery for adjusting outcomes, or even the conceptual apparatus for thinking about them, except as they emerge from market processes; as in Rawls's idea of "pure procedural justice," the market is a domain of transactions that sum to an unplanned distribution. But politics has machinery in its tax and transfer payment mechanisms for adjusting outcomes, and even the most casual political thinking must turn to questions of the poor and the rich, deprived groups and privileged ones. In politics everyone is to some extent forced to consider societal arrangements, in which other people's claims must be considered along with one's own. This is the very heart of the method of philosophical

justice, in which, assuming reason, each person's claim must be treated as having a worth equal to another person's similar claim. Reason is defined as the process that recognizes identical instances as such, without regard for "accidental" personal differences (Hare, 1963).

Hochschild's (1981) important discovery of public "ambivalence" flowing from a tendency to employ a justice of deserts in the economic domain and, on occasion, the justices of need and equality in the political domain, can now be understood as more than a simple application of different social norms to the two different domains. In their discussion of fairness in the economic domain, people were looking at the process of allocation—the fairness of individual payments, or Brickman's "micro-justice." In their discussion of political justice, however, they were led by the perspective of that domain to consider the shape of outcomes—of social rather than individual justice, or Brickman's "macro-justice." Hence the confusion and ambivalence.

The consequence of this difference in perspective is a kind of asymmetry in the way procedures and outcomes are perceived in the market and in politics. While it is the case that market procedures are, in practice, considered more fair than political procedures, at an abstract level both procedures are considered fair in their respective domains. The main difference between them lies in the fact that the market escapes criticisms for unjust outcomes if it adheres faithfully to its criteria of allocation (contribution to productivity), because people apply to it the criteria of Rawls's pure procedural justice. But the polity does not escape such criticism. A fair procedure is sufficient justification in the market, but not in the polity. More than that, the polity may be called upon to redistribute (see below) what the market "fairly" distributed in the first place, for, from a political perspective, the outcome of that "fairness" is seen to be

unjust, making the polity doubly vulnerable to allegations of both unfairness and injustice.

The Causes of Agency Behavior

Market necessity, individual responsibility, and government discretion. Interpreting the causes of an event exercises a powerful influence on the sense of grievance and, usually, though not necessarily, on the sense of injustice. Where the agent intended to inflict pain the sense of grievance and injustice are most acute; where the agent did not intend to inflict pain, but might have avoided it and did not, our sense of grievance and injustice is severe but less acute; where the agent did not intend and could not avoid the pain inflicted, we may feel grieved, but do not feel that an injustice has been done (Utne and Kidd, 1980). This gradation is securely lodged in our concepts of responsibility (Heider, 1958).

Consider now the way in which people interpret the causes of market and political outcomes. In the market people offer two kinds of attributions, both of them exculpating the market from injustice. On the one hand, people believe, with some room for difference (Lipset and Schneider, 1983, pp. 176-83) that a firm must do what it does do, constrained as it is by competition and the laws of supply and demand. If a firm pays more than market wages it risks bankruptcy. The firm cannot avoid inflicting pain. On the other hand, laymen, like economists, read backwards from rewards, a person's level of pay, to discover the source in his contribution to productivity—a doctrine of "revealed contribution" similar to the economist's concept of "revealed preference." Noone knows what his contribution is worth until he sees what he gets; there is no opportunity for discrepancy between worth and reward, no way in which a hypothesis about a person's worth can be falsified. Thus, by the magic

of the market, a person gets what he deserves and a firm must pay what it pays. These two forms of attribution require a person to believe that if he is unemployed it is through some discretionary act of his own, but that if a firm lays him off it is because it could not do otherwise. No system could develop a more elegant device for avoiding attributions of injustice to the rewards and punishments it distributes.

In contrast, benefits received from government are not considered to be the inevitable and precise measure of some previous contribution or status or need; they are perceived to be the consequence of a partisan dispute in which one group won over another group—e.g., labor over managers and owners, dairy interests over the weaker spokesmen for consumers. What is revealed, or so it is said, is political muscle, which until might is converted into right affords only small purchase for a belief that justice has been done. The polity is, therefore, more vulnerable to claims of unjust allocations.

This distinction in vulnerability to claims of injustice is related to the general belief that in both the market and the polity people are animated by self-interest (Lipset and Schneider, 1983, p. 169). In the market, however, self-interest is thought to be both fruitful for the common good and policed by competition, while in the polity self-interest is seen as neither fruitful nor properly constrained. Unwilling to criticize the system of democracy, the public levels its criticisms at the political leaders: "[In recent decades] Americans continued to say that they were proud of our system of government. Their lack of confidence was clearly directed at the people running those institutions" (Lipset and Schneider, 1983, p. 309). Combining a perception of insufficiently constrained self-interest with the belief that politicians hold discretionary powers in allocating benefits and costs, the public is inevitably suspicious of the fairness of the resulting outcomes.

Constraints

Sequence: distribution versus redistribution. We turn to popular responses to an initial distribution of resources and compare them with the responses to a change in that distribution, a redistribution (Jouvenel, 1951). Consider first the concept so dear to the heart of economists, Pareto optimality, which says that, given the proscription against comparing interpersonal utilities, a situation is just when no transfers can be made that improve the lot, however small, of any one person without diminishing the lot, however large, of any other. By this reasoning, before the cake is divided all distributions are just; after it has been divided no further distributions are just. The market divides the cake in the first place; the government must then alter that distribution and, by Pareto optimality reasoning, nothing the government can do is just. The world pays little attention to Pareto optimality, but the point illustrates the problem of changing any distribution, for that involves taking something away from someone in order to give it to someone else.

The justices of need and equality seem to justify a process of redistribution. But how can they justify taking from an individual who has done nothing to deserve the punishment inflicted on him? As we have seen, the justice of earned deserts finds this taking a grave injustice, and that injustice occurs because the government comes second in the distribution process.

The asymmetry of the public's attitude toward giving and taking in redistribution is revealed in attitudes toward income floors and ceilings: in one survey only 9% of the public favored a ceiling on incomes, while 32% favored a guaranteed minimum income and 81% favored guaranteed jobs so that people could earn a decent income (Verba and Orren, 1985, p. 81). Assuming tax and transfer methods, one might say that facilitating *earning* is

best, *giving* to the truly needy is acceptable, but *taking*, even from the rich, is bad. Redistribution implies little earning, and both giving and taking.

Trespassing on rights by government and business. From their reserve of sovereignty the people have granted government certain powers which, if they are exceeded, lead to claims of injustice. These boundaries of legitimate governmental action are marked by rights, among which are the rights to property and contract. While it is certainly the case that corporations are granted their special rights by the state, business in general has not been granted its powers from any source. Rather, it seems, business powers derive, without such a grant, from the same reserved powers that serve as the source of governmental powers. Government, therefore, is more subject to claims of trespass than is business, and, being subject to such claims, more vulnerable to cries of injustice.

Harmony and Conflict of Interest

The problem of distributive justice arises only when there is a conflict of interest; it is moot when there is a harmony of interest (Barry, 1973, p. 15). Of course, businessmen and candidates for jobs know that they are in conflict with others, but the crucial question is the one that Marx (and others) identified: Is there a basic conflict of interests between owners and workers, management and labor? The answer to this question turns on whether one considers periodic pay disputes as "basic," or only as quarrels among allies in the productive process. Following that answer is the further question of the expression of such conflict as there may be in the economic and political domains. We examine these questions in three parts: class conflict, the uses of profits, and the nature of competition.

Class conflict. Of a national sample of Americans asked in the period 1975-1977

whether workers and management (1) "have conflicting interests and are natural enemies," or (2) "share the same interests in the long run," only 13% thought that workers and management have conflicting interests, while about two-thirds agreed that they have "the same interests in the long run" (McClosky and Zaller, 1985, Table 6-4). A different study with a comparable question (without reference to "natural enemies") conducted at the same time also showed about two-thirds holding that management and workers have "basically the same interests," but this time about a third held that they were "basically opposed." Moreover, this second study showed a declining relation between a belief in "basic opposition" and occupational level, such that by 1975 professional and managerial employees were scarcely different in this respect from blue collar workers (Schlozman and Verba, 1979, pp. 125, 130). Without a sense of class conflict the problem of justice between social classes is, as we have said, not a major issue.

But why does this weak or absent sense of class conflict and declining class division not also reduce the perception of conflict in politics? Some of the conditions for de-emphasizing class conflict in politics certainly exist. The two main parties have, over the years, followed the lines indicated: in the 1970s and early 1980s there has been very little difference in the class composition of the voters for the two main American parties (Ladd and Hadley, 1976; but see Pomper, 1985, p. 67). But it cannot be said that political conflicts have declined in intensity, for a variety of other issues—the Vietnam war, the Central American engagements, defense policy, prayer in the schools, racial issues—has kept the political conflict boiling. Structurally, then, the low sense of class conflict and apparently declining class divisions of the American public express themselves in a low sense of worker-management conflict in eco-

conomic matters, where other issues are irrelevant, but not in a comparably low sense of political conflict, where other issues have become salient.

Profits. The idea of a harmony of interest is contained in market theory: as businesses prosper—that is, as they make larger profits—everyone prospers. Behind this thesis is the assumption, not that profits are deserved—though that will be said, for it is an economic theory and not a justice theory—but rather that profits are fruitful for the system: (1) they spur businessmen to greater effort, and (2) they are reinvested in the system, fructifying the economy. The harmony of interest lies, then, in the belief that “surplus value,” to use an unfriendly term, goes back into the economy as the source of future prosperity, which, in turn, will benefit all, even if it is allocated quite unequally. The alternative theory, of course, is that surplus value—profits—goes to make the rich richer and does nothing for the ordinary person; in this version, all would be better off if smaller profits remained after earnings were distributed as wages and lower prices. There is evidence that the public is divided on this point (Lipset and Schneider, 1983, pp. 176–83), but the weight of the evidence lies with the harmony theory: profits are legitimate, both as earned income for owners, and—what is at stake here—as the source of “good times” and future income for workers.

Four different kinds of evidence may be cited. (1) Among the research findings are those from coded answers to open-ended questions showing that of those with opinions, two-thirds believed that profits were reinvested, and a large majority believed that a rise in profits meant “the good life” (Compton Advertising, Inc., 1975, questions 13a–13c). In answer to direct survey questions, twice as many respondents believed that “if business is allowed to make as much as it can . . . everyone profits in the long run” as

believed that “the workers get less.” The profit motive is thought by most people to be the cause of “higher productivity and investment” (McClosky and Zaller, 1985, Table 4-5). (2) A computer simulation of various labor strategies under circumstances in which profits might be used for owner consumption or for reinvestment found that the most advantageous strategy is not to maximize demands but to moderate them (Przeworski, 1980). (3) Studying labor union responses in Germany just prior to World War I, Barrington Moore (1978, p. 220) found that union discipline and restraint “received considerable support from the awareness that in the end jobs depended on the prosperity of German Industry.” Finally, (4) the idea that profits are the source of benefits for all is supported by the overwhelming endorsement of the “free enterprise system” that embraces this tenet among its principles (Harris Survey, in Lipset and Schneider, 1983, p. 286). From such evidence it seems fair to say that people generally regard differences of opinion on the use of profits as family quarrels, and not as basic conflicts.

Skipping over the many objective reasons for public uncertainty on the uses of profits (ignorance of the meaning of the term, possible owners’ uses for their own consumption, investment overseas or in nonproductive but lucrative instruments, concealment), let us examine one of the justice implications of this harmonic interpretation of the system. It is just for an owner, say, to take \$100 from what may be interpreted as a common fund, when each employee takes, say, only \$10. In the harmonic view, the owner is justified in an income ten times that of his employees, on the grounds that if he should invest, say, half of that in the firm—still living five times as well as his employees, given the non-progressive tax system—the employees’ wages might rise to \$12 in a future year. Meanwhile, the owner’s income might fairly rise to \$125, slightly

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increasing the inegalitarian ratio and substantially increasing the absolute difference. Thus, the belief that (a) profits will be reinvested, and (b) the benefits of reinvestment will return to the employees is, in this case, a powerful argument for inequality of income.

Where lie the deserts in this case? Assuming that the owner's rate of return was average, his claim for a reward for skill is minimal. His claim that he deserves the income for "waiting"—Marshall's (1938) argument, now obsolete (Robinson, 1958, p. 394)—is matched by the employee's claim to have waited just as long.

But even if there were a justification for increased gain on the part of the owner, what would justify the employees' higher salary in later years? It seems more realistic to waive all efforts to ground these outcomes in the justice of deserts, and to recognize the play of a self-interest justified only by the recognition by all parties of certain rules of the game—certain procedural requirements within which hedonistic motives are allowed because, shall we say, of the harmony of interest involved. In this sense, then, harmony of interest takes the place of justice, and claims of justice will be muted.

These considerations overflow into the political domain in several ways. In the first place, it has been said that "government policy favoring business is seen [by the public] not as a response to the special interests of business but as a policy that serves the economy more generally" (Verba and Orren, 1985, p. 216). Moreover, anything that makes the "business climate" unfavorable, as businessmen see it, is thought to reduce investment and increase unemployment, "the fiendishly clever" device that Lindblom (1982, p. 324) says imprisons us in a market society. In this context political justice is seen to be served by catering to market justice. Second, confidence in the free enterprise system stifles political reform,

for the public strongly opposes at least two of the ways to alter the flow of profits: they do not want (by 73% to 13%) limits on profits (McClosky and Zaller, 1985, Table 6-2); and they oppose nationalization of industry even more strongly (Lipset and Schneider, 1983, pp. 265-69). One can imagine, indeed, that the fear of government intervention (in these, but not necessarily in more limited ways) is a source as well as a consequence of the more hopeful view of the harmony in the economic system, a kind of backflow following the overflow we mentioned.

But these limits on government do not lead to exculpation of the government for any injustices perceived in the market; in the end, the responsibility for rectifying any injustices of the economic system lies not with the market, but with the government. Rainwater (1974, pp. 180, 186) says of his respondents that "they look to government as the mechanism for developing policies for this improvement [in the fairness of the system] and then enforcing it," but that "they do so without conviction, because it is hard to imagine how the desirable end result . . . might be accomplished." Bricks without straw, ends without means, injustice lodged at the door of the government without any approved policy for rectifying it.

Almost as many Americans name politicians as being unfairly advantaged as name the rich and big business (Jennings, 1983). One reason for this attitude toward politicians may be that, in contrast to the belief that employees benefit from owners' profits, people not only believe high politicians' salaries do not benefit them, but that these salaries come from their own tax payments. There is no harmonic device here, but there is a conflict of interest.

Competition. In both markets and politics people welcome competition: power checking power, firm competing with

firm, party with party, and indeed government and business checking each other. Members of the public "distrust power and its potential for abuse in any setting, public or private . . . [although] most Americans are more fearful of state power than of business power" (Lipset and Schneider, 1983, pp. 256-59). But there is a fundamental difference in the structure of the competition between firms and that of the competition between parties that makes market competition, at least for commodities, seem more benign than political competition. In the commodity market, the consumer is not a competitor, but an interested and favored third party. Not being a member of the competing firms, he has everything to gain from their competition, no matter who wins. In electoral competition, on the other hand, the citizen is a participant in the conflict; thus, like an owner of an enterprise, he can expect to win those things for which he voted only if his side wins. In the commodity market all consumers gain something from the competition, no matter who wins a greater share of the market, but in political competition, if it is closely contested, only a little over half the citizenry gains its partisan objectives.

Put differently, for third parties—like consumers in the market—competition is always a positive sum game, while in politics it is a zero sum game. Positive sum games permit greater harmony (indeed, they permit Pareto optimality), while zero sum games are patently antagonistic.

For the ordinary person the labor market is quite another matter, yet competition there occurs (as we have seen) within a general agreement about the hierarchy of income. There is, however, no such agreed upon hierarchy of political influence, in which each group would leap over the others and place itself at the top (Verba and Orren, 1985, ch. 9). Unlike competition for income, competition for

influence takes place without any consensus as to who should have what, and is therefore bound to be more antagonistic in nature.

There is a more fundamental reason for these different assessments of justice in the very concept of conflict in the two domains. As suggested above, the market can claim that, in some sense, when one person gains something others gain something too. In this fashion economists from Adam Smith on have been able to claim—and a large section of the American public to accept—the idea of a harmony of interest. Democratic theory and its popular interpretations are less happily endowed. Except for Rousseau—and he only by assuming people abandoned their particular interests—democratic theory has located the harmony of interest in the preservation of a framework within which an accepted conflict of interest takes place. The "harmony" does not arise from the reciprocal advantages which each party to the conflict receives from the other's advantage, as in the market, but rather from an exterior, partially removed, joint advantage in maintaining that conflict in peace, if not in actual harmony. Democratic theory assumes a conflict of interests and seeks to channel it, while market theory assumes that underlying the conflict is a harmony of interests. Since questions of justice arise only in situations of conflict, the acute sense of conflict that is present in politics but not in markets stimulates the justice nerve with greater force and arouses sensitivity to political injustice.

Summary and Implications for Political Economy and Justice

The public is endowed with an active sense of justice, a sense that has been said to be "the basic template for organizing one's view of the world" (Lerner, 1981, p. 23). This sense of justice seems to influence judgments of candidates more than

does perceived self-interest (Tyler et al., forthcoming); it is employed in interpersonal relations (Lerner, 1981; Mikula, 1980), and in appraising the police (Tyler & Folger, 1980), the courts (Sarat, 1977; Thibaut and Walker, 1975; Tyler, 1984), and not least, market processes (Curtin, 1977; Hochschild, 1981; Rainwater, 1974; Tyler and Caine, 1981). However, partly for the reasons given and partly because the market has been an enormously fruitful device for generating wealth and relieving poverty, the public favors the market.

What happens in a political economy in which the market is regarded as "fair and wise" and political practices, at least, are regarded as neither? In such a society, however much people sympathize with the unemployed, the handicapped, and even children, they will regard these non-productive others as externalities, for in the market they are undeserving. People tend to prefer somewhat more egalitarian outcomes than the market provides, but their love of market methods inhibits them from advocating any solutions that seem to frustrate these methods. Even where the market's methods are thought to be unfair to certain groups, such as blacks or women, the intrusion of the government into the sacred market precincts is often regarded with suspicion, for the government's program of rectification trespasses on the evaluation of persons by the market's process of revealed contribution. Where government purposes are approved, their implementation is stifled, at least partly because such implementation violates market fairness norms. Belief in these norms weakens the belief, say, that a worker's child does not have a fair chance at success (Schlozman and Verba, 1979, p. 110), as well as a veridical perception of one's own life chances. Astigmatism of this sort also weakens collective effort, through unions and labor parties, to alter life chances. Minimal government is assured and people are

endowed with more commodities and fewer collective goods. The dynamism of history is confined to market dynamics—technical and material, but not ethical. In an imperfect world, employing imperfect mechanisms to achieve these flawed results may be the best we can do, but to give up so easily is contrary to people's belief in their capacity to control their own destinies.

Perceptions and Structures

We have argued that the preference for market justice over political justice has deep structural roots in the purposes and values of the two institutions, in the way they satisfy wants, in their differential employment of justice criteria, and in the levels of popular justice assessments in the two domains. We have seen how the attributions of causes, how the differences between initial distribution and redistribution, and how the application of rights differentially affect justice claims. The perceived harmony of interest in the market was contrasted with the perception of conflict in politics. While these structural effects are indeed significant, at an even deeper level they are themselves partly perceptual, ideological. That is, social structures are vulnerable to variable "definitions of the situation" (Magnusson, 1981; Mischel, 1968), to the "social construction of reality" (Berger and Luckman, 1967)—in short, to ideology (Geertz, 1964; Lane, 1962). Ideologies are, on the one hand, the joint products of both culture and what people bring to an interpretation, and on the other, "structures"—or what may be said to be "objectively" there—of dispositions and circumstances, of culture and science (Kardiner, Linton, DuBois, and West, 1945, p. 34; Nisbett and Ross, 1980). While interpretations of justice, as parts of ideologies, are more cultural and dispositional than other parts, they take their cues from perceptions of circumstances. They are not given

in a situation; they are constructions placed upon situations.

At least three kinds of current evidence support the independence of perceptions (including what we shall call "the community point of view") from structural determination and from perceived material self-interest: (1) research showing that concepts of justice are only loosely related to perceptions of self-interest (e.g., Tyler et al., forthcoming); (2) research showing that people seem to vote more according to national news of unemployment and inflation than according to their own perceived benefits and burdens (Kiewiet, 1983; Kinder and Kiewiet, 1979); and (3) research documenting what we have called "sociological release" (Lane, 1978b). On this last point, it has been found that it is their ideologies, quite independent of their relative incomes, that determine people's attitudes toward income distribution and their perceptions of social advantage and disadvantage (Jennings, 1983; Verba and Orren, 1985, p. 180). Like Mannheim's (1949) intellectuals, people have already been partially liberated from the perspectives of their social positions, and therefore from structural determination.

Without greatly modifying (except for a change in policy emphasis) the institutions that have served us reasonably well, without weakening the justice of earned deserts that is both ethically honorable and psychologically rooted in our desire to be the cause of our fate, and without going against the grain by undermining materialist values, can we reconceptualize social structures to offer a different, and, I think, more veridical construction of reality? We focus on two reconceptualizations: (1) contributions to production, often slighted by egalitarian socialists (e.g., Harrington, 1973) and by philosophers of justice (see comments by Galston, 1980, p. 223; Rescher, 1966, p. 89), and (2) taking a community point of view.

Contributions to production. (1) Instead of being based on personhood, or on the idea that we deserve equal concern from government because we are "human beings with the capacity to make plans and give justice" (Dworkin, 1977, p. 82), public justice might be based on the idea that we all contribute to society—not, it is true, that we contribute equally, but that we all contribute something. In a full employment society (this is the policy change), the problem of deserts is partially solved: while children and the handicapped are embraced by an unambiguous justice of need, everyone else—including those contributing to child care in the home—is embraced by a justice of earned deserts. Although difficult to achieve, a full employment society is not impossible (viz. Sweden) and it is better than guaranteed incomes which both erode work efforts (Danziger and Associates, 1981) and violate the justice of earned deserts. We may keep the market's preferred "fair wage" measured by private net marginal productivity (provided this productivity is everywhere equal, and equal to social net marginal productivity [Pigou, 1949, pp. 549–50; but see Thurow, 1973, pp. 70–73]), and publicly supplement the incomes of those who are below some minimal standard of living. Since people's unfavorable assessments of welfare payments are based on the suspicion that the payees are not trying to earn, rather than that they are not actually "earning," their incomes, supplementary benefits escape popular obloquy. And with this change come some of the benefits of the republican ideal: we are all members of the Republic because we all contribute to it.

(2) Employing the justices of equality and need in its educational and health policies, the government creates human capital that promotes productivity quite as efficiently as does physical capital. Thus, the justices of equality and need can also add to productivity. By these and other means the government creates

wealth, and does not merely redistribute it. As a producer, government may fairly claim its own income by the justice of earned deserts.

(3) To accept the market's criteria of "contribution to production" is to ignore history, the history of the many previous contributions to productivity that make possible the current high level of payments to members of affluent societies. If we are pygmies standing on the shoulders of giants, what is owed to the pygmies and what to the giants, or to the giants' inheritors, society? Without a method and without consideration of incentive effects, Hobhouse's (1922, pp. 161-63) argument along these lines remains barren, but it has far-reaching implications that deserve attention: wages, interest, and profits owe so much to society that their allotments by the market are arbitrary, because they do not accurately reflect contributions to productivity. Social benefits, then, may be seen as society's return to individuals of its inherited contribution to productivity. Taxes may be seen as payment to society for its trusteeship of the giants' past contribution.

(4) By one set of calculations, the market rewards people as much on the basis of luck and chance as on the basis of performance (Jencks, 1973). As a kind of Darwinian device, the market's reward to chance factors does not alter its productive efficiency, nor the fruitfulness of its incentives, but it does alter its justice—and therefore the injustice of "taking" mentioned earlier. This consideration relaxes the relationship between justice and productivity that forms one basis for the preference of the market's justice of earned deserts.

The community point of view. Without giving up their cherished individualism, people can take a community point of view with at least six relevant effects: (1) As we have seen, viewing the world from

the bottom up, people see processes rather than outcomes, emphasizing individual deserts rather than the overall shape of a resulting distribution. The community point of view restores a more egalitarian attention to distribution.

(2) Government, market, and other partial accounting systems are always incomplete from the community point of view, for there are always costs and benefits representing externalities for the unit involved: governments issue regulations for which firms pay the administrative costs; firms profit from relocations, pollution, and possibly unemployment, for which communities bear the costs; government policies have unintended effects for which "community impact," "family impact," and other impact studies are commissioned. In some sense these represent "injustices," that is, undeserved or unmerited benefits and burdens. While economic rent and windfall profits and their opposites are inevitable, accounting systems that embrace these externalities (e.g., Bauer and Fenn, 1972; Schultze, 1977; Sheldon and Moore, 1968; Terleckyj, 1975) reveal the deficiencies of market and governmental accounting by incorporating more of the community point of view. Inevitably, the preference for market justice and the concept of earned deserts is affected by the changed perspective.

(4) The segmented accounting systems of individuals, like those of the market and government, distort perceptions affecting justice assessments. For example, popular perceptions of government discretion, and therefore of unconstrained bias, often rest on the failure to see the necessity of government tradeoffs, especially, but not solely, in terms of the logic of taxes, expenditures, and deficits, leading to a "something for nothing" mentality (Sears and Citrin, 1982). In a segmented accounting system it is "costless" to want everything and to see injustice in any denial. The community point of view

asks the public to internalize these choices: "What am I willing to give up for the policies and services desired?" Thus some of the constraints seen to apply to firms in the market are also seen to apply to government choices, and governments may be perceived as applying justice with less freely available discretion.

(5) The market's advantage over the polity seems to lie in its solution to the problem of free riders pursuing "the logic of collective action" (Olson, 1971)—a moral problem as well as an economic one. But introduce any common good, such as clean air or a healthy and safe environment, and market forces lead to a less felicitous solution, sometimes called "the tragedy of the commons" (Hardin, 1968), collectivizing injustice as well as impoverishment. While the community point of view seems to rely on moral incentives, in the tragedy of the commons it enlists material incentives and the value of productivity discussed above. Indeed, in the tragedy of the commons, market calculations of individually earned deserts lead to lower productivity.

(6) From the community point of view political conflicts are like labor-management quarrels over the disposition of profits: they are frankly quarrels. As in the harmonic view of the market, benefits to others are also, if in lesser degree, benefits to the self, partly because of the market itself: the grocer, made unhappy by the taxes he pays to support welfare, is happy to receive the custom of his welfare clients. The community point of view makes the connection.

Without entering here into the crucial economic implications of these changes in perception, it is clear that reinterpreting productivity and looking at the world from a community point of view somewhat erode the structural advantages of market justice over political justice; these perspectives represent counterweights in the scales of justice now so unevenly tilted against political justice in favor of market justice.

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FROM TRAGEDY TO HIERARCHY AND BACK AGAIN: WOMEN IN GREEK POLITICAL THOUGHT

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*The earliest attempts at a theoretical understanding of politics occur in the city-states of ancient Greece. Women had no place in the politics of those cities. However, the Greek tragedians and philosophers raised questions about the fundamental assumptions underlying political life by introducing women into their writings. Thus, women appear in some Greek tragedies as a counter to the male sense of political efficacy—the sense that men can create through speech and ignore the facts of physical creation entailed in the process of reproduction. A discussion of two tragedies, *The Seven Against Thebes* and *the Antigone*, suggests how the failure of male political leaders to acknowledge the demands of the physical and that which is different brings on tragedy. The Socratic response in the *Republic* is to overcome tragedy by making the male and the female the same. Aristotle attempts to incorporate sexual difference in the theoretical framework of hierarchy. Finally, there is a brief consideration of the role of the pre-Socratic philosophers in setting the agenda for the Greeks' confrontation with the problems of incorporating difference into the political community.*

The Greeks introduced the concept of politics to the Western world; their city-states, or poleis, were the arenas in which citizens might act together, sometimes seeking domination over other cities, sometimes creating orderly sets of rules by which they might govern themselves, sometimes finding in their communal actions glory as cities or as individuals. The Greek philosophers, reflecting on the nature of the polis, gave to the city its theoretical meaning as a realm of potential justice as well as of conflict, of human nobility as well as of fatuity. But always this was the world of men; men were the actors, the seekers of glory, the pursuers of power. They were

the ones who debated in the assemblies, who decided on public policies, who gave expression to the values of the city. The women of Greece were not part of that world.

Conceptions of politics and the models employed to analyze political relations still reflect their origins in the masculine world of the polis, with its concern for domination, self-rule, order, and glory. While the Western intellectual tradition may have accepted the practice of the Greek polis as revelatory of the original meaning of politics, the philosophers and playwrights of ancient Athens reflected critically on that world. In particular, they questioned for their audiences the

focus on power and its pursuit, and the centrality of rationality and its efficacy in ordering the chaotic world of experience. To raise questions about the masculine world of power and reason—a world focused on male potency—they turned to the female, for in her difference from the male she revealed a diversity in nature that threatened the physical order and rational control at which the polis aimed. The male in the Greek tragedies seeks a simplicity, a uniformity, a world he can comprehend through the intellect. When confronted with the female, he must face the problem of difference and complexity, for she introduces the issue of reproduction, which underscores the male's dependence on what is other. The female forces the Greek authors to raise questions and reservations about the ancient polis as a realm of domination and simplicity. These authors indicate for us how the female and the questions she raises about the efficacy of reason and the centrality of power and authority must be acknowledged in all understandings of the nature of the political world, and in the attempt to incorporate that world into theoretically simplified structures.

The life of citizen women in fifth century B.C. Athens was a sheltered one, brief and limited primarily to the production of citizens for the polis and sons to carry on the family religion. However, the portrayal of women in the works of the Athenian playwrights and philosophers is far more complex and sophisticated than the facts of women's daily lives might lead us to believe. Though women themselves probably did not attend theatrical performances, the city of adult males saw on stage powerful women—women whose existence, as the poets reflected on the human condition, could not be denied (Gomme, 1937; Just, 1975; Kitto, 1951, pp. 219–34; Lefkowitz, 1981, pp. 4–11; Pomeroy, 1975, pp. 58–60). Into their vision of themselves as human—somewhere between gods and animals—the

poets introduced the female as a constant reminder of the diversity out of which the world was made, and as a constant warning against the attempt to see the world as one, as uniform and therefore subject to simple answers and rational control. The closeting of women in the home did not shut out their existence from the consciousness of the male poets or of the male citizens for whom they wrote.

The aphorism "know thyself," originally engraved on the Greek temple at Delphi, has often been adopted by political theorists of the modern age. Hobbes, for example, uses it to indicate that we must know our passions, those interior motions that drive us into conflict with others. Rousseau recalls the motto in order to underscore our need to discover our origins, what we were before fateful accidents of history took away knowledge of our true selves. Both Hobbes and Rousseau take the motto as an exhortation to discover one's own nature. For the Greeks it had a somewhat different meaning. "Know thyself," *gnothi seauton*, meant to know the limits of human activity or power, to recognize that as human one was not immortal, and more importantly, that one was not omnipotent—particularly that one could not control all through human reason. The female on the Greek stage forced men into an awareness of the inadequacies of the attempt to control all, of the inability of human courage and human intelligence—often expressed through political action—to dominate the natural world through the denial of variability. He who tried to dominate may have gained stature as the hero, but he was the tragic hero, since such attempts at power and at the imposition of simplicity brought only disaster. Women brought the hero back to what we might call a variable, empirical reality; their presence suggested that there was something other than the abstract city the men had created, and for which they fought.

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It was in the structure of the city that the male showed his most courageous attempt to create by giving birth through institutions, thus ignoring the importance of the female for human reproduction. Many of the Greek tragedies, though, indicate the inadequacy of such assertions of political power when they are not moderated by recognition of the variability of nature—a concept of nature based on physical rather than intellectual generation, which thus arises from diversity rather than from simplicity. The male on the Greek stage who tries to live without acknowledging the female and the diversity she reveals encounters tragedy. The female, even in her varied manifestations on stage, illustrates the dependence of the human being on others. She, as different from the male, but also needing the male, underscores the diversity of the world, and the falsity of any vision of self-satisfied independence, omnipotence, or simplicity. This is not to suggest that all women in the Greek plays are the same; certainly there are many differences among the female characters themselves. Rather, in whatever role they appear, women raise for men the problem of difference. Tragedy, as the Greek playwrights portray it, is not caused by women, but rather by the failure of the hero to recognize man's relationship to a diverse natural world and the need to adapt to that diversity. The male, in the rational construction of political order, thinks he can accomplish too much. Greek tragedies reveal the limits of human rationality and human art. For the Greeks, it is women, absent themselves from the audience of citizens watching the plays, who cause men to know themselves.

In this essay I will consider two very different Greek tragedies, Aeschylus's *The Seven Against Thebes* and Sophocles' *Antigone*, to illustrate some of the themes suggested above.¹ I will then offer some comments on how the philosophers of the

fourth century B.C. unsuccessfully tried to deal with and ease the tensions explored by the playwrights of the previous century. I conclude by returning to the earliest philosophers, the so-called pre-Socratics, to suggest how they laid the foundations for the Greeks' intellectual assessment of the city and the female.

Tragedy: The Failure of Male Omnipotence

Aeschylus's *The Seven Against Thebes*

Aeschylus's *The Seven Against Thebes* is the final play of his version of the Oedipus cycle.² Oedipus has died cursing his two sons, the offspring of his incestuous marriage. His sons, Eteocles and Polyneices, agree to take turns ruling Thebes, but Eteocles then refuses to yield his power. Polyneices, eager to claim his turn, returns to Thebes with an army from the neighboring city of Argos. The structure of the play is simple: the hostile army with Polyneices at its head has attacked; Eteocles presents himself as the calm leader of a besieged city; a chorus of terrified townswomen sing of their fears. Eteocles worries that they will instill fear and disorder among his soldiers. During the middle part of the play a Theban messenger describes the Argive warriors who wait for battle at Thebes' seven gates. At the seventh gate stands Polyneices, and it is at that gate that Eteocles and Polyneices meet and kill one another in the subsequent battle. At the end, Antigone and her sister Ismene mourn their brothers, and a decree enacted by the city council is announced, proclaiming that Eteocles is to be given full burial rites, while Polyneices' body is to be cast unburied outside the city.

As with all Greek tragedies, the story of *The Seven Against Thebes* is embedded in a series of myths that lie behind the action and are subtly referred to throughout the play, thereby underscoring its central

themes. For our purposes, the myth of autochthony, or birth from the earth, gives meaning to the action of the play. Thebes was first founded by men born of dragon teeth planted in the earth—that is, the earth was mother, not the human female. The play begins with Eteocles' appeal to his earthborn ancestor, Cadmus (1; cf. 9).³ He looks towards origins that exclude the female, and thereby denies human motherhood (Caldwell, 1973). In so denying his own origins, he envisions the perfection of a city without women. However, it is a perfection both nature and the playwright deny him.

After reminding us of his autochthonous ancestry, Eteocles portrays himself as a captain of a ship "with hand upon the tiller" (3). As such he urges the defense of the city, equating it with the earth, the beloved mother who nourishes her offspring (15–20; cf. 69). The women, who will make up the chorus, are not perceived as mothers; defending the city is not for the sake of the women within its walls, as it was for the Trojan heroes of Homer's *Iliad* (bk. 6), but for the earth that has replaced the human female.

Into this vision of male exclusiveness intrude the Theban women. They are in a panic, beseeching the gods to save them from the violence raging outside the city's walls. Their screams and their disorderly movement call forth from Eteocles, who has just presented himself as the captain with a firm hand on the tiller, one of the most famous misogynist speeches from ancient tragedy. *Thremmata*, he calls them, vile things (181). Can't you keep still? You endanger the city with your disordered screams. Like the women who complained about the sounds of the approaching army, Eteocles now uses vivid verbs to describe the wailings of the female chorus, howling like dogs, hateful to those who practice moderation (186). Then he implores, "Whether in good or evil times, may I never live with the race of women" (188–89). Women filled with

terror, as are the ones before him, cause evil to the household and to the city (191). Eteocles refuses to acknowledge that without this "evil" there would be neither household nor city. The age of earthborn men is past, despite his invocations and his dreams. If the city is to survive, if the household is to survive, then he must live with the race of women. But Eteocles sees the female only as a danger, because she alerts men to what is other than the city and the earth out of which it grows. Eteocles' reaction to the women is to deny them a place in the city, and to deny that there is anything other than the city. The city is the whole of Eteocles' existence. He believes it would be better if the city could do without women, and creativity could again come from the earth to which he is willing to devote himself. Eteocles' vision is of a city that is one, rather than divided between male and female.

Much of the first third of the play is a confrontation between the fearful females and Eteocles' masculine rejection of their fear. Often this conflict is couched in terms of speech and silence. Eteocles, wishing to live without women, states they must not partake in the counsel of the city; that is, he calls for the women to be silent. They respond by asking him to speak (200–201, 230–32, 261–63). Eteocles, the man with his hand upon the tiller, has and uses logos, while the female, whose shrill wailings spread fear throughout the city, must learn silence. Eteocles, as leader, uses speech to create order by dismissing the feminine passions. The first third of the play concludes with the male controlling both the city and the chorus of women.

During the central part of this play a herald describes each Argive warrior waiting at the gates. Eteocles then sends Theban heroes to meet each one, exhorting them on occasion to fight for their mother—meaning the earth—or recalling their earthborn origins (416, 474). Meanwhile, he ignores the women who live in

the city and comprise the chorus. When Eteocles learns that Polyneices stands at the seventh gate, Eteocles resolves to meet him, setting the stage for their mutual death. The chorus reacts strongly to this killing of brother by brother. There is no *geras*, "no old age" for such a pollution (683). The language is suggestive: "No old age" means the pollution is always present; there is no growth, no generation, for this pollution is the denial of generation. The chorus reaffirms the focus on kinship ties, ties which may be in opposition to those created by the city. By denying women earlier in the play, Eteocles acknowledged only the bonds of the city, where all come from the earth and are governed by his reason. By claiming the earth as mother he avoids the complexity of multiple ties of relationship. Because Polyneices is attacking the land, the city, nurture in a common womb is ignored. Meanwhile, the chorus describes Eteocles as a man eager for a killing "not allowed" (694).

The women plead that he be persuaded by them, though he may loathe them (712). Eteocles allows the women to speak, "but briefly," he admonishes (713). They enjoin him, "Do not go to the Seventh Gate" (714), but though they repeat the warning about the stain of shedding his brother's blood, the words of women have no power over him. "You with speech (*logoi*) do not blunt the edge of the sharpened spear," he rebukes them (715). The chorus now sings of the self-killing (*autoktonos*) which will occur when the brothers meet (734). The chorus sees the bond between the two, but Eteocles, as leader of his city, will not allow such ties to muddy the clear distinctions between friend and foe. The simplicity of the definitions of the city cannot, for him, be undermined by the diversity of natural ties.

A messenger enters to report that the city fares well, but the brothers have been joined in the earth, in their common

grave. At this point Antigone and Ismene enter to mourn the deaths of their brothers; they describe the common sufferings of the family of Oedipus, this closest of all families, which Eteocles and Polyneices both wished to ignore. The unity which Antigone and Ismene in their mournful song affirm, however, is torn asunder at the final appearance of the messenger, who now reports the decree that will impose distinctions between the brothers. He says, "It is necessary for me to proceed to announce what has seemed best and was approved by the council of this city of Cadmus" (1005-1006). This is the formal language of the assembly. They have met and they have spoken. "It was decreed," the messenger reports, "to bury Eteocles for his loyalty to the land (*chthonos*) with the beloved tomb of the earth (*ge*)" (1008). The city also passed a decree concerning the corpse of Polyneices: it is to be thrown outside the city's walls, where, unburied, it will be fodder for the birds and dogs. This is the punishment for he who warred against the Cadmean land (*chthonos*). Through their speech (1020, 1025), the men of the Cadmean city have separated the brothers.

Antigone rejects this artificial distinction and announces that if no one else is willing to bury Polyneices, she herself will bury him, accepting whatever risk may come from burying her own brother (1026-29). She is not ashamed to disregard the speech of the city, for she is concerned with the community (*koinon*), the wondrous community (*deinon to koinon*) that exists between herself and her brother, who had grown (*pephukamen*) in the self-same womb, child of the same suffering mother and ill-starred father. She is eager to share (*koinonei*) his miseries (1033). In defiance of the city's sense of its own potency, she says, "Let it be decreed by no one (*me dokesato tini*) that hollowbellied wolves will eat his corpse" (1035-36). Against those man-made decrees she stands as a woman: "I,

although I am a woman, shall devise this" (1038). When the messenger warns that the city will be forced in these things, Antigone ignores, even mocks, his threats, and in her turn orders that he limit his speech (*me makragorei*) (1053). It is speech on which the city is based. She acts on the basis of bonds of kinship, not the bonds created by the words of assemblies.

The chorus of Theban women watching the interchange between Antigone and the messenger is torn in two directions, capturing the tension of the preceding action. Half of the chorus denies the decree and sides with Antigone. They acknowledge the problem in the justice of human decrees: it has no consistency over time, whereas the ties of the family appear natural and eternal, always to be respected. The other half of the chorus bends to the city's decree, accepting the unity between justice and the speech of the city, and thus the distinction between brothers that the city can impose. The play leaves us with no happy conclusion. Though Thebes still stands, the brothers are dead and the women are divided. Eteocles, fulfilling the curse of Oedipus, pollutes the city by shedding his brother's blood. Eteocles' misogyny, based on his rejection of what is other and his desire to see the world as simple and orderly, had been necessary for him to face his brother in battle. After his death the city, now dependent on its own reason, continues to deny the diversity of ties offered by women. Aeschylus's play ends unresolved, and we learn from Sophocles' *Antigone* that the tragedy continues.

Sophocles' *Antigone*

Sophocles wrote the *Antigone* partially in homage to the work of his predecessor, Aeschylus, for the play begins where *The Seven Against Thebes* leaves off.⁴ In Sophocles' version, though, it is not the counsel of elders, but Creon, brother to Jocasta and uncle to Oedipus's children,

who decrees that the body of Polyneices shall not be honored with the rites of burial. Sophocles' play begins as Antigone announces her plan to defy Creon's orders: She will bury the body of her brother and asks for Ismene's aid. Ismene resists, pleading with Antigone not to attempt the burial, for women are weak and have not the strength to fight against the decrees of the male in the city. Ismene equates speech and power. Antigone scorns such an equation. Neither Creon's speech nor his physical resources threaten her. She envisions forces which transcend the speech of the city. These are the gods of the dead, who stand as an affirmation of the limited strength of the political world that Ismene, with her focus on the present, fears.

The tragedy moves on inexorably as Antigone performs the burial rites, is caught, confronts Creon, and is sent to certain starvation in a cave outside the city. Choosing immediate death and union with those who are beloved to her—i.e., those who have died—she hangs herself. Creon's son Haemon, Antigone's betrothed, follows Antigone to her death, whereupon Creon's wife also commits suicide and Creon, who at the beginning of the day was the firm and certain ruler in a city just recovering from a traumatic war, is shattered; he has learned the importance of custom and respect for the gods that in his arrogance he had originally ignored by passing a decree denying the importance of kinship.

Near the beginning of the *Antigone* there is a powerful and justly famous choral ode in which the chorus of Theban elders sings about the "wonders" of man. The translation "wonders" does not, however, capture the tension of the term *deinos*, a word that entails terror as well as wonder. It is precisely this ambiguity which embodies the tragedy of the play, for the wonders of man include all the actions of man's intellect, whereby he has

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been able to conquer the natural world around him, but which in turn may destroy him.

Many the wondrous things, and none is more
wondrous than the human
who walks upon the earth . . .
And she, the greatest of gods, the earth—
ageless she is, and unwearied—he wears her away
as the plows go up and down from year to year . . .
He controls with craft the beasts of the open air,
walkers on hills . . .
Speech and windlike thought
and the feelings which are part of rule in the town
he has taught to himself.⁵

(332–56)

The portrait is of man the creator against nature. Only death has he been unable to conquer.

The choral ode's optimism about man's capacity to rule matches precisely Creon's vision of himself as the leader of Thebes. He views law as a human creation that springs forth from human speech. Throughout the play there are references to Creon's "orders." Antigone begins the tragedy by asking her sister whether she has heard the "announcement" (*kerugma*) (8; cf. 32, 192, 446). Ismene had accepted Creon's speech as law, and therefore as equivalent to power. Creon's son Haemon views the political as speech as well, asking his father to listen to what the people are saying as they whisper in the corners of the city. Only Antigone denies the efficacy of human speech, scornfully dismissing the spoken decrees of the city's leader. The laws she follows are worthy of respect precisely because they are unwritten and unspoken by men. As such they have always been, and were never the creation of the human intellect. Thus, they are similar to nature, which always exists, but which Creon in his sense of potency feels he can dismiss, and which the chorus praises man for conquering. The uncreated, unwritten laws of the gods stand as a counter to the spoken decrees of the city that Creon rules.

Antigone, though, in denying the efficacy of speech, denies any form of

creativity. Her name itself captures her stand: *anti-gone*, against birth. As Antigone becomes devoted to the world of the dead, unmoving and unvaried, she herself is transformed into a male in both the language she uses to describe herself and that used by Creon to refer to her (Pomeroy, 1975, p. 100). In her focus on what has always existed—i.e., on what cannot be created through human efforts and human speech—she fails to understand her own dependence on that which is other: the city and the male. She focuses on a nature that always is, that never comes into being or grows. She herself denies the prospect of marriage, unmoved by Ismene's pleas that she think of her forthcoming marriage to Haemon. Marriage entails creation; the piety Antigone espouses is an anti-life piety, and like the male Homeric heroes, she becomes the warrior whose glory can be achieved only at the moment of death, in the denial of life and of change.

Creon, in turn, thinks too much of creativity and power. Furthermore, his is a creativity of speech against nature rather than within nature, as abstracted from the creative powers of the family as Antigone's piety. Creon's assertion of male potency is set off by his pride in his masculinity, a masculinity he constantly feels is threatened by Antigone's resistance. She has denied his capacity to make and enforce decrees, to create and order the city. "I am not a male (*aner*), but she the male if she rules in this thing," he says (484–85); and later in his discussion with Haemon he asserts, "While I am alive no woman (*gune*) shall rule over me" (525; cf. 670). Throughout the play Creon and Antigone stand in opposition to one another. Their opposition brings on the tragedy, which would not occur were Antigone to yield to the pleas of her sister to allow male potency full expression of itself (in terms of both Creon's decree and the prospective marriage with Haemon), or were Creon's attempts at self-assertion

to yield to Antigone's denial of the capacity for human creativity. Antigone, rejecting creativity, relies only on what is, and thus must turn to death itself, while Creon, looking only at what comes into being, ignores—according to Antigone—the demands of what is.

While Antigone may alert the audience to unchanging laws which exist above the city, and to the paltry role of human speech, her presence must be supplemented by Ismene, who, while submitting herself to the power of the city, continues trying to remind both Antigone and Creon of the processes of birth—that is, of the dependence of both on the diversity of nature, which each wishes to deny. During the confrontations between herself and Antigone and between Antigone and Creon, Ismene asks Creon, "Will you kill the bride of your own son?" (568). Creon's vulgar response, "There are the arable fields of others," reveals his refusal to acknowledge the particularity of Antigone (Benardete, 1975, p. 23). For Creon, Antigone differs no more from other women than money-grubbing prophets differ from one another (1055). Creon's mind perceives uniformity and simplicity, while Ismene tries to remind him of the unique harmony between Haemon and Antigone (570). The two protagonists of the play, each defending opposing visions of certainty, destroy each other. Ismene preserves her own status as a female, standing between Antigone and Creon reminding them of marriage and family, yet unable to move the adamant will of either as each focuses on her or his own vision of the simple and uniform.

Into this world of conflict between Antigone and Creon, which neither Haemon nor Ismene is able to resolve, comes the seer Teiresias, one who knows the ways of the gods and interprets auguries and sacrifices for the leaders of Thebes. He is an intermediary between the gods and men. He also is an inter-

mediary between the male and the female, for upon killing the female of a pair of coupling snakes he was himself transformed into a woman for part of his life. Teiresias thus understands the perspective of both the male and the female, and his role as an intermediary between gods and humans is in part dependent on this double vision.

Unlike Antigone, Teiresias does not deny the value of the city. Rather, he assists in its guidance. When Teiresias first appears, Creon comments that previously he has never deviated from Teiresias's advice. Teiresias responds that therefore Creon has kept the ship of state upright (993-94). But unlike Creon, Teiresias is unwilling to depend entirely on human reason for such guidance. Human intelligence must accept diversity in the world—the gods of the dead as well as the gods of the living, the male as well as the female—and not attempt to transform that diversity into simplicity. Teiresias offers Creon the means of escape from the tragedy about to befall him, but Creon is arrogant in the power of his own intellect and capacity to understand human motivations: "The race of seers all love silver" (1055). Refusing to accept the advice and vision of the prophet, a male turned female and back again, Creon must endure the tragic destruction of his world, a destruction reason is unable to prevent.

The *Antigone* and *The Seven Against Thebes* suggest, in very different ways, how women stood as threats to the masculine image of potency in ancient Greece, reminding men of what they must escape in order to found and preserve the city: the fundamental diversity of nature, which did not yield easily to the imposition of rational simplicity. For Eteocles there was the chorus of Theban women. For Creon there is Antigone, though she herself may want the same simplicity as he, and thus stands as a worthy opponent. Nevertheless, she threatens Creon

with her status as different, as set apart from the city which should be, as Creon sees it, seamless. For Creon and Eteocles, the simplicity each desires depends on the denial of the female. Tragedy reveals that such denial is destructive of the polis, which could not survive without reproduction—the process of birth that depends on the commingling of opposites.

Let me now turn to the next century. The problem of women for the polity is always present, raising questions about male rationality and the attempt to create a city that does not depend on what is other. Both Plato and Aristotle try to respond to the threat of women and the tragedy that results from ignoring them.

The Philosophers' Response

Callipolis:

Socrates' Escape from Tragedy

The Socrates of Plato's *Republic* is famous—perhaps infamous—for his condemnation of poetry. In the metaphysical critique of poetry in Book X, Socrates describes the poet or artist as being three removes from what is real. In the moral critique of poetry in Books II and III, the gods are shown to be less than divine in their immoral lives. Behind both critiques is a rejection of poetry because it encourages men to see the multiplicity of the world of men and gods (Nichols, 1983a). In Book X Socrates the artist portrays specific—i.e., diverse—objects, rather than uniform or simple ideas—e.g., the idea (or form) of a chair. Artistic representations distract us from simplicity and make us focus on the particular. The critique in Book II is that the epic poets make the gods appear diverse rather than uniform and simple. For example, Socrates asks Adeimantus whether he supposes that "the god is a wizard able treacherously to reveal himself at different times in different *ideas*, at one time actually changing himself and passing his own

form into many shapes" (380d).⁶ They both conclude, "The god would least of all have many shapes" (381b). Similarly, the recitation of poetry must be excluded, for in the process of reciting poems or acting on stage, men may "undertake seriously to imitate in the presence of many . . . thunder, the noises of winds, hailstorms, axles and pulleys, the voices of trumpets . . . even the sound of dogs, sheep, and birds" (397a). Even laughter disappears, since it is a mode of changing from one form to another—i.e., an acknowledgment of diversity within the world and, worst of all, within a man himself (380c).

Art and the human expression of art is variable and diverse. In Socrates' Callipolis, as he calls this most perfect of all cities, there is to be no changing, no variability. No tragedies, no epics, no comedies will disturb the beautiful unity of the city. However, to achieve this condition of perfection—of completion and wholeness—of which his treatment of poetry is but one manifestation, it is necessary that Socrates conflate the situations of the male and female members of his perfect city. The differences between them must be ignored. Any differences that have defined male and female in the past are to be attributed to convention rather than nature. The natural world, in Socrates' understanding of it in this dialogue, pursues the uniform, not the diverse. It is the conventions of society which have accentuated differences.

This portrait of a natural world of uniformity rather than diversity is what Socrates tries to enforce when in Book V he suggests, "We'll suppose that our guardians and their women must practice the same things" (454d-e). The life of the female in the guardian class in Socrates' city is to be as little different from that of the male as possible. The activities surrounding the distinctive characteristics of the female body—namely, the processes of giving birth—are to be eliminated as

much as possible. Making this point, Socrates asks Glaucon, "Do we believe the females of the guardian dogs must guard the things the males guard along with them and hunt with them, and do the rest in common; or must they stay indoors as if they were incapacitated as a result of bearing and rearing the puppies, while the males work and have all the care of the flock?" (451d). The state of pregnancy is ignored and once the child is born to a woman, that child is placed in a pen along with other babies. The mother laden with milk will nurse a child, at intervals determined by her other activities, but she will not know her own child, nor will the care of the child be hers (460c-d). It is, as Glaucon remarks, "an easy-going kind of child-bearing for women guardians, as you tell it" (460d). The processes of birth appear as brief as the moment of conception—hardly to be noticed at all.

The consequences of this conflation of the male and the female in the guardian class are many (Elshtain, 1981, pp. 29-35; Okin, 1979, ch. 1-3), but for my purposes here we must see how it overcomes the ideas of the tragedies of the fifth century. The oppositions between male and female, physical and intellectual, nature and art, and the many and the one, are denied precisely by eliminating the family, the female, and the physical from the city. The tensions thus disappear. But by doing this Socrates creates a city which is a static moment in time. Like the gods he seeks to create in Book II, it has no motion, no capacity for change, and no capacity for self-regeneration. Callipolis is the creation of human speech, the speech of the dialogue engaged in by Socrates and his friends as they talk through the night in Cephalus's house. This speech has no relation to human bodies, which grow, change, give birth, and die—or even eat. The dinner promised (328a) is never served to these men, who devour only words (354a).

When Socrates tries to deal with the question of the regeneration of his city, all sorts of problems and internal contradictions arise. He tries to make prohibitions against incest, but as Aristotle would point out later, the prohibitions do not work. As Socrates goes on and on with his proposals for the proper modes of reproduction, Glaucon notes that the city's rulers must rely on a certain residual eroticism to make the plans work (458d). This is because they have previously eliminated any focus on the body, which could turn a guardian from the whole of which he is a part to an individual body with passions capable of erotic arousal. Furthermore, Socrates' claims concerning the equality of the male and the female are undermined when women are handed over as prizes for the men who are most valorous in battle.

In Book VIII Socrates traces the downfall of the monistic ideal he has envisioned through speech, in the prayers of men who are bound to the cave and to the fundamental variety of the natural world of physical beings (592a-b). The city's failure comes precisely from the incapacity to understand fully the process of regeneration—an incapacity to control, through mathematical knowledge, the seasons and the ways of sex. Because of their failure to combine adequately their mathematical knowledge with the exact movements of the seasons, the guardian rulers arrange for births not propitious for the preservation of the city; decay thus sets in (546a-d).

As the city declines from the aristocracy of Callipolis to the reign of a tyrant, women become distinct from men and their peculiar characteristics become more and more important. In the timocratic regime, the wife, having values different from those of her husband, complains to her son that she is not married to one of the rulers, and that because of her husband's lack of ambition she is at a disadvantage. Her son, she hopes, will do

better (549c-d). The point of complete deterioration is when the male has become the female in the description of the tyrant. He is one confined for the most part to his house, where "he lives like a woman, envying any of the other citizens who travel abroad" (597b-c). The circle is completed and we are back again at the conflation of male and female, only here it is the male ruler who becomes the female, not the female ruler who becomes the male. In this respect the similarity in Plato's work between the best, Callipolis, and the worst, tyranny, is striking, and perhaps suggests the inadequacies of both the attempt to impose the male view as the totality of existence and the attempt to make the female dominant. Both lack the capacity for regeneration, and thus both die ignoble deaths. Tyranny is for Socrates the end of the descending regimes. We must wait several centuries for Polybius to turn the decline into a cycle where degeneration can also lead to rebirth.

Socrates' city fails because men do not have the capacity to abstract from nature and make all simple. His desire to create in speech what is abstracted from the physical is evident in his attempt to destroy the boundaries between male and female. Socrates tries to escape the tragedy depicted by the playwrights by obliterating that which brings on tragedy, but he errs as the tragic heroes did by overemphasizing the efficacy of logos. The human body calls him back into the cave, to the tragic fall of his beautiful city to the petty issues of reproduction. The heroic city Socrates has created has a deathlike quality. There is no creativity, no art, no birth; it is a world in which neither male nor female exists, in which the masculine model of rational omnipotence has reigned to create a vision of monist simplicity from which variable poetry, among other things, must be excluded. In a sense, such a city calls forth its own tragedy, for it is a denial of itself.

Callipolis becomes a wasteland—a beautiful city which survives only in the speech of its creator.

**Aristotle:
The Failure of Hierarchy**

In the first six chapters of Book II of the *Politics*, Aristotle explicitly rejects Socrates' conflation of male and female, accusing him of turning his city into an individual and ignoring the impious consequences of his proposals (Dobbs, 1985; Saxonhouse, 1982). In contrast, Aristotle tries to deal with observed diversity in the world, not through denial, but through separation and hierarchy. Aristotle recognizes the relation between logos, mind, and the world of the senses, and thus acknowledges variety. He tries valiantly to resolve the problem of diversity by imposing hierarchy, rather than by ignoring or conflating differences. Yet because he is committed to a hierarchy that ensures the rule of the best, he leaves us dissatisfied with what hierarchy can achieve, for while it orders relations among humans, it is not always possible for humans to ensure that its ordering is just. Tragedy thus reappears, as Aristotle recognizes the limits of human reason and reveals the inadequacies of the political world. Tragedy on stage arose from men's discovery that they could not achieve the perfection of the gods. In Aristotle's politics the tragic surfaces not with the violence that we see on stage, but with the acknowledgment that the best is beyond human endeavors. Aristotle attempts to deal with this human failure to achieve divine status not with mournful laments or resignation, but through accepting the second best and acknowledging its limited satisfactions (Zuckert, 1983).

Behind Aristotle's political philosophy lie the twin principles of hierarchy and teleology. According to Aristotle both give order to the natural world. Teleology assures that, undisturbed, the natural

growth of animals and plants is in the direction of what is best—that is, toward the most complete expression of its particular form. This is the highest form a living creature can attain, in which, as Aristotle would say, it fulfills its nature and reaches its end: e.g., the colt becomes a horse and the acorn an oak. Motion is purposive. Related to this is hierarchy, which for Aristotle ensures the priority (the authority) of the better over the inferior. On the most basic level, Aristotle claims, this means the authority of the soul over the body (1.5.1254a31).⁷ If the soul does not rule, the individual lives a disordered life, in opposition to nature, a condition harmful to the individual (1.5.1254b5–8). Simply put, the mind must rule over the body or the body will not be fed. Hierarchy gives both meaning and a means of survival to the natural world, for it establishes what is best and allows what has come into being to continue to exist. This model is transferred in Aristotle's political work to the social relations within the community, where the better must rule over the inferior, and, according to his first assessment, the master should rule over the slave and the male over the female.

Aristotle's claim concerning the inferiority of the female is based on a variety of assumptions, and is derived from his discussion of the female in his biological works. The female of any species comes into being as a defect of nature, the result of the absence of adequate heat at the moment of conception. Thus, those eager for sons are well advised to conceive when the wind is not blowing from the north, or when the couple is at the height of passion (*Generation of Animals*, 2.1.732a6–7; 3.1.765b10ff., 766a30ff.; 3.2.767b8ff.). Yet while the female may be a defect of nature, she is necessary to keep the species in existence, and Aristotle acknowledges the mutuality of the sexes in the process of production, an acknowledgment: Ete-

ocles wished to deny and Socrates to minimize. However, though she may be necessary, she is nevertheless defective, and consequently, in a hierarchical world must be under the authority of the male, who is not defective. The principles of teleology and hierarchy are for Aristotle clear. It is the application of these principles to the world around us which ensures, according to Aristotle, an orderly society in which the inferior accepts the authority of the better, and each individual moves towards that end prescribed by his, her, or its nature.

However, Aristotle was an observer, one who did not wish to deny the power of sight. "We see," begins the discussion of the *Politics*. To write the *Politics* he studied 150 constitutions and their histories. They revealed a world that is not orderly, a world often convulsed by revolutions and political conflicts, as Book V of the *Politics* so vividly records. These conflicts arise because the criteria for determining the hierarchy of better and worse have never been carefully articulated, and men disagree. The criteria, as Aristotle understands them, must refer to what is unseen, what is in the soul. To place those who are better in positions of authority over those who are worse, we must know who is better and who is worse. How are we to do this? Political systems establish such criteria through speech—wealth, birth from citizen parents, education at certain universities or colleges, membership in a certain religious group. But these criteria are external and not based on Aristotle's concept of a natural hierarchy. While hierarchy is Aristotle's attempt to deal with diversity, he sees a fundamental problem with the concept, precisely because our sight does not always reveal who is better and who is worse.

Aristotle reveals the problems with this principle of hierarchy as applied to the political community in Book I of the *Politics*, almost immediately after he has

presented it. He turns from generalizations about the growth of a polis to a discussion of the parts of the household, and particularly the master/slave relationship. There are two kinds of slavery: one is conventional, the other according to nature (1.3-5). Conventional slavery, having no basis in nature, is founded only on the principle of conquest, which for Aristotle has nothing to do with better and worse (1.6.1155a25-32). Only corrupt societies—those not based on nature—use conquest as grounds for enslaving people and keeping their children as slaves. Though most slaves are the result of such conquests, this is not, in Aristotle's understanding, sufficient justification for slavery. In a corrupt society such as Athens we find slaves according to convention, but not according to nature—that is, men who are not slaves by nature, but who because of particular circumstances are kept as slaves. The problem is that we cannot see or know the soul, that which, more than the body, defines the natural slave. "It is not entirely easy," Aristotle remarks with due reserve, "to see the beauty of the soul as of the body" (1.5.1255a1).

Though the rule of the worse over the better is not likely to occur in the relations between the male and the female, where the differences between bodies are more apparent than between the bodies of the natural ruler and the natural slave, on occasion a superior female is subject to an inferior male. Though according to Aristotle this is against nature, it can happen. Aristotle does not state that all men are better than all women, only that this is natural; yet, he argues, we cannot be assured that nature is in control at all times. Nature does not always arrange that the child of a slave is slavish, nor that the soul of the female is always inferior to that of the male. To treat all those who live in conquered cities as slaves or all those born female as lacking in sense is to fail to recognize the diversity of nature.

The classic example of this is the story of Tecmessa, the wife of Ajax. In Sophocles' play the *Ajax*, when Tecmessa tries to calm Ajax down, urging him not to put on his armor in his present state of rage, she is told by her husband that silence is beautiful in women. Ajax in his madness proceeds to kill all the cattle of the Greeks, thinking that he is killing the Greek heroes themselves. The attempt to silence Tecmessa was a sign of Ajax's failure to see the wisdom in her soul, which outshone his own, though clearly his body—that of the great warrior—was more beautiful than Tecmessa's defective (because female) body (Nichols, 1983b, pp. 181-82).

The problem for Aristotle, the scientific observer of animals and constitutions, known for his empiricism, is precisely the limits of observation; namely, that we cannot see the soul, though this is where we must look if we are to understand goodness. Observable criteria such as wealth, birth, or sex are used by polities to establish worth and hierarchical order. But hierarchy in the city is according to convention, not nature, and thus, while it may be convenient for ordering the social world, is not best.

Plato's Socrates, along with Creon and Eteocles, assumed the capacity of the logos to overcome the problems presented to them by the sexual dimorphism of the human species. The male heroes of the tragedies tried to deal with women by denying them, ignoring their demands and assuming the priority of the creative logos. Socrates tried to eliminate the female by fusing the male and the female. Aristotle accepted the existence of the female, and offered a theoretical construction, hierarchy, that could incorporate in the social structure the diversity of natural and human forms. However, in his investigation of the problem he found that the theory, the logos, resisted application because of the limits of human observation. Sight, which had told

Aristotle so much about the workings of the natural world, was of no use in elaborating accurate criteria of virtue, which could not be seen.

In Book III of the *Politics* Aristotle leads us away from problems of sexual differentiation, which had been critical throughout the first two books, and into the world of citizens, in which the polity makes men equal. He eschews the permanent hierarchies he had tried to find and justify in nature. Yet always behind Aristotle's theories of democracy and oligarchy, stability and revolution, are the questions he has raised about slaves, the subordination of women, and what we can really know about the better, the worse, and the foundations of justice. The artificial equality of citizens in a polity is a practical solution, but one which means the failure of Aristotle's theory, for such equality denies what he would understand as the rule of the best.

Conclusion

Early Greek philosophy of the sixth and early fifth centuries confronted the question of unity and diversity. The philosophers saw a world of variety—a world of animals, humans, plants, stones—beneath which they searched for a source that could unify it all. Some of the early philosophers turned to an underlying substance such as fire, water, or air, explaining that the diversity we see around us derives from the varied forms of that one fundamental element.

This understanding of the world forced these so-called nature philosophers in many cases to deny their senses. The senses did not offer us knowledge of the fundamental elements at the base of the world we observed. The senses perceived a world that was constantly changing. In contrast, the mind was able to look behind the diversity and find unity and order (Snell, 1960, ch. 7). The mind, indeed, was able to show the fallibility of

the senses, as in the famous paradoxes of Zeno: An arrow, for example, could never reach a target. In order to do so it would have to travel half the distance to the target, but since there are an infinite number of halves, which the arrow could never cross, it never reaches the target. And yet, the eyes see the arrow hit the target, a fact which the mind knows is impossible, because it knows the arrow can never reach the end of infinity. Therefore, according to the logic of the time, the senses are unreliable and fallible. The eyes see motion and change, whereas in fact there is none.

In the conflict between the mind and the senses, the mind was declared the winner. The males of fifth-century tragedy also declared the mind the winner. They sought simplicity and unity as they ruled over the city. They hoped not to need to deal with a world which might turn them away from the priority and unity of the city, but in so doing they had to deny women, difference, and human reproduction, and thus act much as the philosophers who said that the arrow could not hit the target. Socrates, in *The Republic*, also tried to deny the senses in his abstraction from body and consequent willingness to equate the male and the female. Only Aristotle tried to retain a vision of the world as multiple, and to be valued precisely for its variability. His vision leads, however, to a hierarchical structure that must fail when applied to political life, since for him, as for the other Greek philosophers, the unseen becomes more important than the seen. Again, the tragedy comes from the inadequacy of the human intellect, which cannot transform what is variable and multiple into simplicity, and can neither demand complete silence of women who fear an invading army nor transform the female into the male, as Socrates tries to do. In their diverse roles throughout the corpus of Greek literature, women give the lie to the male's dangerous and tragic love of his

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own imagined potency, creativity, and intellect, and reveal the potential limits of the masculine political perspectives that we have inherited from the Greeks.

Notes

An earlier version of this paper was presented at the 1985 annual meeting of the Western Political Science Association, as well as at Carleton, Reed, Rutgers, the University of California at San Diego, and the University of Washington.

1. The selection of plays has been determined in part by the desire to study carefully plays I have not discussed before (cf. Saxonhouse, 1980, 1984). *The Seven Against Thebes* will receive more extensive treatment, since political scientists are likely to be less familiar with it than with the *Antigone*. For other treatments of the Greek plays also focusing on the significance of the female, though not always from the same perspective as mine, see Euben (1982), Foley (1981), Pomeroy (1975, pp. 97-119), and Zeitlin (1978).

2. The ideas in this section owe much to Benardete (1967), and Orwin (1980).

3. The numbers in the parentheses here and throughout the discussion of the plays refer to the lines of the Greek texts. The Oxford Classical Texts are used for all Greek authors, unless otherwise indicated. Unless otherwise noted, all translations are my own.

4. Particularly helpful in this section were the discussions of the *Antigone* in Knox (1964), and Benardete (1975). Other readings of this tragedy that focus on its significance for those concerned with women in political theory include Elshtain (1982) and the critique of Elshtain by Deitz (1985, pp. 26-30).

5. Apart from the first and last sentences, the translation is that of Elizabeth Wychoff in Grene and Lattimore (1960). See also Segal (1964) for a full discussion of this passage.

6. I cite Plato according to standard Stephanus pagination. I have used Bloom's translation.

7. I cite Aristotle according to the standard reference form, including book and chapter. Unless otherwise noted, all references are to the *Politics* and all translations are my own.

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FOUCAULT'S CHALLENGE TO CRITICAL THEORY

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Power, subjectivity, otherness, and modernity are concepts that contemporary political theorists increasingly find to be closely interwoven. In search of an adequate comprehension of the interrelationships among these concepts, I examine the work of Michel Foucault and Jürgen Habermas. I argue that Foucault, although he is provocatively insightful on a number of key points, ultimately provides a less satisfactory account than Habermas. The core problem is Foucault's inability to conceptualize juridical subjectivity, something which is necessary if he is going to connect his notion of aesthetic subjectivity with his endorsement of new social movements.

In the last few years, philosophers and political theorists, particularly those on the Continent, have become increasingly occupied with the interrelationships among the concepts of power, subjectivity, otherness, and modernity (Dallmayr, 1981, 1984b). The key issues involved in this nexus of concepts can be brought into sharp relief if one compares the work of Jürgen Habermas with that of Michel Foucault. My intention in this paper is to consider which theorist offers the most adequate overall perspective on power, subjectivity, otherness, and modernity. I will argue that although Foucault brings a number of significant insights to critical theory, his analysis is ultimately less persuasive than Habermas's. More specifically, Habermas's work provides a framework that can incorporate many of Foucault's key ideas without at the same time leading us into a conceptual cul de sac involving subjectivity.

Before turning to this discussion, a few general remarks may be useful in regard to why the four concepts under analysis demand the attention of those interested

in the study of politics. No one of course would question the centrality of power to politics, but its connections to the other concepts may not be immediately apparent. Subjectivity, as it will be used here, refers to an account of the human subject or agent, usually developed in terms of concepts such as rationality, intentionality, responsibility, mutuality, interest, etc. Now most political theorists would subscribe to the proposition that any conception of power must in turn presuppose, at least implicitly, some account of subjectivity. And yet, if one admits the truth of this proposition, there remains a real problem with subjectivity to which Foucault and others have drawn our attention: Are the conceptions of subjectivity dominant in the modern world not themselves phenomena shot through with power (Foucault, 1977)? In other words, are the notions associated with the modern subject and used in our constructions of conceptions of power not themselves already an effect of power? If so, a political theory that moves from such notions of the subject to a conception of power may have rendered itself blind to a

whole range of inconspicuous but significant constraints specific to modern life. Social and political life in modern society enforce a whole range of constraints that are embodied in our views about what is rational and normal for subjects. Outside of this range lies the sphere of "otherness"—that which is excluded and devalued. This exclusion and devaluation may operate intersubjectively, that is, upon other individuals or groups whose forms of life are sufficiently different from our own; or it may operate intrasubjectively, that is, upon the body or dimensions of psychological life that threaten the smooth functioning of the normal, rational subject in modern society. In either case, these operations are fraught with political implications.

Interpretation, Power, and History

One way of locating Foucault's work is to see it as occupying an important position in that contemporary philosophical movement often referred to as "textualism" (Rorty, 1982, pp. 139-59; Shapiro, 1984, pp. 387-91). The texts referred to here can be literary or social. The key idea is that the interpretation of a text can no longer be understood as the excavation of its "real" meaning. For social and political theory this implies, for example, that it is a self-deception to see the real meaning of Western modernity as the slow struggle of humanistic subjects to construct institutions of liberty. Such a Whiggish view of modernity as the rise of the liberal, juridical subject is merely one of an infinite number of possible interpretations (Rorty, 1979, p. 341).

Freeing up our thinking about interpretation also means calling attention to the ultimately arbitrary quality of all interpretation, and to call attention to this is immediately to raise the question, Why does one or another interpretation predominate at a given time and place? The

textualist answer to this question draws one almost inexorably to the phenomenon of power. One interpretation prevails over another not because of some rational superiority, but because it reflects a certain relation of power. Thus, textualism is often characterized by a "hyper-politicizing consciousness" (Shapiro, 1984, p. 391).

Probably more than anyone else, Foucault (1980a, pp. 198ff.) has attempted to elaborate a theory of power which would be compatible with textualism (see also Shapiro, 1984, p. 391). The connection between the two is made patently clear in his work. Foucault refers to the "development of humanity" as "a series of interpretations." For him, interpretation is

the violent or surreptitious appropriation of a system of rules, which in itself has no essential meaning, in order to impose a direction, to bend it to a new will, to force its participation in a different game.

In sum,

Humanity does not gradually progress from combat to combat until it arrives at universal reciprocity, where the rule of law finally replaces warfare; [rather] humanity installs each of its violences in a system of rules and thus proceeds from domination to domination. (Foucault, 1984a, pp. 85-86)

This perspective is perhaps best illustrated in Foucault's study of punishment and prisons since the eighteenth century. What he finds there is not the story of the slow struggle of Whiggish subjects to change barbaric institutions into those more appropriate to free men. On the contrary, he finds merely the displacement of one form of penal discourse and practice by a new, modern one, in which power simply manifests itself in a less conspicuous form. Modern power does not reveal itself spectacularly in the form of the executioner and bodily dismemberment. Rather, modern power operates in conjunction with the human sciences, and proceeds by means of the continual and

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authoritative categorization, grading, and monitoring of human behavior. The discourse of the human sciences creates an expanding net of permissions and exclusions, which categorize increasingly large areas of human behavior into normal and abnormal. The expansion of this new mode of "power/knowledge" is the hallmark of modernity. Foucault (1977, pp. 209, 216, 218ff., 296ff.), in his typically arresting way, wants us to consider the implications of living in an increasingly "disciplinary" or "carceral" society.

Thus, for Foucault, social texts are shot through with power, and modern society with a particularly insidious form. Correspondingly, subjectivity is not to be understood as something behind events and discursive practices, pushing them forward, as it were. Subjectivity is rather something constructed or constituted within such practices. Subjects are effects of strategies of power, and strategies of power are strategies "without a subject" (Foucault, 1980a, p. 204; 1977, pp. 29, 104). Given these claims, the task of the social theorist becomes one of making clear the different ways in which power relations arise through discursive practices, and the different ways in which subjectivity is thereby constituted at different periods in history. These are the ends which Foucault's archeological-genealogical method is designed to serve (Dreyfus and Rabinow, 1983, p. xxv; Poster, 1984, p. 89).

Foucault's Challenge

What makes Foucault's work so challenging to critical theory? At bottom, it is the fact that his method, like Nietzsche's, discovers power operating in structures of thinking and behavior that previously seemed to be devoid of power relations. In effect, Foucault provides us with an incisive way of interrogating the structures of culture. His specific targets are

the cognitive and institutional structures of modern life. He wants to jolt us out of our self-assured assessment of these structures as necessarily superior and uniquely privileged. He wants to show us that structures we take to be thoroughly enabling are always simultaneously constraining. This orienting intention of all of Foucault's work is clearly expressed in the following: "It seems to me that the critical question today [is:] in what is given to us as universal, necessary, obligatory, what place is occupied by whatever is singular, contingent and the product of arbitrary constraints?" (1984b, p. 45).

This question and Foucault's method of analysis command the attention of critical theory because the latter has always claimed to be on its guard against mistaking new forms of mystification for enlightenment and new forms of subjugation for emancipation. There is thus much in Foucault's work which can give fresh impetus to critical social analysis (Poster, 1984). However, the totalistic quality of Foucault's position—history as the "endlessly repeated play of dominations"—constitutes a sharp break with the Frankfurt tradition of critical theory.

The ambivalence of critical theory in relation to Foucault is captured succinctly by Habermas (1979a): critical theory must try "to formulate an idea of progress that is subtle and resilient enough not to let itself be blinded by the mere appearance [*Schein*] of emancipation. One thing, of course, it must oppose: the thesis that emancipation itself mystifies."¹ "Emancipation" here has the juridical sense of a society in which there is a just arrangement of freedom (Habermas, 1979a, pp. 56–59). Hence, Habermas is saying that critical theory cannot give up the idea of more enabling political structures, nor the inherently related idea of juridical subjectivity—since the concept of politically enabling structures only has sense when one can say for whom those structures are more enabling. Furthermore, Habermas's

notion of the possibility of emancipation, as well as his account of subjectivity, are inextricably tied to the emergence of certain modern structures of consciousness.

Here the lines of controversy between Foucault and Habermas emerge clearly. The latter cannot accept the former's totalistic critique of either modernity in general or modern notions of juridical subjectivity in particular. Foucault debunks the latter, Whiggish notion, and takes the theoretical position of a detached observer in regard to the spreading networks of normalization in modern life.

In *The Theory of Communicative Action*, Habermas (1981a, 1984a) lays out his own conception of modernity. Although Foucault is not mentioned, the conception which emerges from this work engages Foucault's work at a number of key points. The reason for this is that Habermas is explicitly attempting to find a way out of the dilemmas of Horkheimer and Adorno's (1972) own version of a totalistic critique in the *Dialectic of Enlightenment*.

Habermas pursues this goal by developing a two-level interpretation of modernity. On the one hand, he presents a critique of the social processes of modernization. Extending and modifying the project originally envisioned in his *Legitimation Crisis* (1975), Habermas (1981a, pt. 4) sees in modernization a growing "colonization of the lifeworld" by the imperatives of the administrative-political and economic subsystems. For present purposes, the specifics of this argument are not important. The reason is that there are strong similarities between Habermas's idea of the "colonization of the lifeworld" and Foucault's idea of increasing normalization (Habermas, 1985, pp. 420-21; White, 1985, ch. 5). The key source of controversy thus arises not at this level, but at the second level of Habermas's analysis of modernity.

Basically, Habermas argues that the

phenomenon of *modernization* must be separated from certain more basic characteristics of "modern structures of consciousness" that together constitute "cultural *modernity*" (*italics mine*). Rather than pursue a totalistic critique that tries to establish its critical standpoint "above" or "outside" of modernity, Habermas uses this second level of analysis to secure the standpoint from which modernization processes can be critically assessed. In short, he argues that "cultural modernity" establishes a "potential for reason" that has not been given full play, but rather has been utilized in a one-sided way in the process of Western modernization (Habermas, 1981a; 1984a, ch. 2, pt. 1, and p. 221).

It is this analysis of the underdeveloped potential of modern structures of consciousness that provides Habermas with a way of approaching the problem of modern subjectivity in general and juridical subjectivity in particular. It is from this potential, which is always present in the linguistic interaction of modern subjects, that he develops the idea of a minimal, communicative ethics. The resulting framework gives Habermas a distinctive way of reading the significance of new forms of social and political opposition. He sees movements among women, radical ecologists, antinuclear activists, gays, and countercultural types as functioning, at least partially, outside the normal parameters of political give-and-take in contemporary capitalism. In his terms, they are fighting a protracted "border conflict" (*Grenzkonflikt*) along those points in modern society where the colonization process is pressing forward (Habermas, 1981a, pp. 575-86; 1985, p. 423). In addition to this distinctive way of conceptualizing what such movements have in common, Habermas's model provides a certain normative orientation to these forms of collective action. In short, he is able to locate new social movements in relation to modern power phenomena

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and the possibility of expanding freedom.

Interestingly enough, Foucault (1984b, pp. 46-47) also emphasizes the significance of such new social movements. He sees them as types of authentic resistance to modern forms of power. Thus, Foucault and Habermas both take a clear step away from Marxian notions of a single collective subject whose role is to reorganize thoroughly the social totality. Both are seeking theoretical modes that show such global projections to be fraught with danger (Foucault, 1980b, pp. 80-81; Habermas, 1985, ch. 12).

However, Foucault's endorsement of new modes of collective resistance raises a serious problem for his work, as a number of his critics have pointed out (Poster, 1984, pp. 4, 148; Taylor, 1984). To single out and endorse such movements means that Foucault must move out of the detached perspective assumed in the archeological-genealogical analysis of fields of behavior and into the analysis of situations of action. But to adopt an action-theoretical perspective requires one to speak of subjects of action in a way that cannot be grasped in archeological-genealogical categories.

Foucault's dilemma is that he wants to explicate the significance of and normatively endorse "new kinds of subjectivity" and collective action (1983, p. 216). But such a discourse is highly problematic for him both normatively and methodologically. Foucault's own theory seems to tell us that such discourse about action can have no other status than that of another act in the "endlessly repeated play of domination." This raises the problem of how his discourse about new social movements can recommend itself to us in a normative sense. Or, to put the problem slightly differently, if Foucault is recommending a particular conception of subjectivity and resistance, is he not violating his own basic methodological canon?—Thou shall not privilege any interpretation (Dreyfus and Rabinow, 1983, pp.

257-58; Frazier, 1983, p. 64ff.; Poster, 1984, p. 4).

A New Kind of Subjectivity

Foucault was notoriously reluctant to engage in extensive speculation about a new kind of subjectivity. Yet in some of his last work, the outlines of such a conception do appear. In examining this view of subjectivity, I want to focus primarily on whether or not it provides a conceptual basis adequate to justify his endorsement of new social movements.

Basically, Foucault offers an account of an *aesthetic* subject, whose character is extraordinarily interesting in the sense of highlighting some aspects of subjectivity that have been systematically excluded and devalued in the modern world in favor of cognitive and juridical aspects.² However, I will argue that Foucault's aesthetic account is flawed for at least one reason: It provides no resources for moving from aesthetic criteria to juridical criteria; or, to be more precise, it does not provide the sort of criteria one would have to have in order to distinguish clearly just those sorts of movements he wants to endorse.

The point at which to start thinking about subjectivity in Foucault is where he speaks of the resistance to the process of normalization offered by "bodies and pleasures," as well as by the "*insurrection of subjugated knowledges*" (1980c, pp. 155-59; 1980b, p. 81). Although for a long time Foucault refused to flesh out these somewhat cryptic remarks, they nevertheless gave the reader of his genealogies of the penal system and sexuality the haunting sense of a lost potential of human being in the modern world. The presence of this sense gave those genealogies their intense moral undertone. However much Foucault wished for his analysis to be that of a detached observer, one cannot help hearing the voice of a subject deeply injured

by the social processes under observation. In fact, the reader of Foucault cannot help sensing the implicit presence of a distinctive subject any more than the reader of Rousseau's *Discourse on the Arts and Sciences* (1964). Both draw our attention to disturbing unlearning processes that are being driven forward in modern ways of life. And for Foucault, that process is one of the subjugation of the body and aesthetic-expressive capacities that is enforced by our modern fixation on cognitive and juridical subjectivity. In short, Foucault allows us to glimpse in an arresting manner the degree to which the predominant ways of conceptualizing subjectivity in modernity close off access to the subject's own pre-rational, embodied otherness.

Foucault's shadow subject emerges, as I have said, in some of his last work. There he begins to articulate a model of a reflective self that is more open to the body and aesthetic pleasure. This subject is one who is oriented not around making the body and its needs the object of knowledge or juridical regulation, but rather the object of an aesthetic attitude. Foucault finds some elements of such a conception of the subject among the early Stoics, who began to distance themselves from the classical Greek *techne* (system) of life focused on the city and to envision a different sort of *techne*—one focused on "the *bios* as a material for an aesthetic piece of art"—in short, "an aesthetics of existence." In addition to this aesthetic focus on the *bios* (individual course of life), Foucault is also attracted to the associated early Stoic idea of a "strong structure of existence without any relation with the juridical per se, with an authoritarian system, with a disciplinary structure" (1984d, p. 348).³

This theme of an aesthetics of everyday life finds a nineteenth-century counterpart, according to Foucault, in Charles Baudelaire's idea of "*dandyism*." The dandy is the individual "who makes his

body, his behavior, his feelings and passions, his very existence a work of art." And again, this elaboration of the self is produced exclusively by attention to the body, pleasures, and aesthetic form, not by attention to the social or political realms (Foucault, 1984b, pp. 41–42).

Foucault thus provides us with hints toward a different way of thinking about subjectivity, one which is constituted so as to give a central place to the body, spontaneity, and expressiveness. What remains unclear is how he can bridge the gap between this aesthetic subject and his endorsement of new forms of collective action, particularly given the explicit distancing of his notion of subjectivity from whatever is juridical, that is, intersubjectively normative.

Juridical and Aesthetic Subjectivity in Habermas

Before confronting Foucault with this question it is important to try reversing it, confronting Habermas with the resulting query: Does his approach to subjectivity, which emphasizes juridical aspects, offer any link to aesthetic subjectivity? If Foucault is right about the exclusion and devaluation of the body and aesthetic-expressive capacities, then critical theory is obliged to press this question upon itself. For, as I said, critical theory must continually hold itself open to the possibility that its own concepts make it blind to some dimensions of power and self-deception.

In order to show what place the aesthetic dimension has in Habermas's thought, it is necessary to look very briefly at how he treats the whole notion of subjectivity and its relation to modernity. Habermas's model of the subject emerges out of his attempt to reconstruct the basic presuppositions of the ongoing linguistic interaction of everyday life. What is reconstructed is an actor's "com-

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municative competence," the know-how he or she manifests in speaking and acting. Habermas claims that ongoing linguistic interaction, or "communicative action," presupposes an actor's having the ability to raise and be accountable for redeeming a differentiated, universal set of validity claims. These three claims are truth, normative rightness or legitimacy, and truthfulness or authenticity; and, corresponding to them, there are three general pragmatic functions of language: cognitive, interactive, and aesthetic-expressive (Habermas, 1979b, pp. 50ff.; 1984a, pt. 1, ch. 3).

Habermas's project has, of course, hardly been immune from criticisms (Held and Thompson, 1982). For present purposes, however, I am going to leave these aside and focus on a couple of the implications of this project. First of all, the analysis of communicative action serves as the framework for thinking about the normative or juridical aspects of subjectivity. More specifically, the idea of a mutual accountability between speakers and a possible unconstrained consensus between them provides Habermas with a way of interpreting equality and the mutual recognition of subjects in situations of normative conflict. From this basis for juridical subjectivity, Habermas also gains his minimal ethical perspective on politics in general and new social movements in particular.⁴

Secondly, it is the notion of a basic set of differentiated validity claims that provides Habermas with the key to his two-level interpretation of modernity. He interprets the shift from magical-mythical societies to modern ones as a process in which these claims not only become clearly differentiated in everyday life, but also become institutionalized in respective, relatively autonomous "cultural spheres of value." These are constituted by the practices and institutions of science and technology, modern law and post-conventional morality, and modern art

and aesthetic criticism (Habermas, 1984a, pt. 1, ch. 2; pt. 2, ch. 1, esp. pp. 165ff.).

It is this differentiated and reflective quality of "modern structures of consciousness" that Habermas wants to emphasize as the hallmark of "cultural modernity." These qualities give modernity its unique and privileged potential for learning. Clearly this claim of superiority brings with it a heavy burden of proof and a strong suspicion of ethnocentrism (Bernstein, 1985). For present purposes, what is distinctive about this claim is that Habermas wants to assert that learning processes go on not only in the scientific-technological sphere, but also in the moral-practical and the aesthetic-expressive spheres. In fact, it is cultural modernity's three-fold potential for learning that provides the critical perspective from which one can begin to speak of a "one-sided," scientific-technological manifestation of this potential in the social processes of modernization. In short, Habermas is arguing that the resources of cultural modernity are adequate for illuminating the pathologies and unlearning that have accompanied modernization (1984a, pp. 183, 221, 232, 239-40). The upshot of this line of thought is that the critique of modern society can be adequately pursued within the parameters of modern consciousness. One does not need, and in fact cannot make sense of, notions of a radically post-modern critical standpoint.

The foregoing synopsis of Habermas's position on juridical subjectivity and modernity allows one now to locate more precisely the place of the aesthetic-expressive dimension in his work. My point of entry here will be to ask, Exactly what sort of learning process is manifested within the aesthetic dimension? And in what sense is this learning process one which is particularly sensitive to what has been excluded, devalued, marginalized—in short, "unlearned"—in modern life? This line of inquiry should keep

the controversy between Habermas and Foucault in sharp focus.

One can begin to answer these questions by examining what Habermas means by authentic self-expression; that is, what is claimed in the expressive use of language. An authentic self-expression is one which claims to express a speaker's real or true needs. Now Habermas offers us no philosophical device by which "true" needs can be unequivocally separated from "false" needs for any given person (White, 1985, chs. 3-4). One can only examine ongoing behavior for clues as to whether someone has truthful interpretations of his own needs or not. Clues would be constituted by evidence that, say, a person's need interpretations are or are not self-debilitating or self-deceptive. However, such substantive criteria are notoriously difficult to apply and they involve a substantial assumption of authority on the part of the observer. There is nevertheless a procedural criterion which, while hardly unambiguous, also gives some basis for evaluating the authenticity claim. As Habermas (1984a, p. 20) says, "We call a person rational . . . if he can adopt a reflective attitude to the . . . [cultural] value standards through which desires and feelings [*Bedürfnisnatur*] are interpreted."

Now how does this notion of truthful or authentic self-expression connect with the aesthetic dimension and some sort of associated learning process? One way to imagine such a learning process is to think of a progressive accumulation of epistemic contents in works of art, the way such contents are said to accumulate in scientific theories. Habermas, however, suggests something different:

What accumulates are not epistemic contents, but rather the effects of the inner logical differentiations of special sorts of experience: precisely those aesthetic experiences of which only a decentered, unbound subjectivity is capable. (Habermas, 1984b, pp. 235-36)

In order to understand what Habermas

is getting at here, one must understand his interpretation of what is implied in the process by which the aesthetic dimension became an autonomous cultural sphere in the modern world. Already in the eighteenth century, idealistic aesthetics began the separation of "the beautiful and the sublime, on the one hand, from the useful and the desirable, on the other." In the course of the nineteenth century there emerged out of this romantic spirit the radicalized consciousness of "aesthetic modernity." Like Foucault, Habermas sees the work of Baudelaire as the first clear manifestation of this consciousness. Baudelaire was the first to radicalize the distinctiveness of aesthetic experience, divorcing it completely and unequivocally from the claims of tradition, morality, and society. The distinctly modern aesthetic experience is thus seen as emerging when "the categories of the patterned expectations of organized daily experience collapse, . . . the routines of daily action and conventions of ordinary life are destroyed, and the normality of foreseeable and accountable certainties are suspended." Implied in such experience is an "unbounding of subjectivity," a willingness to transgress the normal, a playfulness, imaginativeness, and inventiveness (Habermas, 1984b, pp. 235-36; 1981b, pp. 4-5; 1982, p. 25).

This distinctiveness of modern aesthetic experience, this radical questioning of everything, even other shibboleths of modernity, both cognitive and juridical, must nevertheless be recognized as itself dependent upon modern structures of consciousness in general. What ties the ethos of "aesthetic modernity" to "cultural modernity" as a whole is that the "reflective experiences of an 'unbound' subjectivity" partake of that general experimental or "hypothetical approach" to phenomena and experiences that modern structures of consciousness in general first make possible (Habermas, 1984b, p. 240). A radicalized aesthetic

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consciousness is thus, like scientific thinking and post-conventional ethical consciousness, something socially available only to the modern subject.⁵ This point must always be kept firmly in mind whenever an appeal to the aesthetic sense is used by totalist critics of modernity.⁶ Briefly, it means that the aesthetic sense cannot be employed as the basis of a critical perspective that claims somehow to observe modernity's problems from a detached perspective.

Turning back now to the original question of learning, what exactly is there in the modern aesthetic experience that allows the accumulation of some sort of insight? Apparently, the key is greater fluidity and flexibility in modes of access to our desires and feelings. The radical decoupling of the aesthetic sense from the imperatives of society and tradition has the potential for informing consciousness about how we normally interpret our desires and feelings in ways that unreflectively mirror the prevailing value standards of the culture around us. The modern aesthetic sense thus has a unique potential to make subjects more reflective in relation to who and what is actually structuring the interpretation of their *Bedürfnisnatur*.

This aspect of insight, while important for Habermas, would hardly be adequate for someone like Foucault. For here one finds the subject engaged in a discipline of self-interrogation, and it is just such a procedure that Foucault is always wary of. He is at his best in exposing the darker side of modes of self-discipline. In this sense, one can be justly suspicious of the compulsive implications of some of Habermas's (1973, p. 252) early remarks about making the self progressively more "transparent." However, this ambivalent aspect of reflection is only one side of what is made available in the modern aesthetic experience. The other side conforms less to a model of interrogation and more to one of unexpected discovery of

things exceeding the confines of any rational, methodical, self-disciplining interrogatory framework. It is this latter sort of insight that has at least the potential for generating more sensitivity to the subordination of the pre-rational and embodied aspects of human being. One common characteristic of the modern aesthetic consciousness from Baudelaire to the avant-garde in the twentieth century is its inextricable involvement with the shocking, the unexpected, the unknown—with what shatters normal patterns of seeing, thinking, and feeling (Habermas, 1981b, pp. 4–5). A central feature of this consciousness is thus its

increased sensitivity to what remains unassimilated in the interpretive achievements of the pragmatic, epistemic, and moral mastery of the demands and challenges of everyday situations; it effects an openness to the expurgated elements of the unconscious, the fantastic, and the mad, the material and the bodily, thus to everything in our speechless contact with reality which is fleeting, so contingent, so immediate, so individualized, simultaneously so far and so near that it escapes our usual categorical grasp. (Habermas, 1984b, p. 236)

Hence one way to think about the value of a modern work of art is in relation to its capacity to keep the rational, reflective consciousness exposed to what is pre-rational, or what is left out or unassimilated in any given conceptual framework or set of cultural standards. In short, it leaves the cognitively and juridically rational subject with an awareness of the irony of its own achievement; that is, with the insight that the structures within which this subject moves are always constraining in potentially serious ways, even when they appear on balance to be the most enabling ones available (Connolly, 1983a). One can now see that one thing which is crucial about aesthetic learning for Habermas is the openness it offers to awareness of the unlearning that necessarily accompanies the other dimensions of cultural modernity and their in-

stitutionalization in processes of modernization.

From Aesthetics to Community

If the foregoing analysis is correct, it appears that Habermas's project has conceptual resources on the basis of which he can comprehend at least some of the insights to which Foucault draws our attention with his aesthetic subject. In short, Habermas is able to locate aesthetic subjectivity within a broader conception of subjectivity in the modern world that links aesthetic-expressive, juridical, and cognitive aspects.

I want to return now to the original question posed for Foucault. Does his framework possess the conceptual resources that would allow him to connect his aesthetic subject to his endorsement of new social movements? Can one find an opening from his aesthetics into an ethics and a politics?

Foucault himself clearly sees his "aesthetics of existence" as having ethical implications. It has at its core, he says, an idea of self-control, of "the kind of relationship you ought to have with yourself, *rapport a soi*." What Foucault means by this is an ongoing formation and articulation of the self according to aesthetic criteria. The primary criterion apparently is the will to "live a beautiful life and to leave to others memories of a beautiful existence." Such an ethics, Foucault hopes, could in no way be implicated in "an attempt to normalize the population" (1984d, p. 341).

What is striking about Foucault's ideas here is that he defines ethics simply as this activity of self-formation: "*rapport a soi*, which I call ethics." Thus any reference to other subjects, and even the possibility of more liberating or consensual forms of common life, seems problematic from the start. Foucault speaks of the process of aesthetic self-formation as involving "asceticism in a very broad sense." He

intends for this phrase to call attention to the practice of self-control that is part of any ethics; but in the case of his own aesthetics of existence, the phrase seems also to have the unavoidable connotation of solitude (1984d, p. 355; 1980a, p. 208). Ironically, in the process of constructing such a provocative mode of access to the self's own pre-rational, embodied otherness, Foucault seems to have simultaneously cut off access to intersubjective otherness. At this point it becomes difficult to see how Foucault could possibly bring his aesthetics of existence into any coherent relationship with his endorsement of some forms of collective political action; yet it seems just as clear that he intends some connection. In the context of discussing the early Stoics, he says rather explicitly that "recent liberation movements suffer from the fact that they cannot find any principle on which to base the elaboration of a new ethics" (Foucault, 1984d, p. 343).

Even if Foucault himself is not very helpful in establishing a link between aesthetic and juridical aspects of subjectivity, one can try to speculate about how such a link might be established. One might point out, for example, that an aesthetics of existence could not literally be a private activity any more than speaking a language. What constitutes art must always be related to some public, shared understanding of style and form (Dreyfus and Rabinow, 1983, p. 258). Although true, this insight does not take one very far toward any normative conceptions.

Another possible avenue might be the one hinted at by Adorno, whose totalistic critique of modern society and its cognitive and juridical forms of consciousness was also accompanied by an appeal to aesthetic consciousness (Adorno, 1966, 1984; Horkheimer and Adorno, 1972). What appealed to Adorno about aesthetic consciousness was the nongrasping, non-dominating way it related to its object. Thus he found in the aesthetic dimension

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a model of intersubjective reconciliation —of a community that allows intersubjective otherness to flourish (Dallmayr, 1981, pp. 133–43; Wellmer, 1984–1985).

Perhaps Foucault's aesthetics of existence could be thought through in a similar fashion. I doubt, however, if this would really be possible. The problem with thinking that the aesthetic dimension can be a model for community has been clearly stated by Albrecht Wellmer:

The aesthetic synthesis represented by the work of art, even if we concede to Adorno that it contains a *promesse du bonheur*, can hardly be understood as a model of dialogical relationship between individuals, who recognize each other in their individuality, as equals and as absolute others both at the same time. If beauty is a promise of happiness, of reconciliation with our internal and with external nature, the work of art would be a *medium* of this transcending experience rather than a *model* of reconciliation itself. For at least the *moral* "synthesis" of a dialogical relationship can only be *mediated*, but not be brought to *appearance* by the aesthetic synthesis of the work of art. Even if, as Adorno stresses, the subject, which comes to speak in the work of art, is a "we" (and not the individual artist), this collective subject speaks with *one* voice, speaking to itself, as it were; i.e., the rules of "synthesis" of this trans-subjective speech cannot possibly prefigure the open rules of a dialogue with *many* voices. Aesthetic synthesis is no possible model for a state of society free from repression. (1983, p. 94)

If we use this distinction between model and medium we might be led to think in ways which build on both Foucault and Habermas. Foucault's image of an aesthetics of existence might be interpreted as a provocative way of thinking about the process of self-formation, in which aesthetic criteria might come to have a constitutive significance for the way a subject thinks and acts in general. Thus, Foucault might be seen as giving us a way of conceptualizing how the aesthetic dimension's potential for tapping into the self's pre-rational, embodied otherness can be released in such a fashion that its effects can infuse all aspects of everyday life. By this I mean that the

aesthetic sense can be seen as permeating not only our need interpretations, but also our moral-political judgments about the kinds of social institutions and technological infrastructure we find satisfying and self-empowering, as well as our judgments about what sorts of knowledge we find worth seeking.

Taken in this light, it is likely that Habermas would find Foucault's ideas suggestive, since his own speculations about art and everyday life have a similar ring. He argues that when aesthetic experience is infused into everyday life, it then

no longer only affects our evaluative language or only renews the interpretation of needs that color our perceptions; rather it reaches into our cognitive interpretations and normative expectations and transforms the totality in which these moments are related to each other. (Habermas, 1984b, p. 237)

But as Wellmer suggests, and Habermas is aware, such a role for aesthetic subjectivity cannot replace an at least partially independent elaboration of juridical subjectivity. A useful way of clarifying this point is to look at recent feminist thought. A recurrent theme there, as in Foucault, is how modern society and consciousness deny our character as embodied creatures and suppress our aesthetic-expressive capacities. Women's experience in this society is seen as making them especially sensitive to these problems. So an important question for feminist thinkers is how this sort of gender-based sensitivity can infuse everyday life in a transformative way. Part of this inquiry involves asking what this means for how we might reconceptualize moral and political life.

Some feminists have argued that the different experience of women can be used as a model for this reconceptualization. Thus "mothering" and "attentive care" become new models for the moral-political community (Elshtain, 1982; Ruddick, 1980). This view has been countered, correctly I think, by other

feminists, who, while not denying the importance of an emphasis on embodiment, aesthetics, and expressiveness as mediums for reconceptualizing the substantive qualities a community might have, nevertheless feel that the form of moral-political relationships among equals cannot be modeled on maternal thinking (Dietz, 1985; Ferguson, 1984, pp. 170-73).

Conclusion

What finally hangs on Foucault's failure to bridge the gap between the aesthetic and the juridical, while he nevertheless endorses political resistance to the normalizing processes of modern life? The most immediate implication revolves around the fact that without any way of conceptualizing juridical subjectivity, Foucault's recommendation of collective resistance has such a blind and undifferentiated character as to be almost politically irresponsible (Connolly, 1983b, p. 332). He provides us, ultimately, with no way of distinguishing the resistance of the women's movement or the Polish Solidarity movement from, say, the Ku Klux Klan or Jim Jones's People's Temple. Of course, Foucault himself would have never endorsed such things. But that is not the point; the point is that his denials have an ad hoc quality.⁷

What about the broader theoretical implications that emerge from the foregoing analysis, particularly in regard to the possibility of mutual enrichment of Foucault's work and critical theory? As I suggested earlier, there are some interesting parallels between Habermas's and Foucault's theories of power. In this regard especially, the latter's work is provocative. If the critical theorist is to take seriously the social and political dimensions of the emerging "informational revolution," then Foucault's distinctive approach to the inherent relation of discourse and power is likely to be a useful

vehicle of analysis (Luke, 1983; Luke and White, 1985; Poster, 1984, ch. 6).

Moreover, the critical theorist might, as William Connolly (1983b, p. 331) suggests, accept "Foucault as a double," in the sense of always keeping his or her categories open to "the voice of the other." This sort of recommendation is, as I suggested earlier, one that critical theory has an obligation to embrace, given its own traditional theoretical goals. Thus, for example, Habermas's categories should be pushed even to the point of asking what constraints or subjugations haunt the very idea of an ideal speech situation.⁸ However, one thing must be kept firmly in mind in this game of "double." As such questions are posed and played out, there comes a point where we must face the problem of weighing degrees of constraint against degrees of enablement implicit in any concepts or recommendations about the structures of social life.⁹ Such a weighing process only becomes possible and comprehensible by the light of some notion of juridical subjectivity.

It might be objected at this point that my criticism of Foucault is too heavily premised on an implicit argument that Habermas's conception of juridical subjectivity has no problems of its own. Clearly, I have not here submitted Habermas to the same degree of scrutiny as Foucault. But that is not fatal to my argument, because it depends less on whether Habermas's particular arguments are entirely correct, and more on the fact that his theory, unlike Foucault's, confronts a certain necessary range of philosophical problems. "Necessary" here is to be taken in the sense of necessary if social theory wants to avoid the extremes of endorsing either blind actionism or ethical-political quietism.

Notes

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ated out of the lively intellectual atmosphere of William Connolly's 1984 National Endowment for the Humanities Summer Seminar on "Genealogy and Interpretation." I would like to express my thanks to him, the other seminar participants, the National Endowment for the Humanities, and my colleague, Timothy W. Luke.

1. I have corrected a translation error in this text. In the last sentence, "enlightenment" was incorrectly inserted in place of "emancipation."

2. On the general theme of "aestheticism" in Foucault's work, see Megill (1985, pp. 2-4 and chs. 5-6).

3. Foucault (1984d, p. 354) attributes to later Stoicism the more familiar juridical ideas of universal reason and community.

4. For how this communicative model of ethics is particularly open to intersubjective otherness, see White (1984) and the exchange between McCarthy (1984) and Dallmayr (1984a). Habermas has been particularly concerned recently (1981a, 1984a, 1985) to emphasize that an account of subjectivity arising out of the communicative model does not suffer from the deficiencies of accounts arising out of the monological "philosophy of consciousness" that has dominated Western thought and been appropriately criticized by Foucault and others.

5. Cf. Charles Taylor's (1982, pp. 98-100) remarks about the difference between our symbolic activity and that of primitive societies.

6. Habermas (1982, pp. 23ff.) suggests that Nietzsche, in his critique of modernity, is also guilty of forgetting this point.

7. See, for example, his affirmations of "consensual politics" and "consensual disciplines" (Foucault, 1984c, pp. 378-80). While laudable, such notions have a rather oxymoronic quality when viewed from within Foucault's own theoretical perspective.

8. Cf. Frazier (1985, pp. 178ff.). Habermas (1979a, pp. 57-59) considers this possibility himself.

9. In the same interview where he speaks of "consensual politics," Foucault (1984c, p. 379) also admits this problem; but again, his theory gives us no insight into it.

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THUCYDIDES ON THE CAUSES OF ATHENIAN IMPERIALISM

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Thucydides' investigation of Athenian imperialism is in part an investigation into whether imperialism as such is based on universal human compulsions, and hence cannot simply be condemned. It is generally recognized that for Thucydides, Athenian imperialism is connected to the Athenian national character, but it has not been widely appreciated that Thucydides provides a detailed account of the foundations of the Athenian character in human nature itself. That account revolves around what he calls "daring" and the human impulse of eros. The erotic and daring character of the Athenians is connected by Thucydides both to the unique democracy of the city and to its unique experience in the Persian Wars. The unique Athenian character stems from an unprecedented liberation of certain impulses of human nature. This produces Athenian imperialism and dynamism, but also destroys the city in time.

The conspicuous theme of Athenian imperialism in Thucydides forms a crucial part of his broader investigation into the place of justice in relations among states. The phenomenon of imperialism poses what is surely the threshold question concerning the place of justice in international affairs: If the unprovoked subjection and rule of weaker states by stronger cannot be unambiguously condemned, the applicability of moral categories to international politics altogether must be seriously compromised. Imperialism is ordinarily considered a grave injustice in those who practice it, but there is an argument in Thucydides, championed most vigorously by the imperial Athenians, that imperialism cannot be blamed, because it is only a reflection of certain universal compulsions—compulsions of power and of human nature. The Athenians assert that, in acquiring and maintaining their empire, they have done no more than any other state in their position would have done,

indeed would have been "compelled" to do by these universal forces (e.g., 1.75-76, 5.105).¹ Thucydides' investigation of Athenian imperialism, especially its causes, is in large part an attempt to explore and test this notorious Athenian thesis on justice and empire, and turns very much on the question of whether or to what extent Athenian imperialism is rooted in deeper principles of human nature. Thucydides' presentation of Athenian imperialism in fact represents a prototypical study of political psychology in foreign and domestic politics, and of the ramifications of this psychology for the question of justice in international politics especially. To explore the deeper levels of Thucydides' analysis of Athenian imperialism is the primary object of the present study.

Daring and the Athenian Character

When we ask the question about the

causes or roots of Athenian imperialism in Thucydides, we are immediately thrown into a consideration of what we can only call the Athenian character (Bluhm, 1962, p. 22; Connor, 1984, p. 39; Cornford, 1907, p. 167; Ehrenberg, 1947, p. 47; Finley, 1967, p. 143; Romilly, 1963, p. 77; Shorey, 1893, pp. 72-73; Thibaudet, 1922, pp. 113-14). Friends and enemies alike speak of the Athenian character and Athenian manners when called upon to explain Athenian imperialism; Thucydides, in his own portrayal, appears to do likewise. If we bring together all that is said about the Athenian character in the course of Thucydides' History, whether by Thucydides himself or by his speakers, we get what is in fact a remarkably consistent portrait of the Athenians, revolving around certain commonly acknowledged core traits. The most prominent of these, and the one most widely held to separate the Athenians from other men, is called "daring" (*tolma*) by all who broach the subject. This becomes almost a technical term in Thucydides.² The Athenians are daring in the History; the Spartans are the opposite (cf. 8.96.4-5). Brasidas is the single Spartan exception that proves the rule (cf. 5.7.2; 2.87; 2.89.5). The Syracusans alone succeed in countering the Athenians by becoming, like them, daring (6.69.1; 7.21.3-4; 8.96.5).

The Corinthians are the first to emphasize this trait, in their famous description of the Athenian character in Book 1 (1.70.3). Daring is closely linked by them and by others in the History to Athenian restlessness and expansiveness, and hence to Athenian imperialism. It seems to describe precisely the frenetic, astoundingly bold, even reckless quality the Athenians display in their many far-flung enterprises. As a character trait, however, it is rather enigmatic, not only because it is virtually unique to the Athenians, but because it seems to replace other, more ordinary or traditional qualities, in par-

ticular courage.³ Daring is the first puzzle presented by the Athenian character in Thucydides.

Our investigation of this peculiar Athenian trait may appropriately begin with the most famous description of the Athenian character in Thucydides, Pericles' Funeral Oration. Pericles' speech is given over primarily to praising Athenian greatness, including the empire, and he rates the Athenian character as highly as such things as laws and institutions in attributing the cause of that greatness. Yet when it comes to describing the Athenian character, "courage" is barely alluded to by Pericles, who speaks instead of the "virtue" of his countrymen, or, more pointedly, of their "daring."⁴ The men who have died, he says, showed a daring that should be the model for those who survive (2.43.1). The Athenians as a race are characterized by a native daring that allows them, without toil, to be the equal of others who take great pains to cultivate virtue (2.39.4). The greatness of the empire, which makes the city of Athens uniquely worth dying for, was built, according to Pericles, by the daring and dutifulness of the present Athenians' ancestors (2.43.1). Pericles goes so far as to boast that the Athenians have "compelled" every sea and every land to yield access to their daring, enabling them to leave "immortal" monuments of themselves "everywhere" (2.41.4).

In Pericles' presentation, the daring of the Athenians is not only one of their most praiseworthy traits, but one very closely connected with their empire. The primary effect of Athenian daring seems to be the empire, and both are treated by Pericles as uniquely glorious Athenian achievements. Yet there was a time, in fairly recent historical memory, when Athens did not have an empire. Pericles remarks that the empire was built only by the previous generation, a feat for which that generation deserves greater praise than any that preceded it in the city.

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(2.36.2). If that is so, however, we are forced to wonder if not only Athenian imperialism but Athenian daring was born with that generation.

Thucydides' presentation of the rise of the Athenian empire appears to bear out this hypothesis. In Book 1 of the History he shows elaborately how the Athenian empire first grew out of circumstances connected with the second great Persian invasion of Greece, an invasion in which the Athenians were driven, in order to help defend the Greeks, to abandon their homeland and fight the enemy at sea. Before that time Athens was not even a particularly great power in Greece; after that time the Athenian naval power and empire grew dramatically and with astonishing rapidity. Pericles, in a speech prior to the Funeral Oration, ties together this general sequence of events in the following way: The Athenians of that time, he says, beginning literally with nothing, not even their homes and possessions, succeeded in both defeating the Persians and building the empire by dint of resolve and daring more than by luck or power (1.144.4). Thus, in Pericles' view, the Athenian deeds in the Persian Wars are of the same stuff as the Athenian imperialism that followed, at least so far as the fundamental character traits that made them possible are concerned. As to the pivotal moment of the Persian Wars itself, the first time Thucydides mentions it he says that as the massive Persian force advanced, the Athenians resolved to abandon their city, and packing up their goods, embarked upon their ships and "became sea-men" (*nautikoi egenonto*, 1.18.2). The formulation connects this moment and these deeds somehow with a transformation in the character of the city itself.

Thucydides and Pericles are not the only ones to attribute special significance to the Athenian feat of abandoning the city to fight the Persians. Their attitude

was, in fact, the common one at Athens, as Thucydides indicates by giving perhaps the fullest description and greatest praise of those deeds to certain anonymous Athenian envoys who speak at Sparta before the war (1.73–78).⁵ What they say is relevant to the question before us. These anonymous envoys, like Pericles, dwell on Athenian daring. Their presentation, however, places greater emphasis on the period of the Persian Wars; they imply that the Athenian actions at that juncture were paradigmatic—that is, those most illuminating for anyone desiring to know what kind of a city Athens is.⁶ It required the most extreme daring,⁷ the envoys correctly say, for the Athenians to abandon their city when all up to their borders had been enslaved, to rely on themselves as their last, slender hope, and face the Persian throng at sea (1.74). This Athenian action, as the anonymous envoys note, was the most important contribution to the Greek cause, as it was the most amazing. As such, it could not but be welcomed by the other Greek cities. Thucydides indicates, however, that there was something in this daring action that aroused apprehension in the other cities as well. When the Athenians were returning to an Athens devastated by the Persians, and were undertaking to rebuild its walls, the other cities opposed it, fearing, Thucydides says, not only the size of the newly enlarged Athenian navy, but the daring the Athenians had displayed in the war as well. More specifically, Thucydides says that they feared the daring that had "come into being" in the Athens during the Persian invasion (1.90.1). The Athenians were not alone in viewing their own actions during that crisis as a great watershed in Athenian experience, and in the development of the Athenian character. The Athenian character—in particular its daring side—was formed, or at any rate came into its own, only at the time of the Persian Wars. The

cities saw this, and were not heartened by the sight. The great period of Athenian imperialism followed.

Daring and Athenian Imperialism

The question that arises on the basis of this account of the birth of the Athenian imperial character is how we can understand the traumatic moment of the second Persian invasion to have shaped the Athenian character, and given it in particular the volatile and expansive daring that became its hallmark. To recur to Thucydides' terse, early formulation, at that moment the Athenians packed up their things, got into their ships, and became sea-men (1.18.2). That Athens was a naval power is sometimes taken to be sufficient explanation of the Athenian character and Athenian imperialism (Finley, 1967, p. 143, and 1963, p. 90; Romilly, 1963, pp. 67, 69, 70; Thibaudet, 1922, p. 79, cf. pp. 84-85), but other cities, such as Corinth or Corcyra, were maritime or sea-faring cities as well, and none of them resembled Athens in the crucial respect. It is not just that Athens was sea-faring, but the manner in which it was sea-faring that explains the Athenian character and imperialism. Of no other city could it have been said that "becoming nautical" involved such a baptism, or that its citizens became sea-men in so complete or literal a sense.

In order to grasp the political significance that this has in Thucydides' account, we need first to think about all the things that a fixed location means to a political community, and what it means to give that up. It seems that the least we would have to say is that in becoming, however briefly, men without place, the Athenians in some way severed connections with all the fixed things around which the life of a community normally revolves, and that normally serve as its stable, conservative base.⁸ There is, however, another aspect of the matter to

which I believe we must give equal or greater weight, an aspect to some extent peculiar to the Greek cities, and which has to do with Greek piety.

Generalizations about Greek piety are always difficult to make, because that piety consisted of numerous elements that were often inconsistent with one another and are not fully known to us.⁹ Nonetheless, some of its most significant elements, especially for the life of the city, were inextricably bound to place—to sacred ground, to temples or shrines, to ancestral graves. From the point of view of this piety, the city was in effect constituted by common rites and sacred festivals, inasmuch as citizenship was identical with competence to participate in the common cults. These festivals and rites included the city's guardian divinities as well as its generations of ancestors, divine and spiritual beings believed to inhabit more or less immovably the places associated with their cults. When the Athenians abandoned their city to the Persians, therefore, they would also have abandoned their holy places, the abodes of their gods and their ancestral graves. It would be a real question within the conventions of Greek piety whether a city could have any being at all under such circumstances. This is the thought that seems to be in the background when the anonymous Athenians emphasize at Sparta that part of what was amazing about Athenian behavior at the time in question was that they did not simply disperse (1.74.2), did not consider the city to have been ruined or dissolved (1.74.4), and joined the common fight although they were in effect issuing from a city that was no more (1.74.3).¹⁰ The Athenians did not disband, and went on instead to glorious victory. However, the principle or foundation of their community could not help being altered in some way by the experience.

From the point of view of the other Greek cities, the unexampled zeal

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exhibited by Athens in the Persian Wars would thus have not only an admirable, but a terrible or shocking quality as well. The astonishing deed of the Athenians, which seems to display the greatest courage, seems also to bear a certain tincture of impiety. It might go beyond what merely human courage is permitted. It is audacity; it is daring. In any event, it would certainly be mistaken to assume, as the Athenians sometimes seem to (e.g., 1.74.4; 6.82.4), that the cities that refused to abandon their land as the Persians advanced surrendered to them purely out of cowardice.

It is sometimes said that the defeat of the Persian hordes ushered in a new era of Greek self-assurance by giving apparently indisputable proof of the superiority of the polis and freedom over barbarism and Persian despotism (see Bury, 1909, p. 44; Ehrenberg, 1968, p. 77). Thucydides suggests that in the case of the Athenians in particular, the self-assurance gained from this experience was of a revolutionary, not to say hubristic, kind. It is the peculiar character of this self-assurance that leads the Athenians from the desperate, defensive action against the Persians to the offensive explosion of imperialism that followed. For one might say that what the Athenians discovered as a body on their ships is the enormous potential of purely human power—that is, human power standing on its own and bereft of its traditional supports, terrestrial or otherwise. It would be difficult to overestimate the political significance of such a discovery for a community, for traditional piety acts not only as a support, but as a restraint on the activities of men and states. Therefore, insofar as daring among the Athenians represents a transformation of and a replacement for traditional courage, it is an innovation predicated in part on overcoming the inhibitions imposed by piety. Those inhibitions include restrictions on the accumulation of power and on its exer-

cise, in the name of justice. Amorality is at the core of what daring signifies in Thucydides' Athenians.

The speech of the anonymous Athenians at Sparta is a good index of this. What those Athenians say shows that they have liberated themselves in thought as well as action from the restraints that the Greeks and others had thought applied to cities in their relations among themselves. The Athenians at Sparta are the first to voice the thesis that, in establishing their empire, they were simply acting in accord with certain universal compulsions that affect nations: fear, honor, and profit (1.75–76). These Athenians, it is true, temper their argument with assertions to the effect that they hold their empire "not inappropriately" (1.73.1), that they are in some sense worthy of rule (1.76.2), and that their rule is "more just" than necessary. These things are undeniably important to the Athenians' vision of their empire, but they do not disguise—and are not meant to disguise—the fact that the empire and their speech about it represent at bottom an unapologetic overturning of the principles of justice ordinarily supposed to apply to relations among states (cf. 1.76). They altogether refuse to submit to judgments stemming from the supposed precepts of justice. In that respect, at least, their position does not differ from that later voiced by the Athenian envoys on Melos (5.85–113).¹¹

Pericles, the premier statesman in Athens at the time of the anonymous Athenians' speech, is in accord with the new Athenian manner. He has nothing but praise for Athenian daring. His Funeral Oration is devoid of any serious reference to the gods, which is particularly striking given that it is, after all, a funerary speech over those who have died in war. Moreover, Pericles shows by deed that he is willing to abandon Attica, doing so for the first time since the Persian Wars. In connection with that policy,

Pericles in effect tells the Athenians that they should consider the land as something of no significance to the city: human beings are what make the city what it is (1.143.5). In the Funeral Oration he depicts Athens, Athenian power, and Athenian greatness as monuments purely of human endeavor. His reduction of the city to its human beings—and even the present generation of its human beings (cf. 2.36.1–3)—is, to be sure, typical of this imperial Athens; but that is only to underscore its revolutionary character. Pericles' "humanism," of which these remarks are primary examples, is often noted by commentators. The radically anti-traditional and even impious implications of this humanism have received much less attention, yet they are as important as any other facts in Thucydides' understanding of Periclean Athens. They also prove to be crucial in explaining the problems with Pericles' policy and the fate of Athens after his death.¹²

Individualism and the Policy of Pericles

There is one other aspect of Athenian humanism or liberation that must be explored before its full bearing on the subject of Athenian imperialism can be appreciated. When Pericles praises Athenian freedom in the Funeral Oration, he has in mind less the freedom of the city as a whole from traditional restraints on its behavior than the freedom of individual Athenians from the myriad conventional restraints that regulate the lives of men in traditional cities. This unprecedented internal freedom at Athens has created, according not only to Pericles but to other observers in the History as well, a novel political form (cf. the remarks of the Corinthians, 1.70.6). This form represents perhaps the nearest approximation to what we could call "individualism" in all of ancient politics.¹³ The liberation of

individual talent and initiative at Athens is one source of the city's great power and dynamism, as Pericles correctly points out (2.41.2). Athenian individualism is for this reason one of the things that made Athenian imperialism possible. Athenian individualism and imperialism are also connected—and more intimately so—through their common abandonment of traditional restraints. If the city is willing to dare so much, it is in part because the Athenians individually have freed themselves from conventional inhibitions. Athenian individualism represents, in that sense, the domestic correlate to the city's daring in its relations abroad. Not surprisingly, it is also the point at which important problems with the whole Athenian project begin to appear.

The Athenian experiment with individualism, if we may call it that, produced a city of great energy and many devices, but also had to confront the one great problem always connected with individualism, the problem of political cohesion. The tremendous success of Athens from the time of the Persian Wars through the period of Pericles' ascendancy seems to indicate that Athens solved this problem. Thucydides, on the other hand, draws our attention as early as his eulogy of Pericles to the fact that Athens was later devastated by internal quarrels and factionalism, which were the real reason for her defeat in the war (2.65.7, 10–12). These problems came into the open only after Pericles' death. Nevertheless, Pericles himself, in his Funeral Oration, can be seen to address the problem of the cohesion of the city, in addressing what can make the individualists he has praised dedicate themselves to its good. In the Funeral Oration he is confronted with this problem in its most extreme form, for he must find an argument that can induce Athenians to die for their city, as have those being eulogized in the speech.

Pericles responds to this problem in a very striking way, and in doing so intro-

duces what altogether becomes a very important theme in Thucydides' treatment of Athens. Throughout his speech, Pericles appeals to the Athenians' love of glory: Athens is the most glorious of cities, a monument to the virtue and daring of her citizens. Contemporary Athenians, in serving and even dying for the city, share in that glory (2.42.1, 2.43.3; see also Palmer, 1982b). This argument in itself, however, does not sufficiently meet the problem, if only because glory held in common is scarcely glory at all, especially from the point of view of true individualists. Therefore Pericles, at the climactic moment of his speech, has recourse to quite another appeal, one that appears to be as novel as the problem it is meant to address. It is an appeal to erotic passion. In the passage in question, Pericles allows that calculations of advantage are not, of course, to be excluded in serving the city, but he says, finally, that Athenians should become devoted, willing servants of the city by beholding its power, manifested every day in deeds, and becoming lovers of it (2.43.1).¹⁴ It is by means of a kind of erotic attachment that each Athenian should ultimately become a patriot.

Pericles' sudden recourse to erotic passion as the centerpiece of Athenian cohesion or community is striking because we are not at all accustomed to thinking of patriotism in these terms, although it is a seldom-noticed fact that *eros*, or human eroticism as such, is a significant, if somewhat concealed, theme in Thucydides' work.¹⁵ The word *eros* and its derivatives appear only seven times in the History, but each appearance is at a crucial juncture, and each plays a significant role in Thucydides' treatment of Athenian imperialism and the political psychology of Athens.¹⁶ The Greek notion of *eros* is of course broader than ours, embracing potentially all objects of desire, but it still differs from other kinds of desire, both in its intensity and in its unexpungeable

sexual reference. Pericles' appeal to *eros* in this context would be striking and paradoxical to Greek audiences as well as to modern ones, though somewhat less so.¹⁷ With regard to this passage, we might upon reflection agree that an appeal to this kind of passion is appropriate to the Athenian case, particularly given the warmth of Pericles' praise of the city. Erotic passion may, after all, be the one thing capable of attaching even the most individualistic human beings to something outside of themselves. Erotic passion is individualistic, even egoistic, yet leads to the most intense devotion and willingness to sacrifice. In any event, it is clear that Pericles, whose Athens has largely forsaken traditional supports to community and patriotism, must have recourse to extraordinary devices.¹⁸ Patriotism or "love of city" (*philopoli*) of the ordinary kind, based in such things as traditional civic piety and subordination of self, is no longer sufficient grounds for community at Athens. The Athenians have all but abandoned public piety, and Athenian individualism owes its very existence to the abandonment of the kinds of conventional strictures that cement the political community in a city like Sparta. Pericles' appeal to *eros* circumvents or supplants those conventional mechanisms of community, and seeks to bind the Athenians directly or immediately to the city, depicted as a beloved object.

The question is whether this new alternative proposed or supported by Pericles is itself sufficient to the task of holding the city together. For Pericles of course, that includes its ability to keep Athens under his direction and guided by his policy—and his policy, according to Thucydides, was a very measured or restrained one, both in general and with reference to the war against the Spartans (2.65.5). His policy for the war in particular is to resist all temptation to indulge in imperial expansion for the duration (1.144.1; 2.65.7). Thus, when Pericles exhorts the

Athenians in his Funeral Oration to become erotically attached to the city, it is a relatively moderate or restrained policy and vision he has in mind.

This is precisely the part of Pericles' policy that proved most untenable after his death. The Athenians did not follow his advice and in fact, according to Thucydides, after his death they did "everything" contrary to what he had said (2.65.7). Among other things, they proved unable to resist the imperial temptation, and embarked on a vast project to conquer the island of Sicily. They did so, according to Thucydides, under the influence of erotic passion. In what is his only explicit authorial reference to the eroticism of the city as a whole, Thucydides says that when the deliberation concerning the Sicilian expedition finally drew to a close, an erotic passion fell upon all alike to sail for Sicily; the passion was so overwhelming that if anyone still opposed the vote, he was cowed into silence by the "excessive" desire of the city as a whole (6.24).

Pericles appealed to the *eros* of the Athenians in the cause of a general policy of restraint, but when that policy is finally and definitively overthrown by Athenian *eros*, the development, in retrospect, looks all but inevitable. When Athenian passion reaches its height, forces its way to the surface of Thucydides' narrative, so to speak, it expresses itself as a longing for the most spectacular increase of power, the most audacious project of imperial expansion since well before the war. Nicias, in his futile speech against the expedition, pleads with the Athenians, at least the old Athenians, not to succumb to an unhealthy or perverse erotic passion (*duseros*, 6.13.1) for things faraway. His hope is as vain as the hope of Pericles. Indeed, it seems that erotic passion is naturally enflamed more by the splendid, the faraway, and the grand than by any vision of restraint. It powerfully resists domestication, even if a man of Pericles'

stature makes the attempt. And in the Athenian case this passion lies at the core of the city's imperialism.

The Sicilian expedition is the most erotic of all Athenian undertakings during the war—perhaps of all Athenian undertakings—as it is the most daring. It is in this respect a peak of what Athens represents in the History. It is not inappropriate, therefore, that Thucydides should use the sequence of events connected with and surrounding the Sicilian expedition to reveal more clearly than elsewhere his own understanding of the Athenian character and imperialism. The expedition shows the failure of the Periclean policy of imperial restraint, but Thucydides uses the opportunity it provides to record more comprehensively his view of the regime of democratic individualism at Athens. Thucydides' view, as opposed to Pericles', accounts for the decline and eventual failure of the Athenian experiment in politics.

Eros, Daring, and the Athenian Democracy

It appears that for Thucydides, Athenian imperialism is compounded out of a volatile combination of erotic passion and daring, qualities woven into the fabric of the Athenian regime. Thucydides understands the dynamics of this combination better than a man like Pericles, and better, we may add, than the Athenians themselves. The last point is of particular interest because it appears to be the reason why Thucydides, in connection with some of the events surrounding the launching of the Sicilian expedition, inserts a peculiar but very interesting digression on the alleged tyrannicide of Harmodius and Aristogeiton (6.53; 6.54–59; 6.60). The digression, he says, is intended to correct some important defects in Athenian self-understanding. It draws our attention not only for this

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reason, but because its subject turns out to be Athenian daring and *eros*; it contains more references to *eros*, in particular, than are found in the rest of the History put together.¹⁹

The Athenian characteristic immediately in question at this point in the History is the excessive suspicion shown by the Athenian democracy toward virtually all outstanding men. The Athenians' suspicion is excessive, according to Thucydides, because of a memory in the city of the hated tyranny of the Peisistratids. It was this tyranny that provoked the legendary attack of Harmodius and Aristogeiton. According to Thucydides, however, the Athenians are mistaken both about the character of the tyranny, which was actually rather mild until attacked (6.54.5–6; 6.59.2), and about the story of Harmodius and Aristogeiton itself. Thucydides begins his account by saying that the "daring deed" of Harmodius and Aristogeiton was actually undertaken incident to a love affair (6.54.1; cf. 6.56.3; 6.59.1). Harmodius and Aristogeiton were lovers; when Hipparchus, the brother of the tyrant Hippias, threatened their affair, the two of them plotted to overthrow the tyranny. They failed to kill Hippias but managed to kill Hipparchus, and as a result came to be regarded as champions, and practically as founders, of the democracy at Athens after its reestablishment. Indeed, the remembrance of the "tyrannicide" came to occupy a place in Athenian lore as significant as that occupied by the memory of the city's heroism during the Persian Wars.²⁰ The legend became a keystone of the democracy's self-understanding, and a parable in particular for the city's extreme love of freedom.

One of the first things that strikes us about Thucydides' presentation of this episode is that it controverts in one important respect the impression we get from Book 1—namely, that Athenian daring made its first appearance at the time of

the Persian Wars. Thucydides now speaks of a daring in Athens prior to that time, a daring connected with the foundation of the Athenian democracy. He seems to agree with ordinary Athenians that the story of Harmodius and Aristogeiton captures the essence of the Athenian democracy and character. As formulated by Thucydides, the story thus corroborates the view that Athens is a city characterized by a kind of erotically charged daring. However, Thucydides' rendition represents, in precisely that respect, a fundamental correction of the popular understanding. He introduces the digression, once again, for the purpose of correcting the popular understanding; the first and most important of these corrections has to do with the motives that the Athenians attribute to their heroes. The Athenians suppose them to have been public-spirited champions of democratic liberty; Thucydides shows that their motivation, being erotic in nature, was private, even selfish, and aimed at public benefit only incidentally, if at all. Their jealousy toward the tyrants was a purely private, erotic jealousy, and the freedom they sought was primarily what we might call private erotic freedom. Within its context, Thucydides' retelling of the story implies; first of all, that the excessive Athenian suspicion of prominent men in the city is something that presents itself, and understands itself, as a laudable and public-spirited defense of liberty, but is in fact a thing rooted in a rather questionable private jealousy. Thucydides seems to suggest, moreover, that the basis of the jealousy and, altogether, of the devotion to the democracy is not dedication to the common good as such, but rather to a kind of private freedom from restrictions of all sorts, a freedom to follow where one's passion might lead. The force of this passion can inspire those under its sway to acts of the greatest daring on its behalf.

Thucydides' rendition of the Harmodius and Aristogeiton story provides

us with a vivid and revealing image of Athenian individualism and the intense love of freedom that characterizes it. It also provides us with a specific link between the character of the Athenian regime and Athenian imperialism, inasmuch as their psychological foundations now appear to be the same. We may speculate about the historical transition from private *eros* and its ancillary daring to the collective *eros* and daring of Athenian imperialism. The Athenians loved freedom with unequalled passion, and there is a genuine sense, appreciated by all the Greeks, in which rule over others is simply the most perfect form of freedom (see Romilly, 1963, pp. 80-82; Thucydides, 8.68.4). In this sense the empire does represent the crowning emblem and achievement of Athenian freedom. The empire also represents the greatest guardian of Athenian freedom against external attack, a fact the Athenians never lost sight of (e.g., 1.75; 6.82). This consideration may have been particularly important in the historical transition from the democratic love of freedom to the imperialistic impulse forged at the time of the Persian Wars. Reflection on the experience of foreign invasion may have led the Athenians to the conclusion that, just as their heroes Harmodius and Aristogeiton eventually were compelled to enter the public realm merely to safeguard their private freedom, so would the city have to expand externally in order to safeguard its own freedom and the freedom of its citizens (see especially the reflections of Themistocles, 1.93.3-8). Thucydides, at any rate, concludes his narration of the Harmodius and Aristogeiton story with the following terse summary of events: after three years the tyrant Hippias was deposed and fled to the court of the Persian king, whence he returned many years later with the Persians on their expedition to Marathon (6.59.4). Thus the fight against Hippias and tyranny was trans-

formed literally, at least, into the fight against the Persians and its aftermath.

Decline and Fall

It is a long way, of course, from deciding that imperialism is advisable for the safety of the city to justifying it openly for that reason—to say nothing of the justifications from honor and profit that the Athenians simultaneously offer for their empire (1.74-75; 2.63-64). The audacious amorality required for these justifications or exonerations, and thus for the uninhibited development of the imperial project as a whole, is the specific ingredient that Thucydides appears to trace to the daring of the Persian Wars. The unique democracy at Athens provided or bred intense and excessively jealous attachment to freedom; such erotic love of freedom may always verge on simple hatred of all restraints, sacred or profane. Consequently, the democracy also tapped a tremendous source of human energy, but it took the catalyst of the city's experience in the Persian Wars, as well as the opportunities opened up by the Persian defeat, to set the Athenian character definitively on the imperial course. This combination of things produced a city of unprecedented vitality and power, but also a city that in effect destroyed itself within three or four generations after the Persian Wars. The decay of Athens and Athenian imperialism must be traced to the same causes that brought the city such great success in its prime.

We already know from Thucydides' eulogy of Pericles that Athens was eventually consumed by factionalism and internal decay (2.65.7, 10-12). In Pericles' time the city was still a solid and cohesive community, due in significant part to the influence of Pericles himself (cf. 2.65.8). It was Pericles' belief that the Athenians had a kind of native respect for law and for each other (2.37.2-3), and that deference to good leaders was their charac-

teristic trait (2.37.1). Thucydides' view, epitomized in the Harmodius and Aristogeiton story, is that excessive suspicion and jealousy toward leaders was Athens' most characteristic attitude, one derived from the core traits of the Athenian character. It is much easier on the basis of Thucydides' digression than on that of Pericles' Funeral Oration to understand how Athenian erotic passion could lead to faction and internecine strife. Pericles could exhort his fellow citizens to focus their erotic passion on the city as a whole, and with a certain measure of success, but the fundamental character of that passion was private, individual, or individualistic. This is apparent not only from the digression, but from Thucydides' description of the city's erotic passion for Sicily. Thucydides' description indicates that the motives of that passion were widely different for three different types of people in the city (6.24.3). For at least two of those types, comprising the great majority of the population, it was a desire motivated by the prospect of some kind of gain or personal gratification: the prospect of far-away sights for the young, the prospect of present and "eternal pay" for the majority (6.24.3).²¹ The pronounced trifurcation of erotic passion at this point, as well as its predominantly private thrust, shows how ambiguously it is related to the common good. In that respect it is a genuine heir to the tradition of Harmodius and Aristogeiton.

Daring plays a more sinister role in the history of Athens and its imperialism. Pericles praises daring without qualification in his Funeral Oration, but in the plague at Athens, which Thucydides describes immediately after the Funeral Oration (2.47-54), daring shows itself in the city in a rather more questionable light. With an eye to the dissensions that later devastated Athens, Thucydides says that greater lawlessness was first introduced into the city by the plague, as men dared more openly to pursue their own

private pleasure in contravention of all restraint (2.53.1). This imperial quality proves conspicuously less praiseworthy when directed against fellow citizens than against outsiders. Even more to the point is Thucydides' description of the civil wars that destroyed a great many cities during the war. In a famous passage detailing how all the moral categories and the words that denote them were turned on their heads during these upheavals, the first, and perhaps controlling, transformation is said by Thucydides to be the supplanting of loyal courage by "irrational daring" in the minds of the citizens (3.82.4; cf. 3.82.6).²² The passage that begins this way culminates in Thucydides' pronouncement that respect for the divine law then ceased any longer to be a basis of trust or community among men, as oaths lost all their force under the pressure of almost universal lawlessness (3.82.6-7).

These remarks, which represent Thucydides' most systematic pronouncements in his own name on the character of daring certainly give us pause when it comes to considering the case of Athens. Athens has replaced courage with daring in the precise sense of disembarassing itself of the traditional restraints of justice and "divine law." The resulting freedom is what has allowed Athens as a city to accumulate such vast power and such an empire—to run circles, so to speak, around its more traditional and more inhibited opponents, in particular the Spartans. Daring undeniably represents the discovery of an "effectual truth" about the character of international politics. It also represents, however, a very corrosive incubus at the heart of the Athenian regime, though it is one that takes its time to develop.²³ It takes its toll not only on the internal cohesiveness of the city, but on the character of Athenian imperialism itself.

The question as to whether, in what sense, or to what extent Athenian imperialism changes over the course of

Thucydides' History is a very vexed one.²⁴ The foregoing analysis suggests that it remains constant, inasmuch as amorality and impiety are at its core from the moment of its inception. On the other hand, the Athenian consciousness of and attitude toward this amorality undeniably changes, giving Athenian imperialism a much different cast over time. Thus the anonymous Athenians who speak at Sparta before the war proclaim that justice does not apply to their empire, and yet maintain that their rule is "appropriate," that they are "worthy," even that the empire is an expression of their powerful love of honor (1.73, 75-76). In the Funeral Oration, Pericles' very theme is the worthiness of Athens and the appropriateness of the position she holds. In his third and final speech, under the sobering influence of the plague and the continuing war, Pericles concedes that the empire is a tyranny or is like a tyranny (2.63.2); however, he still holds that it is a glorious and honorable thing to maintain (2.63.1; 2.64.5-6). It is precisely the argument or belief that the empire is somehow honorable, however, that decays and finally disappears under the impact of the harsh reality of imperial rule—under the dawning awareness of what it really means to dare to rule over others in defiance of justice, and to admit that one's rule is a tyranny. The amorality of such a project destroys in the long run any claims to higher motives or sanctions, because empire and tyranny simply are not an honorable business. This is the logic that is starkly revealed in the Melian dialogue (cf. Jaeger, 1939, pp. 401-402; Finley, 1963, p. 89; and n. 11). Even at the moment of Sicilian enthusiasm, which seems a much more generous manifestation of the imperial impulse at Athens, we note that, according to Thucydides' description, there was no group that was in love with the glory or "honor" of the enterprise (6.24). Their only goals were safety or profit understood in one way or

another. In that respect, at least, the Sicilian episode is legitimate kin to the Melian dialogue, which Thucydides juxtaposes it to. The extractive and exploitative elements in the imperial impulse and the empire in the end undo all the higher pretenses that Pericles and the Athenians liked to pride themselves on. The reality of imperialism is much harsher than that.

Our final question—that with which we began—concerns whether Athenian imperialism, based on the impulses Thucydides traces to it, reflects a universal compulsion of human nature, as the Athenians argue, and if so, whether its compulsive character, as the Athenians also argue, exonerates imperial powers from moral blame. That communities that follow the impulse to empire are likely thereby to destroy themselves does not alter the state of this question on the level of principle. Thucydides, in tracing Athenian imperialism to human erotic passion, does indeed ground it in the deepest strata of human nature. That the Athenians, in their daring, have to an unprecedented extent unleashed this erotic impulse from traditional restrictions makes them unique, but does not alter the naturalness of their imperial impulse. What is unprecedented about the Athenian regime in general is precisely its liberation of human nature.

There is one passage in Thucydides uniquely relevant to this point, containing the one reference to erotic passion we have not yet discussed, and that is the speech of the Athenian Diodotus in the debate over the rebellious Athenian ally Mytilene (3.42-48). The central portion of Diodotus's speech is a highly theoretical discussion of the relation of human nature to law and restraints of all kinds. It is, in fact, the most sustained theoretical treatment of this issue in Thucydides, and both *eros* and daring figure prominently in it (3.45).²⁵ Diodotus argues that human nature is irrevocably hostile to restraint, because it is governed by a powerful and

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promiscuous erotic impulse (3.45.5). This impulse is responsible, according to him, for human *hubris* and human daring (3.45.4). Although his argument points in the first instance to an erotic impulse that has led the Mytilenaeans to revolt, he makes it clear enough that his argument applies to Athens and Athenian imperialism in equal or greater measure: the human erotic impulse, he says, is the more irresistible the greater the objects it has in view; the greatest of these objects is independence for some, and rule over others for those who have the power (3.45.6). Diodotus's implicit account of the basis of Athenian imperialism and its relation to human nature in general is fundamentally the same as that of Thucydides. It also seems to endorse the Athenian conclusion that moral blame cannot attach to this imperialism, any more than the Mytilenaeans can be blamed for revolting from the empire.²⁶ Both are equally compulsive. The world of recurring, equally justified or unjustified domination and revolt that emerges from Diodotus's speech is indeed a world where traditional notions of justice and law among nations are suspended or greatly restricted.

The only conspicuous vindication that justice or divine law receives in Thucydides, then, is the unhappy spectacle of Athens destroying herself after daring systematically and self-consciously to deny that law in its dealings with others. That does not in retrospect alter the truth of the Athenian denial, and certainly does nothing to help cities in the position of Melos, or even to justify their resistance. It invites compassion more than condemnation for those overcome by the hubristic impulses of human nature, but also yields a counsel of prudence to future statesmen. Thucydides is best understood as an educator of statesmen (see Bruell, 1974, p. 11; Connor, 1984, pp. 12-13; Finley, 1963, p. 309; Romilly, 1956; Strauss, 1977, p. 202n;

Wettergreen, 1979, p. 93; White, 1984, p. 88) and statesmen who have been enlightened by Thucydides will not, to be sure, adhere foolishly to the pious hopes and moralistic illusions of the Melians or of Nicias; neither will they heedlessly praise and advance qualities like daring and unbridled erotic passion in their states, like Pericles did. The conventions that restrain these impulses do not have the divine sanctions they were once supposed to have, and they do represent restrictions on or suppressions of powerful elements of human nature. As Diodotus says, such conventions can never be wholly effective, especially when great temptations to empire arise. We nevertheless learn from Thucydides that the integrity of political communities and the maintenance of those human values that culture or civilization does serve are in the long run dependent on those conventions. To the extent that culture or civilization represents a genuine peak of human flourishing—and Thucydides clearly thinks that it does—those conventions could even be said to have a grounding in nature themselves.

Notes

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1. I cite Thucydides by the standard form: book, chapter, and, where appropriate, section. For convenience, I will refer to Thucydides' work as the *History*, capitalized but not italicized, to reflect the fact that Thucydides never himself gave his work this or any other title (cf. Edmunds, 1975, p. 6; Finley, 1963, p. 3n; Strauss, 1977, p. 143).

2. The patterns of the use of this word can be traced with the help of any word index to Thucydides, for example, that of Bétant (1961). The patterns, as indicated, are quite striking, though to my knowledge they have gone virtually unnoticed in the literature. See e.g. Huart (1968, pp. 431-36) and Hunter (1973, p. 26). Interpreters have often noted that Thucydides' presentation of Athens revolves

around certain significant words—e.g., *polypragmosune* (Ehrenberg, 1947) and *gnome* and *techne* (Edmunds, 1975)—and all with justification. *Tolma* is one of these and, I believe, in some respects the most important.

3. It should be noted that the Greek word *tolma* covers the same range as the English word "daring," from courage to rashness (courage proper is in Greek *andreia*). In fact, the rarity with which the word "courage" is applied to the Athenians is quite as striking as their corner on "daring" in Thucydides, a distinction lost or blunted in all translations I am aware of, which are as likely to translate "courage" as "daring" for *tolma*.

4. Pericles does speak of the "courage" of the Athenians once, when he is developing a specific contrast to the Spartans (2.39.4; cf. 2.39.1). This, if I am not mistaken, is the sole instance of the direct application of this word to the Athenians in the History (cf. also 2.64.2).

5. Their anonymity seems designed to indicate that they speak for the city as a whole (cf. Pouncey, 1980, p. 62; Romilly, 1963, pp. 108, 242). For a different view, see Jaeger (1939, p. 393).

6. This seems to be the reason for the peculiar way they introduce their account of these Athenian actions. They will refrain, they say, from speaking of ancient things that are not to the point, but are "compelled" to mention the deeds of the Persian Wars (1.73.2): they are repeating the story because it is indispensable to the point of their speech, which is to show how great or formidable a city Athens is (1.72; 1.73.1). Narrations of quasi-mythic ancient deeds were, in fact, common in speeches of a certain kind—e.g., the Scholiast mentions possible tales about the Amazons or the Heraclids. See for example Isocrates *Panegyricus* 20–32; 68–72.

7. This appears to be the only occurrence of the superlative "most daring" in the History.

8. The traumatic character of the abandonment of the land can be judged in part from 2.15. For interpretations of that passage relevant to our present purposes, see Thibaudet (1922, p. 199) and Palmer (1982b, p. 827). For an ironic but also important view of it see Pseudo-Xenophon *Athenaion Politeia* 2.14.

9. See e.g. Dodds (1951, chs. 2, 6, et al.), Grundy (1948, vol. 2, pp. 87, 88), and Glotz (1904). The following discussion owes much to Fustel de Coulanges (1980).

10. It should be noted that dispersal was a common (though not a universal) fate for peoples ejected from their cities in the war.

11. Cf. Adkins (1960, p. 222), Connor (1984, p. 153), De Ste. Croix (1972, p. 14), Ehrenberg (1947, p. 52), Gomme, Andrewes, and Dover (1945–1970, vol. 4, p. 184), Romilly (1963, pp. 65, 250), White (1984, pp. 77, 81), and Woodhead (1970, p. 17). The Athenian remarks on Melos are of course much meaner in tone than the earlier statement, which

does reflect a changed vision of Athenian rule (cf. Cogan, 1981, p. 89). This question will be addressed in greater detail in the concluding part of this essay.

12. On Pericles' "humanism" see Grene (1950, p. 90), Grundy (1948, vol. 2, pp. 7–8), and Bury (1909, p. 146). On the impiety of this humanism see Cochrane (1965, pp. 23, 55), Edmunds (1975, pp. 26, 39, 45–46, 76, 82), and Connor (1984, p. 72n).

13. Individualism is sometimes supposed to be the exclusive preserve of modern politics. On individualism as characteristic of Periclean Athens, see Cochrane (1965, p. 23), Grundy (1948, vol. 1, p. 171, vol. 2, p. xv), Gomme (1962, pp. 139–55), and Finley (1963, pp. 49, 301; 1967, p. 149). For somewhat different views see Adcock (1963, p. 50), and Grene (1950, p. 31).

14. I have attempted in my paraphrase to retain an ambiguity of the original: it is not clear whether Pericles exhorts his listeners to become lovers of the city or of its power.

15. One of the few studies to take this theme seriously is Cornford's (1907), but he takes it in a "mythic" sense, which I believe is unjustified. See Bury (1909, p. 124), Wallace (1964, p. 256), Dodds (1951, p. 186), and Huart (1968, p. 391).

16. The seven appearances are at 2.43, 3.45, 6.24, 6.54 (twice), 6.57, and 6.59, not counting one reference by Nicias to *duseros*, bad or unhealthy erotic passion, at 6.13. All of these references will be discussed in the course of this essay.

17. See Euripides *Phoenissae* 359, and Finley (1967, p. 21), who refers to Euripides *Erechtheus* Frag. 360.54.

18. Some perceptive observations in this vein regarding Pericles' appeal to erotic passion may be found in Thibaudet (1922, p. 25), who speaks of an unusual political "crystallization" of erotic passion at Athens; Cochrane (1965, p. 55), who speaks of *eros* here as a substitute for ordinary patriotism based upon piety; and Immerwahr (1973, pp. 27–28), who connects it to the "pathological" course Athenian policy later took.

19. There has been a great deal of scholarly commentary on this digression, much of it assuming the passage does not have great significance for the History as a whole. See the summary in Gomme et al. (1945–1970, vol. 4, pp. 317–29), which concludes with the suggestion that the digression derives from the historian's obsession with setting the record straight. Bury (1909, pp. 89–90) believes its aim is simply to correct popular misconceptions, apparently of no great significance. Finley (1967, p. 168) and Romilly (1963, p. 217) suggest that the digression has a deeper purpose, which they do not however attempt to identify. For other treatments that take the passage seriously, see Rawlings (1981, pp. 100–113), Palmer (1982a, pp. 105–109), and Connor (1984, pp. 178ff).

20. Among other things, the Athenians erected a statue of them in the agora, not once but twice. See

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Lang (1955).

21. The desire of the old men in the city seems to revolve more exclusively around the question of simple safety.

22. The phrase "irrational daring" is used by Thucydides on one other occasion: to describe the act of Harmodius and Aristogeiton (6.59.1).

23. The general point that the amorality of the Athenian empire caused Athens itself to degenerate is one that has been argued before. See Grene (1950, p. 32), Strauss (1977, pp. 192-209), and Ehrenberg (1947, p. 53).

24. The literature on this subject cannot even be canvassed in a note. Romilly (1963, pp. 59, 65) sees no development, but the presence of two opposed and coexisting strains; White (1984, ch. 1) describes a complicated process of degeneration that embraced the whole Greek world. See also Jaeger (1939, pp. 401-402), Finley (1963, p. 89), and note 11, above.

25. It is agreed by perhaps most of the commentators who address the issue that, within the context of the debate over Mytilene, Diodotus's speech reflects Thucydides' own views. See Shorey (1893, pp. 67-70), Romilly (1963, pp. 329f), Cornford (1907, pp. 121, 135), Grene (1950, pp. 59, 66), Ehrenberg (1947, p. 51), Bury (1909, p. 137), Finley (1963, p. 83), and De Ste. Croix (1972, p. 21). There is disagreement as to whether Diodotus is uniquely close to Thucydides in his views. This, at least with regard to the points raised in the text, is my interpretation. See Strauss (1977, p. 231), and Bruell (1974, pp. 16-17).

26. This is not to say that justice is as completely absent from Diodotus's argument as he claims. For one thing, it now becomes "unjust" to blame the Mytilenaeans indignantly, as Cleon does, though not to punish at least some of them for reasons of expediency. See also Orwin (1984).

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SOVIET ELECTIONS REVISITED: VOTER ABSTENTION IN NONCOMPETITIVE VOTING

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This analysis of voters and nonvoters in a large sample of recent emigrants from the Soviet Union shows that nonvoting is correlated with high interest in politics, a critical political outlook, and dissident modes of behavior. Thus, voter abstention in noncompetitive balloting can be hypothesized to constitute a significant political act rather than passivity. Single-party states use single-candidate elections for a variety of purposes, one of these being the psychological reinforcement of unity between regime and subjects. In this context, the only choice left to the dissenting citizen is not to vote at all. In the contemporary Soviet Union, nonvoting is regionally focused on Moscow and Leningrad, and is associated with post-Stalinist generational change. The covert nature of vote evasion and its informal tolerance provide a new perspective on the character of the Soviet system and its political culture.

Research on elections has focused mainly on democracies, on the assumption that there is little point in studying state-controlled, noncompetitive elections, since their outcome is preordained. This assumption bears reevaluation. Elections in non-democratic systems have functions other than selecting leaders, and the extent to which these functions are fulfilled can tell us much about the nature of the systems.

Three theories have been put forward to explain why undemocratic systems hold elections: (1) the elections, although noncompetitive, help legitimate the regime; (2) noncompetitive elections politically socialize the masses; and (3) such elections help mobilize populations (Friedrich and Brzezinski, 1966; Jacobs, 1970; Schapiro, 1967). These and other

theorists (Rose and Mossawir, 1967) have also alluded to an intent on the part of the regime to inculcate allegiance and undermine any inclination toward dissent among the population. This last aspect has been highlighted by Guy Hermet (1978) in his recent reconceptualization of the functions of no-choice elections. He notes that in this context, even numerically minor voter abstention attains significance as a deliberate act of disaffection. Based on this line of argument, we intend to analyze the meaning and implications of recent trends in nonvoting in one of the most thoroughly state-controlled political systems of this age, the Soviet Union.

Past research using nonvoting and negative voting in the USSR as an indicator of trends in dissent has been based

exclusively on official election statistics (Gilison, 1968; Maggs, 1968). Here other sources will be used as well—mostly interviews with former Soviet citizens now living in the West. As past research has shown, circumspect use of this unconventional source can contribute to both factual and analytical knowledge about the Soviet system (DiFranceis and Gitelman, 1984; Gitelman, 1977b; Inkeles and Bauer, 1961; White, 1978).

In the case of Soviet elections, several recent studies providing major new insights have made extensive use of in-depth interviews with recent emigrants (Friedgut, 1979; Zaslavsky and Brym, 1978). Building on this work, the present article incorporates a new empirical and analytical dimension by using the results of the Soviet Interview Project, the most systematic emigrant survey to date. A sample of 2,973 former Soviet citizens who came to the United States between January 1, 1979, and April 30, 1982, were interviewed by Russian-speaking, trained interviewers using a highly structured questionnaire. While every effort was made to safeguard the scientific character of the survey, no claim is made here that the sample constitutes a representative cross-section of the Soviet population. Rather, the internal variance and patterns of association found in this sample more likely than not reflect similar associations in an urban European Soviet referent population (see appendix for more details). In light of the lack of opportunity for Western social scientists to conduct independent survey research in the USSR itself, surveys of this proxy population must do.

The first part of this article will demonstrate that nonvoting in single-candidate elections constitutes a dissident political act by analyzing the role of such elections in the perpetuation of single-party systems, and by examining the electoral process in action. The second part will focus on an empirical substantiation of

the argument by an analysis of the emigrant survey data. Results suggest that nonvoting is significantly correlated with various modes of dissident behavior and with an intense and critical interest in politics. Since a high interest in politics is one of the primary attitudinal correlates of voting in democracies, this suggests that nonvoting in undemocratic systems is comparable to voting in democracies.

The Soviet Electoral Context and the Significance of Unanimity

Elections are taken very seriously in the Soviet Union. As calculated by Hill (1976, p. 590), Soviet citizens are exposed to election campaigns for an average of one and a half months per year. There are elections for the formal parliaments of the USSR and of the union republics, as well as for a substructure of lower-level councils, or "Soviets," as they are officially called. Enormous efforts are expended on the electoral process. Every campaign results in a flood of exhortative articles in the press, in dozens of meetings at places of work and in neighborhoods, and in the building up of a huge apparatus of electoral officials. The enormous flurry of activity culminates in election day. Always a Sunday, it is marked by a festive atmosphere, with bands playing and banners flying as streams of voters arrive in the polling places. Typically, the Soviet voter takes his ballot with the single candidate's name on it and drops it unmarked into the ballot box—this constitutes an affirmative vote.¹ It is widely publicized that every voter is given a chance to participate, even if he lives in the remotest region, travels on a train or ship, or is confined to his bed. The results are noted with gratification: a voter turnout of 99% or more, with the same percentage voting for the official candidates. In an acclamation of the significance of this event, *Pravda* (March 7, 1979) writes:

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The results of the elections to the USSR Supreme Soviet and the unanimous election to the country's supreme body of state power of the candidates of the indestructible bloc of Communists and non-Party people provide striking new evidence of the monolithic unity of the Party and the people and of the working people's full support for the domestic and foreign policy of the CPSU [Communist Party of the Soviet Union] and the Soviet state. The elections have convincingly shown the thoroughly democratic nature of the world's first society of developed socialism and the working people's firm resolve to persistently strive for new successes in all sectors of communist construction.

This summarizes the themes of the election, which, while exhorted as the high point of Soviet democracy, focuses on demonstrating the lack of divisions in the society. The 99% is repeated over and over again as if it were a magic number safeguarding the future.²

As pointed out by Linz, this striking insistence on unanimity is at the core of a regime's intent in staging totalitarian elections. Referring to the work of Simmel, Linz (1978, p. 47) argues that this insistence has a deep ideological and political significance, symbolically anticipating the result of concrete battle and coercion. Other analysts of the purpose of non-competitive elections in the Soviet or comparative frameworks have advanced variations of this argument. Hermet (1978, p. 14) speaks of the alienating role of such elections in creating the psychosocial conditions for the system's smooth working, and Zaslavsky and Brym (1978, p. 371) argue that the demonstration of fictitious democracy is a means of buttressing the regime, since it prompts the population to show that it has accepted as legitimate what in fact is illegitimate. Or, in the words of Reshetar (1978, p. 198):

The elections also have a psychological significance because the voter is not permitted to boycott the balloting ritual but must participate and make a commitment to the regime. Thus, the voter is periodically reminded by means of this mass loyalty test and exercise in assent that there is no alternative to the Communist party and its regime, and that dissent is not permitted.

The logic behind mobilized political participation is similarly explained in most definitions of totalitarian systems, and this is taken as a major trait distinguishing them from authoritarian regimes, which are more reliant on the apathy and passive obedience of subjects (Friedrich and Brzezinski, 1966, pp. 163-65; Linz, 1978, p. 43).

This is not to posit that the contemporary Soviet Union fits a conventional definition of the totalitarian state, but rather to raise the question about the significance of the USSR's retention of certain totalitarian trappings, of which the demonstration of unanimity on election day is one. The current electoral system was adopted at the height of Stalinism, and the first general elections under it were held in 1937. Such continuity in a major aspect of Soviet politics bears investigation to determine how real it is and whether there is evidence of change under the surface.

Indeed, some change has taken place. There has been a trend toward nonvoting, but this change is unsystemic and is due to individual grassroots impulses that have been allowed to take their course through the connivance of low-level electoral officials who themselves have been coopted for the voting exercise. And although the nonvoters engage in evasion, which suggests passivity, it does entail considerable symbolic and real activity.

To elaborate first on the symbolic aspects, let us return to the psychological and socializing role of noncompetitive elections. The Soviet election process demonstrates a striking formal thoroughness and elaborate organizational structure. It is not just that the media and officials give extensive, ritualized coverage to the elections and that an enormous amount of time, money, and effort are expended on them. A major point is also made of the democratic nature of the process itself. There are painstakingly detailed rules and regulations about

nominations and the establishment of electoral districts and commissions, as well as guarantees of the equality of the vote, its secrecy, and the fairness of the count (Grigoriev and Zhdanov, 1980; *Kommentarii*, 1977, 1979). But it is not for nothing that so much political workmanship goes into this elaborate artificial structure. It is designed to have a deep psychological impact on the voter, who either will accept its democratic nature at face value or will feel intensely alienated. Even if the latter is the case, going through the motions of voting induces compliance.

The balloting epitomizes the gaps between theory and practice that the voter is expected to accept happily. Roy Medvedev (1975) focuses on one of these gaps when he cites the instructions to the voter to "leave the name of the *one* candidate for whom you wish to vote and cross out the remaining ones." Since there is always only one name on the ballot, this is patently absurd. Similarly absurd is the regulation that "ballots are filled out by voters in rooms for secret balloting" (Electoral Law, Art. 53, passed on June 6, 1978). This too is theoretically in line with democratic procedure, but in practice there is nothing to fill out, because a vote for the single candidate is not marked and the ballot is simply deposited in an urn. Affirmative votes, far from being secret, are demonstratively open. This also takes away secrecy from the rare voter who enters a voting booth, since the implication is that he intends to strike out the name of the candidate and thus cast a negative vote.³

In the Soviet Union as well as other single-party systems, voting is primarily an act of allegiance to the regime. The citizenry's involvement in regime-directed routines and rituals fosters habits of compliance and instills a perception of the leadership's omnipotence (Almond and Powell, 1984, pp. 319-20). Seen in this light, electoral evasion is a political act.

We now turn to the question of how this evasion occurs and how extended it is.

The Mechanics and Extent of Nonvoting in Soviet Elections

Nonvoting in the USSR occurs in four ways: (1) by not being registered on the electoral rolls; (2) by simply failing to vote; (3) by one's ballot being cast by somebody else; or (4) by requesting an absentee ballot and not using it. Only the second way is acknowledged by official sources, and then only partly. The other forms of nonvoting are evident from incongruities in Soviet data as well as from unofficial sources such as *samizdat* (self-published work) and interviews with emigrants.

As noted by Powell (1982, pp. 113-15), the calculation of electoral turnout depends on one's definition of the universe of potential voters, and is most accurately measured by reference to all eligible voters, and not just those who are registered. Because Soviet calculations of turnout are based only on the number of registered voters, one has to consider how many eligible voters are being omitted from the electoral rolls. Since this question has not previously been addressed in the literature, it is beyond the scope of this article to venture more than a rough estimate that the relevant number is several million, or between 1% and 3% of the Soviet population. Keeping complete lists of eligible voters is a complex task (Rose, 1974, p. 69). There are at least two groups that constitute a special problem. The first consists of migrants striving for permanent residency in the larger cities, which are the most desirable places to live but require hard-to-obtain permits. Hundreds of thousands of these migrants end up as illegal residents (Grandstaff, 1980, p. 16), which means that they are unlikely to be on the electoral rolls prepared by official bodies. People in prison and labor camps are also unlikely to be on the

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rosters. While the legal restrictions on the electoral rights of individuals convicted of crimes were annulled in 1958, and theoretically there is "universal suffrage for citizens who have reached the age of eighteen, except the legally insane" (*Kommentarii*, 1979, p. 14), there are indications that people who are "in custody" do not vote (Latov, 1974, p. 25; Reshetar, 1978, p. 194). The total imprisoned population in the USSR is estimated to be in the millions.⁴

Leaving aside the problem of voter registration, one still has to assess how many of the people who are on rolls do in fact cast their ballots. According to Soviet electoral statistics, voter turnout has always been extremely high, and reached the 99.99% mark in the 1984 elections to the Supreme Soviet. In other words, it is claimed that only 23,039 out of 184,029,412 voters failed to vote, and in the case of the Turkmen SSR just 1 voter out of 1,531,610 (*Pravda*, March 7, 1984). This claim is likely to strain anyone's credulity. It appears that the actual percentage range is somewhere between 90% and 95%. While there appears to be little outright falsification of returns, some votes are cast by persons other than the voter, and another contingent of voters is stricken off the registers and does not affect the final calculation of the turnout. This method of arriving at the desired 99.99% figure illuminates Soviet political culture and should be considered in some detail.

Voting in the USSR is not legally mandatory, but intense psychological and social pressures are applied to get the vote out. Especially important is the use of a network of "agitators," who are enlisted by electoral commissions to contact from 20 to 30 voters before the election, provide a political educational talk, and enjoin them to be sure to vote. In effect, the agitator is made responsible for "his" (or "her") voters, and is called to task by the Communist party (agitators usually are party members) or others if the

assigned voters should fail to vote. Feeling highly pressured to fulfill his norm, the agitator transmits pressure to the voters. His message—"it is your duty to vote, please do not get me into trouble," and "you yourself may be in this role one day"—becomes most insistent on election day if the voter fails to show up at the polling place early and is therefore visited at home. If a voter cannot be found, refuses to vote, or turns out to be incapacitated, the pressure remains on the agitator to do something about it. The easiest way is to cast the ballot for the voter, and this is what happens with some regularity (Friedgut, 1979, p. 114).

Soviet electoral law stipulates that "each voter has one vote" and "each voter casts his ballot in person" (*Kommentarii*, 1979, arts. 3 and 46). Thus it is illegal to cast ballots in somebody else's stead, but apparently this practice is tolerated relatively widely (Friedgut, pp. 115-16; Schmemmann, 1984). In the words of Roy Medvedev (1975, p. 143):

Commissions in charge of polling places sometimes allow a daughter to vote on behalf of her mother, a husband for his wife, or one neighbor for another. It often happens that members of the commission themselves cast votes for people who are late or have failed to appear.

In one case, an Estonian biologist working at Tartu State University voted not only for his wife, but also for 30 of his students, apparently because the student turnout was only around 70%,⁵ and he simply took it upon himself to take care of the "problem."

If a nonvoter wants to be sure that nobody casts a ballot in his name, the way to do so is to request an absentee voting certificate and then not use it. Any voter may request an absentee ballot by stating that he will be out of town on election day, and this ballot can be cast in any other electoral district in the country. (One may note that this undermines the principle of direct electoral representation.) While some absentee ballots are, of

course, cast—Latov (1974, p. 45) claims that in June 1970 half a million Moscovites did so—emigrants report that not doing so is a popular means of vote evasion. It is a technique welcomed by agitators and other members of electoral commissions, since absentee voters are stricken from their electoral rolls, thereby absolving them of any further responsibility (Friedgut, 1979, pp. 116–17; Zaslavsky and Brym, 1978).

How extensive is nonvoting? In the most thorough study to date, Friedgut (1979, p. 108) concludes that although it "may reach as high as 5 percent of the electorate, it almost certainly does not reach 10 percent." His estimate is based on interviews as well as on a comparison of the number of registered voters with census data about the voting age population. The resulting discrepancy of several million—ca. 2.5% of the Soviet population in 1970—represents the theoretically eligible voters who "wilfully, or by negligence, avoid the election system" (1979, p. 117). Although quite convincing, Friedgut ignores the group of migrants mentioned above, who are unaccounted for in either statistic since they are not living at a legal residence,⁶ and he does not count as nonvoters people whose ballots have been cast for them by somebody else, and who in our definition are nonvoters.

Another estimate of vote evasion is provided by respondents in the Soviet Interview Project who had at one time or other served on electoral commissions. Of the sample, 459, or 16.4%, had done so, most as agitators. This rate is credible since the system puts a premium on coopting many people into electoral commissions: in a single election (1975), 5.7% of the electorate served in this capacity (Soobshchenie, 1975, p. 20). Individuals who had this experience were asked how many eligible voters voted in their districts. While quite a few did not remember (26%), the largest group (45%) said that the turnout was 99% or 100%, another

20% said it was in the 88% to 98% range, and 9% chose a figure lower than that. References were primarily to local elections, which are more frequent, and to elections held between 1973 and 1981. Percentages reported for the Russian Union Republic (RSFSR) tend to be lower than for other regions.

These reports both support and challenge our proposition that only 90% to 95% of the Soviet voting-age population cast their ballots in elections held in the 1970s. The support comes from those mentioning turnouts of 98% and below, and, indirectly, from those who could not remember, since 100% is easier to remember than anything under it. But many did cite the official figure of from 99% to 100%. However, it is not known whether they were using the definition of nonvoting we have established here, which includes the nonuse of absentee voting certificates, voting by others, and omission from electoral rolls. As these are relative "fine points" more evident to individual abstainers than to persons serving on the commission, they could easily be overlooked.

We contend, therefore, that from 5% to 10% of the eligible voters in the USSR did not, in fact, vote during the 1970s. As the pursuant discussion will show, nonvoting is more prevalent among the sample of emigrants surveyed, for several reasons. For one, the sample is not representative of the entire Soviet population; it is, for example, more highly educated (see appendix) and presumably more disaffected. Both these factors are correlated with nonvoting, as we will show. In addition, it is conceivable that some respondents misreported their voting behavior, a tendency that is also known to affect calculations of voter turnout based on survey results in other systems. But as Sigelman (1982) has indicated, the substantive conclusions about the factors that influence voting or nonvoting are largely unaffected by the misreporting phenome-

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non, since the basic correlations remain the same.

There is one more reason why the rate of nonvoting is larger in our sample than we expect it to be for any single Soviet election. The relevant survey question refers to several elections falling into a five-year "last normal period" (LNP) before the respondent's life was significantly changed due to emigration plans. The question asked: "Did you yourself always vote in elections to the soviets during your last normal period, or did you sometimes not vote?" Reply categories distinguished among "always voted" (69%), "sometimes did not vote" (20%), and "never voted in LNP" (11%), but since there is little distinction between respondents who chose one of the latter two categories, they are merged here. Nevertheless, the reader should be aware that a subgroup of nonvoters failed to vote only sometimes. Since three elections were held during the primary period of reference, 1975-1979, the "sometimes" usually refers to one or two elections.⁷

Having discussed the general significance of nonvoting in undemocratic systems as well as its mechanics and extent in the USSR, we now turn to a closer analysis of its meaning, as discerned from the data of the Soviet Interview Project. We hypothesize that nonvoting is not haphazard, but is more often engaged in by specific subgroups of the Soviet population. Furthermore, nonvoting is, to a significant degree, an expression of political disaffection and active concern with politics rather than of apathy. While this will be shown through data analysis, it can also be concluded from our examination of the mechanics of nonvoting, which, we have seen, usually involves more of an effort than voting.

Data and Discussion

The Demographic Profile of Nonvoters

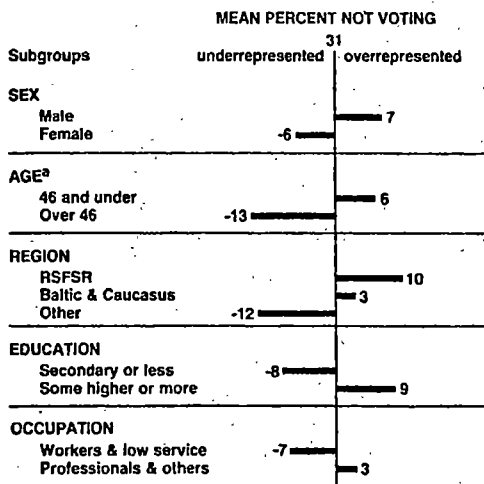
The first step of our analysis is to out-

line the demographic profile of the nonvoters and, by implication, that of the voters as well. Once the main fault lines of demographic differentiation are identified, the rest of the analysis will focus on an explanation of them.

Demographic characteristics such as age, sex, region, education, and occupation reveal major differentials. Thus, older Soviet subjects are much more likely to vote than younger ones. Females vote more often, and so do the less educated. Occupationally speaking, blue collar workers and service personnel are most likely to vote, whereas professionals and "others"—of whom those holding high-level technical jobs are most significant—are less likely to do so.⁸ Regional comparisons show a major emphasis on nonvoting in the RSFSR as contrasted with the rest of the USSR, with the partial exception of the Baltic and Caucasian republics. These patterns of under- and overrepresentation of subgroups among the nonvoting population are summarized in Figure 1.

Setting aside the Soviet regional phenomenon, this demographic profile of nonvoters tends to support the proposition that nonvoting in the Soviet Union is a political act qualitatively similar to voting in democratic systems. In the latter the same subgroups—males, the middle-aged, younger people (except the very young), and those with higher educational and occupational standing—tend to be overrepresented among voters. This phenomenon was first noted in Merriam and Gosnell's (1924) pioneering study of nonvoting in Chicago, and has since been confirmed in numerous other studies (Lipset, 1960, pp. 182-85; Rose, 1974, p. 698; Verba and Nie, 1972). As to the slightly curvilinear pattern of electoral participation by age found in democracies, our survey again shows a reverse equivalent for the youngest age-group, which was more likely to have voted than the preceding cohort (64% compared to

Figure 1. Demographic Profile of Nonvotes, Index of Representation (%)



^aThe cutting point is between the people born before and after 1931, with 1977 the median reference year for age during the "last normal period" in the USSR.

55%). In the Soviet case this higher participation rate—and at the same time lower evasion rate—may be due to the special attention paid to first-time voters, who are welcomed at polling places with speeches and flowers (Friedgut, 1979, p. 110).

While it is of some interest to note the demographic profile of nonvoters in the USSR, its real significance emerges only in the analytical elaboration to be pursued below. Region is an exception, as it appears largely to stand on its own. It is a contextual variable reflecting the properties of various communities rather than the individual characteristics of respondents. The higher rate of nonvoting in the RSFSR—which primarily reflects respondents from Moscow and Leningrad—remains in force even if one controls for other variables, including the political ones to be discussed below. This regional phenomenon is confirmed by other

sources, including official Soviet election statistics.⁹ It may be explained by a global awareness of politics present in the political centers of the country. Moreover, nonvoting is associated with a critical attitude toward Soviet politics, and for the two population groups represented in our sample—Russians and Soviet Jews—Moscow and Leningrad are the centers of attitudinal and behavioral nonconformism. While the link between dissent and nonvoting can be shown once we examine individual-level correlations, the nonconformist atmosphere in these two cities also has a group effect on respondents.

As for the other demographic variables, some are correlated with each other and are partly spurious. Thus, occupation is strongly correlated with education, which in turn is correlated with age ($r=0.39$) and with region ($r=-0.27$).¹⁰ The association between nonvoting and education is considerably weakened if one controls for age and region, which emerge as being more influential variables. Therefore, although the demographic profile of the nonvoters shows that they tend to have more education, we will omit further analysis of this variable due to its intercorrelation with the other demographic variables focused on here.

The impact of the remaining demographic factors—age, region, and sex—is linked to intervening variables such as interest in politics, nonregime political values, unsanctioned behavior, and loss of fear of sanctions. But before proceeding to these points, it is useful to consider the intensity with which the three main demographic variables are associated with nonvoting behavior. While Figure 1 already indicates the relative and cumulative effects of the demographic variables, Table 1 does so more precisely. Cumulatively, the largest differential, 45%, is between younger males from the RSFSR and older females from other regions.

A comparison of the cells also shows

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Table 1. Generational Differentials in Nonvoting, by Region and Sex

Region, Sex, and Age	Percentage Not Voting	Number of Cases
<i>Russian Union Republic (RSFSR)</i>		
Males:		
46 years old and younger	57	209
Over 46 years old	34	151
Females:		
46 years old and younger	45	458
Over 46 years old	19	269
<i>Other Republics</i>		
Males:		
46 years old and younger	33	458
Over 46 years old	16	212
Females:		
46 years old and younger	18	498
Over 46 years old	12	298

considerable consistency in the direction and intensity of nonvoting under various control conditions. An analysis of the relative effects of these variables shows that the influence of region is most intense, with an average percentage differential of 19%, closely followed by age (18.3%), with sex coming in third (11.5%).¹¹ Although the technique used here is simple, it meets the standards of methodological sufficiency and has the advantage of showing the thrust of the findings clearly.

Nonvoting Related to Nonconformist Political Attitudes and Behavior

Interest in politics. The analysis of competitive elections shows that people who are interested in public affairs vote more often than those who are not (Verba and Nie, 1972, pp. 118-19). If, in noncompetitive elections, nonvoting is in fact a more political act than voting, one would expect nonvoting to be similarly associated with a higher interest in politics. Our data about the Soviet case overwhelmingly suggest that this indeed is so. As illustrated by the first row in Table 2,

respondents who were "very" or "somewhat" interested in public affairs were much more likely not to have voted than those who had no interest or only a slight interest in politics.

When delving deeper into this relationship by bringing in additional variables, some of the demographic factors stand out even more, while others tend to disappear. The significance of sex, for example, virtually disappears among those "very interested" in public affairs, which for our analysis is substantively the most important subgroup. Nevertheless, Table 2 also shows that the significance of gender differences reappears increasingly strongly with decreasing political interest, which suggests that some other as yet unexplained variables are associated with the lower level of nonvoting among Soviet women. While noting this finding, we shall not try to explain it, since the political activism gap between men and women constitutes a pervasive and difficult to explain crosscultural phenomenon (Verba, Nie, and Kim, 1978, chap. 12).

In contrast to sex, nonvoting differentials by age and region emerge even more prominently among the respondents who

**Table 2. The Effect of Interest in Politics on Nonvoting
Controlling for Sex, Age, and Region**

Sex, Age, and Region	Percentage Not Voting ^a			
	Very Interested in Politics	Somewhat Interested in Politics	Only Slightly Interested in Politics	Not at All Interested in Politics
<i>Sex</i>				
Men	47 (447)	35 (439)	31 (148)	28 (169)
Women	43 (298)	27 (611)	20 (295)	12 (370)
<i>Age</i>				
46 years old and younger	52 (531)	36 (764)	28 (288)	22 (245)
Over 46 years old	28 (214)	17 (286)	16 (155)	13 (298)
<i>Region</i>				
RSFSR	55 (443)	39 (480)	31 (188)	23 (166)
Other	30 (302)	24 (570)	18 (255)	14 (373)
<i>Total</i> ^b	45 (745)	31 (1050)	24 (443)	17 (539)

^aNumbers in parentheses are Ns.

^bChi-square = 129.04; *df* = 3; significance = .00; Pearson's *R* = -.21; significance = .00.

are very interested in politics, and show a decreasing effect with decreasing interest (Table 2). This suggests that both these factors have an inherent significance for nonvoting behavior. In the case of region, the special characteristics of the RSFSR, as compared with other republics of the Soviet Union, are again highlighted. As for age, its role is likely to be partly linked to the life cycle—the elderly tend to be more passive—but there also appears to be a major generational difference that has little to do with physical age, but rather reflects a specific set of experiences associated with historical eras. As will be argued at more length below, the behavioral gap between the older generation and those who are young or middle-aged seems to reflect the different political experiences of the generations molded in

the Stalinist and post-Stalinist eras. Those raised within the Stalinist system seem much more likely to “go along” against their own inclinations. If so, this is a major empirical confirmation of the long-standing proposition that the death of Stalin in 1953 marked a turning point in Soviet politics, the consequences of which are still unravelling. What may be even more significant is the nature of the change our data reflect—namely, change in the behavior of the citizenry. The initial changes triggered after the great dictator's death were regime and policy changes; our data suggest that there has also been a long-term change in the political behavior of sizable population groups.

Political outlook. The association between a high interest in politics and

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**Table 3. Divergent Associations Between
Nonvoting and Various Motives for Emigration**

Motive for Emigration, by Frequency Mentioned	Percentage Not Voting	Number of Cases
Politics ^a		
0	21	1630
1	41	805
2	48	318
3	62	40
Discrimination ^b		
0	31	1672
1	30	901
2	26	205
3	40	15

^aChi-square = 171.1; *df* = 3; significance = .00; Pearson's *R* = .24; significance = .00.

^bChi-square = 2.76; *df* = 3; significance = .43; Pearson's *R* = -.02; significance = .16.

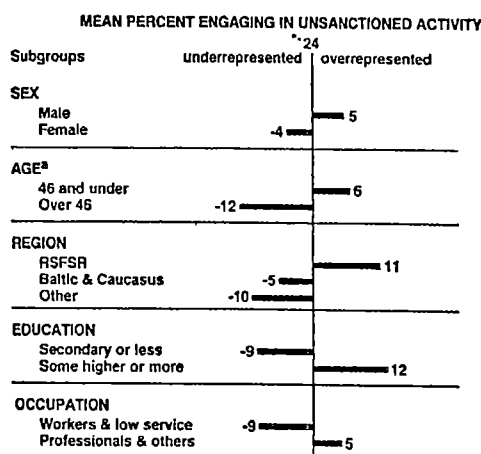
nonvoting is just one indication that the latter is linked to critical attitudes toward Soviet politics. This relationship is further illuminated by respondents' replies as to why they left the Soviet Union. Up to three responses were recorded to an open-ended question. They provide a rough but adequate indicator of basic political attitudes toward the Soviet system. If a respondent mentioned one or more political reasons for exit, we take this to signify a critical attitude toward the system; if no political reasons are mentioned, we take this to mean that political disaffection is not a significant component of the individual's outlook.

This operationalization of the "why did you leave" variable deliberately challenges the assumption that emigration per se signifies political disaffection. As empirical studies of emigration from the USSR have shown (Gitelman, 1977a; Karklins, 1981), a host of factors are involved, among which traditional motives for migration, such as the search for a better life or family reasons, are the most significant. In the case of Soviet Jews and Soviet Germans, a quest for cultural religious autonomy and escape from ethnic discrimination are also important,

but while these may be construed to be political in a broad sense, they hardly constitute a form of direct political disenchantment with the Soviet system of government. Thus, we take the view that the emigrant respondents represent a varied group of people, and unless they specifically note politics as a reason for emigrating, we assume that their attitudes toward the Soviet regime are either neutral or positive.

From this we hypothesize that individuals who mention a political motive for emigrating are less likely to have voted in the state-organized elections than those who did not. By the same token, one can show the explicitly political nature of nonvoting in the USSR by the voting behavior of those individuals who cite other motives for emigration, such as ethnic discrimination: the expectation here is that nonvoting is not significantly associated with this motive. As Table 3 shows, both expectations are confirmed by the data: while nonvoting is significantly more prevalent among the politically motivated emigrants (42% of the total sample), this is not the case for those for whom discrimination is the motive for emigration.

Figure 2. Demographic Profile of Dissidents, Index of Representation (%)



^aThe cutting point is between people born before and after 1931, with 1977 the median reference year for age during the "last normal period" in the USSR.

A critical attitude toward the Soviet regime is similarly correlated with nonvoting if other indicators are used. Thus, responses to a question about what is worth keeping in the Soviet system show that those saying "keep nothing" have the highest incidence of nonvoting (44%).¹² The nonvoting rate is 32% among respondents who thought that something was worth keeping (usually education or health care), and just 21% among those who said "don't know," or who gave no answer at all.¹³ This illustrates that nonvoting is more prevalent among people more hostile to the Soviet system, while voting tends to be strongest among those who have no opinions or do not want to express them.

Dissident behavior. Having linked nonvoting to political nonconformism by showing its correlation with an expressed interest in politics and with a political motivation for emigration, the next step is to test its relationship to overt unsanctioned behavior. In turning from an

attitudinal tendency to actual behavior, the expectation is that here the association between the two phenomena will be even closer if it is indeed true that nonvoting signifies deliberate political action rather than apathy.

The interview project included a sequence of questions about dissidence ranging from the attending of unofficial art shows to participation in open protest against some Soviet policy.¹⁴ While the latter is much more political in nature than the former, and one might weight them in a more refined index of dissident behavior, a simple summary score is sufficient to tap the broad range and differential intensity of unsanctioned behavior.

For some purposes it even suffices to use a dichotomous measure for the involvement in unsanctioned activities, such as in the summary demographic profile presented in Figure 2. A comparison of Figure 2 with Figure 1 shows that the overall pattern in the demographic profile of nonvoters and nonconformists is closely congruent, suggesting that the two behaviors may be associated. As Table 4 illustrates, the relationship is in fact of considerable strength, and intensifies as the dissidence score increases. Among those respondents who had engaged in more than one of the listed behaviors, nonvoting occurred in more than 50% of the cases.

The association between nonvoting and unsanctioned behavior is similarly strong if one uses other measures of dissent. The reading of unofficial publications (*samizdat* and *tamizdat* [work published abroad]) might even have been added into the unsanctioned behavior scale, but this was rejected because the patterns of association are so similar that nothing is gained analytically. In addition, including the *samizdat* question poses a methodological disadvantage: It was only asked of a third of the respondents, and this decreases the potency of the data. Nevertheless, there is one dimension that is

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Table 4. Nonvoting and Participation in Unsanctioned Activities

Activity	Percentage Not Voting	Number of Cases
Number of dissent activities engaged in ^a		
0	24	2119
1	44	451
2	61	129
3 to 6	78	94
Reading <i>samizdat</i> or <i>tamizdat</i> ^b		
Yes	51	280
No, not available	25	364
No, no interest	15	228
No, unexplained	12	52

^aChi-square = 235.13; *df* = 3; significance = .00; Pearson's *R* = .29.

^bChi-square = 96.87; *df* = 3; significance = .00; Pearson's *R* = -.07; significance = .01.

illuminated by a closer look at the *samizdat* variable, namely, the role of opportunity. Many forms of dissident behavior cannot be engaged in alone, but are linked to the presence of like-minded individuals and specific activities. One cannot attend an unofficial art show or join an unofficial study group if none is present in the vicinity. Similarly, one cannot read *samizdat* if none is available. Since nonvoting is not linked to opportunity, it can occur among people who have not participated in any other unsanctioned activity. This is clear from the second part of Table 4: a comparison of "no, not available" and "no, no interest" indicates that nonvoting tends to be higher among individuals who gave the former reply. Thus, a part of the gap between nonvoting and other unsanctioned behavior may be due to a lack of opportunity for the latter.

Another gap exists between a critical view of the Soviet system and actual behavior expressing this stand. This is illustrated by Table 5 and the significant subgroup of respondents (*N* = 525) who had a high interest in politics and mentioned political dissatisfaction as a cause for emigration, but never participated in dissident activity. While this is theo-

retically to be expected, since social science has often noted a gap between attitudes and behavior, the fear of sanctions plays a special role in a system such as the Soviet one, a point which is to be elaborated upon below.

It is the conclusion of this part of the analysis that nonvoting in the USSR is indeed associated with an explicit interest in politics, with critical political attitudes, and with dissident types of behavior. This finding is summarized in Table 5, which also shows the cumulative and relative effects of the three variables. Cumulatively, the respondents who expressed an interest in politics, who gave at least one political motive for emigration, and who had engaged in at least one form of dissident activity have the highest incidence of nonvoting (59%). In contrast, the lowest percentage score (16%) is found among those who were hardly interested in politics, listed no political motive for emigration, and had not participated in dissident activity. A calculation of the relative effects of the three variables on nonvoting shows dissident activity coming in first, with an average differential of 18%, followed by a critical view of the Soviet system (15%), and interest in politics (12%).¹⁵

Table 5. Nonvoting and the Relative Effects of Interest in Politics and Dissident Outlook and Behavior

Political Interest, Motive for Emigration, and Dissidence	Percentage Not Voting	Number of Cases
<i>Interested in politics^a</i>		
Political motive for emigration		
Dissident	59	388
Not dissident	38	525
No political motive for emigration		
Dissident	47	158
Not dissident	21	724
<i>Hardly interested in politics</i>		
Political motive for emigration		
Dissident	51	57
Not dissident	25	187
No political motive for emigration		
Dissident	26	69
Not dissident	16	669

^aDichotomized versions of the previously cited variables are used here; in the case of the "interest in politics" variable, the categories "very" and "somewhat" interested, and the categories "only slightly" and "not" interested, were collapsed.

The Role of Sanctions and Fear

Students of Soviet politics have traditionally assumed that much compliant mobilized behavior is due to a fear of sanctions. This assumption has also applied to electoral participation, although the form of sanctions against vote evaders has remained unspecified, implying a general black mark leading to negative consequences sooner or later (Mote, 1965, p. 78). One of the more interesting findings of the interview project is that in the 1970s practically no official sanctions for nonvoting were implemented, and that fearfulness is diminishing, although it persists among part of the population.

The survey raised the issue of sanctions by asking nonvoters: "Were any official actions taken against you as a result of this?" Only 1.8% said "yes," and the sanctions reported consisted mostly of reprimands, warnings, or threats. Thus, in essence, no major sanctions were applied, a finding that coincides with that

of Friedgut's in-depth interviewing. He adds, however, that among his respondents "there was general consensus that a refusal to vote out of simple political passivity would be noted somewhere and could at some point in the future cause unpleasantness to the nonvoter" (Friedgut, 1979, pp. 114-15).

Several points should be considered in this regard. For one, as noted in our analysis of the mechanics of nonvoting, this usually occurs in a nonconfrontational manner, through evasion. If nonvoting occurs openly or demonstratively the consequences are likely to be more severe.¹⁶ Furthermore, the nature of the pressures used to get the vote out—primarily peer pressure by "agitators"—suggests that the negative consequences of vote evasion may be similarly unofficial and indirect.

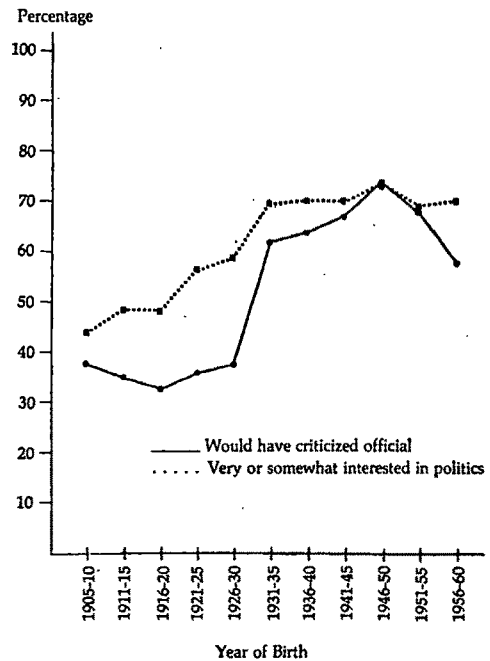
While official sanctions for nonvoting appear to have been rare in the 1970s, the fear of sanctions has not necessarily dissipated, and can still affect voter turnout. As comparative studies have shown

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(Irwin, 1974; Powell, 1982), the threat of sanction is sufficient to make many people comply with official pressures to vote. Unfortunately, our survey did not directly address the question of the role of fear and the perception of potential sanctions in voting decisions, but there is a way of testing the association by proxy. In one series of questions, respondents were asked about topics they talked about with various people in the USSR, including with whom they would have criticized a governmental official. While 83% would have criticized a governmental official with members of their families, only 55% would have done so with their closest friends. The percentages for others are much lower: 25% for relatives other than close family, 12% for co-workers, 4% for a boss, and 9% with no one. This constraint with close friends is a rough indicator of apprehensiveness about potential sanctions. Given the political development of the Soviet Union, the older generation is much more likely to feel such anxieties than the younger one, as the illustration in Figure 3 of a major breaking point between individuals born before and after 1931 dramatically confirms. Since the system began "softening" almost immediately after Stalin's death in March 1953, this break makes sense as a watershed between the generations who reached adulthood with and after Stalin's death, and those who spent most or all of their adult lives under Stalin.

This conclusion is confirmed if one examines the alternative life-cycle hypothesis that older people might be less likely to criticize government officials with their friends because they tend to be less interested in public affairs. As visually illustrated by Figure 3, the "interested in politics" line is quite close to the "would have criticized" line for younger age groups, but there is a noticeable gap for those born before 1931, indicating that something other than lack of interest explains the older generation's reticence to

Figure 3. Generational Gap in Percentage of Those Who "Would Have Criticized Government Official Among Friends," Compared with Age-Related Trends in Interest in Politics



discuss politics with friends. Viewing the respective relationships in another manner—by the crosstabulation in Table 6—one notes that compared to generational difference, the "interest in politics" variable explains less variance. While the percentage difference between generations remains in the 20% to 34% range, even if one controls for interest in politics, the difference between people within the same generation but with different levels of interest ranges between 11% and 15%.¹⁷ Thus, interest in politics is somewhat associated with the expression of criticism, but relatively less so than age is, supporting our thesis that this variable reflects the perception of how freely one may talk about politics, and the generational differences associated with this perception.

Table 6. Generational Differences in Fear to Criticize, by Interest in Politics

Age	Percentage Feeling Free to Criticize Officials With Friends:			
	Very Interested in Politics	Somewhat Interested in Politics	Only Slightly Interested in Politics	Not at All Interested in Politics
46 years old and younger	79 (165) ^a	68 (264)	53 (105)	54 (80)
Over 46 years old	45 (65)	43 (89)	33 (51)	23 (78)

^aNumbers in parentheses are Ns.

Lack of openness in expressing one's views on public affairs, even among friends, is found mostly among the older generation. This may be largely explained by historical experiences and long-term effects of the atmosphere of fear created in the Stalin era, but the fact that the middle-aged and younger respondents also include fearful individuals should not be disregarded. If fear limits the likelihood that people will engage in unsanctioned political behavior, this helps explain why some of the people we would expect not to vote in Soviet elections—namely, those

who are interested in politics and critical of the Soviet system—nevertheless did vote, as Table 7 shows. Among respondents who were very or somewhat interested in politics, those who felt that they could criticize officials with their friends were much more likely to abstain from voting than those who were fearful. Similarly, people who felt free in discussions with friends and had a critical attitude toward the Soviet system were much more likely to evade voting than those who held similar attitudes but did not talk freely with their closest friends. In sum,

Table 7. Fearfulness and Nonvoting Among Politically Interested and Critical Respondents

Political Interest, Motive for Emigration, and Fearfulness	Percentage Not Voting	Number of Cases
<i>Political Interest</i>		
Interested in politics, feels free to criticize		
Yes	46	378
No	25	205
Not interested in politics, feels free to criticize		
Yes	26	134
No	22	180
<i>Motive for Emigration</i>		
Political motive for emigration, feels free to criticize		
Yes	51	268
No	33	130
No political motive for emigration, feels free to criticize		
Yes	29	244
No	19	257

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fear inhibits nonvoting among individuals who are inclined toward it, but to a significantly lesser degree among the post-Stalinist generation.

Conclusions

In a political system where a choice between candidates is not part of the election process, and where elections are designed to enhance the position of the regime, the only option left to the dissenting citizen is not to participate. Typically, the authorities organizing noncompetitive elections are aware of this single remaining choice, and make every effort to prevent its being taken. Vote evasion, therefore, tends to become a political act both in principle and in form, since nonvoting frequently involves more effort than casting a ballot.

The meaning of voting and nonvoting must be assessed within the context of regime objectives. The goals most directly challenged by the nonvoter are the demonstration of unanimity and the regime's wish to have every single voter "accept a truth about the value of which there can be no doubt" (Linz, 1978, p. 48). In state-organized, single-candidate elections, the citizen is not only "acted upon" by the state, but is also required to be active himself, most of all in the affirmation of the regime's legitimacy and power and, by implication, of the illegitimacy and hopelessness of any alternative politics. Participation in the great charade of totalitarian and authoritarian elections is highly valued by the regime, because the act of each citizen in being part and parcel of it is a small but significant politico-psychological victory that is a key to general relations. Evasion thus constitutes a first step toward challenging other facets of the regime's claim for a monopoly of political thought, word, and activity.

While the political significance of nonvoting can be shown theoretically and contextually, the empirical substantiation

of our argument for the Soviet case has consisted of newly available emigrant survey data. Based on the assumption that such data can be useful for the study of variance in social and political phenomena, we have compared the voters and nonvoters in our sample and found that the two groups differ in their demographic and political profiles. Compared to those respondents who stated that they had always voted in Soviet elections, the nonvoters are younger, more frequently male, more highly educated, and more often from the RSFSR, especially Moscow and Leningrad. Politically, they tend to be more interested in public affairs than the voters, their attitudes toward the Soviet political system are more critical, and they have more often been engaged in unsanctioned cultural and political behavior. The nonvoter tends to be a nonconformist and dissident, and is also less fearful than others, especially the older generation.

These findings have several implications for our understanding of the Soviet system. The diminished fearfulness among the middle-aged and younger generations reflects a significant long-term movement away from the legacy of the Stalin era that is still noticeable in the behavior of the older generation. This change is bound to become more pronounced with time. Regionally speaking, Moscow and Leningrad emerge as centers of dissent, which is significant, since they also are the centers of official politics. What is more, the style of vote evasion provides insights into the contemporary political culture of the Soviet Union. Nonvoting is mostly nonconfrontational, and involves the pretense of absentee voting and illegal vote casting by third persons, the latter being not only tolerated, but even promoted by election workers concerned about fulfilling their official voting norm. These low-level representatives of the system, who themselves often have been pressed into service, collude with the

citizenry in circumventing one of the primary goals of the electoral process—namely, making every citizen individually cast his vote. This shows disbelief in the purpose of the process, as well as a propensity toward avoiding “true” nonvoting, which might be seen as a direct challenge to the system and might have major consequences. This circumvention rather than confrontation constitutes the prevalent form of unsanctioned political behavior, a finding closely in accord with other recent studies of Soviet political culture (DiFranceisco and Gitelman, 1984; White, 1979).

As noted above, the authoritarian regime’s acceptance of citizen passivity and evasion, as contrasted with the totalitarian regime’s insistence on activism, has been defined as a major criterion distinguishing the two. In the case of the Soviet Union and its contemporary electoral processes, although the regime insists on keeping the outward form and the official claim of 99.9% unanimity, it allows covert evasion and circumvention. This suggests a systemic change toward a quasi-totalitarian system that is authoritarian at heart.

Appendix: Sampling Frame Population and Sampling Procedures

The population eligible for the survey was composed of Soviet emigrants who arrived in the United States between January 1, 1979, and April 30, 1982; who were between the ages of 21 and 70; and who were listed by immigration aid societies, permitting the development of a systematic list of this population. Biographical abstracts yielded a sampling frame population totaling 33,618 individuals, of which 3,738 were selected into the general survey sample. The response rate was 79%.

The primary goals of sampling were to safeguard the principle of random selec-

tion, while at the same time approximating certain characteristics of the Soviet population more closely than a purely random sampling would allow. Consequently, a stratified random sample was selected, the four sampling dimensions being nationality, region of last employment in the USSR, highest education attained, and size of the city where last employed. Since 98.4% of the people in the sampling frame were Jews, all non-Jews in the sampling frame were included in the sample, in order to maximize ethnic diversity (resulting in 13% of the respondents being Russians and other non-Jews). The regional distribution of the emigrant population was quite varied, and thus the Soviet model could be approximated. As the sampling frame population was more highly educated than the Soviet adult population in large cities, the latter were used as a proportional guideline for stratifying on education. Only limited adjustments could be made for the overwhelmingly urban origin of the emigrants, although people from medium-sized cities were oversampled.

The referent Soviet population whose experiences the survey population can be expected to resemble most closely is the adult European Soviet population in large and medium-sized cities (Silver & Anderson, 1986; Tourangeau, O’Brien, and Frankel, 1983).

Notes

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1. Write-in candidates are not allowed, and if one nevertheless shows up on the ballot and the name of the official candidate is left untouched, the vote is counted for the official candidate. If the

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official candidate is crossed out, the ballot is counted as a vote against him (*Kommentarii*, 1979, art. 49). Although we are not focusing on negative votes, it should be mentioned that a number of such votes are cast in every Soviet election. According to official Soviet electoral statistics, and calculating for the entire USSR, the number of negative votes has never surpassed 1% in the postwar period. It usually is 0.02% in elections to higher-level bodies, and ca. 0.3% in elections to local soviets (*Itogi*, 1971, 1975; *Pravda*, March 4, 1979, March 1, 1980, June 26, 1982, March 4, 1984). In lower-level council elections, where the number of electors per district is only in the dozens, a candidate on rare occasions fails to receive the required 50% of the vote; thus 61 candidates out of 2.2 million failed to get elected in 1977 (*Izvestiia*, June 25, 1977).

2. See translations of official Soviet election returns as provided by the *Current Digest of the Soviet Press* (e.g., vol. 36, no. 10, pp. 13-14), or entire reports on elections (e.g., *Itogi*, 1971, 1975), which repeat the 99% for pages and pages.

3. Only from 1% to 5% of the electorate enter booths (Friedgut, 1979, p. 112; Mote, 1965, p. 80). Although people intending to cast a negative vote are most likely to enter booths, so are individuals intending to write a slogan or request on the ballot.

4. Juwiler (1976, p. 106) states that the most conservative estimate entails one million prisoners, but that the actual number could be double. No official Soviet statistics are available.

5. The regime's sensitivity to this low rate is illustrated by the fact that dissident Juri Kuk's reporting of it to a Western acquaintance by telephone was made into one point in the criminal charge against him. Kuk later died in a Soviet labor camp (Taagepera, 1984, pp. 76, 96).

6. The various reasons for statistical inexactness in Soviet census data are discussed in Anderson and Silver (1985).

7. Elections to union and autonomous republic soviets as well as lower level bodies were held in June 1975; local soviet elections occurred in June 1977, and elections to the Supreme Soviet in March 1979 (*Izvestiia*, June 21, 1975, June 25, 1977, March 7, 1979). On the basis of the 1977 USSR Constitution (Article 90) the interval between elections for the USSR, union-republic, and autonomous-republic supreme soviets was changed from four to five years, while that for the local soviets was changed from two to two and a half years.

8. Closer analysis of the associations of occupation with nonvoting shows that although there are some differences in addition to the rough one mentioned here, they are interrelated with age and sex, and detailed analysis does not yield any significant additional insights.

9. In the 1979 elections to the Supreme Soviet, 86% of the abstainers, as compared with 55% of the voters, were from the RSFSR (*Pravda*, March 7,

1979). The high incidence of nonvoting in Moscow and Leningrad is remarked upon by Zaslavsky and Brym (1978) and Friedgut (1979, pp. 117-18). Lev Kopelev (1984) notes that at one time reports circulated in Moscow that nearly one and a half million voters had abstained in an election, which would come to about 25% of the Moscow electorate.

10. The variables used here are in the form of a detailed categorical education variable, and dichotomous variables for age (born before and after 1931) and region (RSFSR/other).

11. In calculating the relative effect of the variables, I followed the logic and technique outlined in Rosenberg (1968, chap. 7).

12. The question was asked of a third of the respondents, and read: "Think for a moment about the Soviet system with its good and bad points. Suppose you could create a system of government in the Soviet Union that is different from the one which currently exists. What things in the present Soviet system would you want to keep in the new one?"

13. This subgroup is relatively large (13%). Those citing something as worth keeping were 69% of the sample; 18% fall into the "keep nothing" category. The association with nonvoting is significant at the .0002 level.

14. The questions were: "During your last normal period, did you ever . . . (a) attend an unofficial art show, poetry reading, or concert? (b) take part in an unsanctioned study or discussion group? (c) distribute *samizdat* [self-published] or *tamizdat* [published abroad]? (d) take part in an open protest against some Soviet policy? (e) take part in a strike or protest at work? (f) belong to any (other) unofficial groups or take part in any (other) unsanctioned activities?"

15. For methodology see source listed in note 11.

16. The refusal to vote of the well-known dissenter, Larissa Bogoraz, was part of the reason cited by the authorities to reject remission of her exile. Demonstrative voting has also been used as a pretext for denying exit visas (Friedgut, 1979, p. 124). Zaslavsky and Brym (1978, p. 368) note that the refusal to vote has led to students being expelled from university. There is also one documented case in which the writing of an "anti-Soviet" comment on a ballot in 1971 led to an arrest and a five-year sentence in a Soviet labor camp (*USSR News Brief*, April 30, 1983, p. 3).

17. Since the variables have different subcategories, and this could provide misleading results (Rosenberg, 1968, pp. 176-77), a test was made to determine whether the relative difference remains the same if only two categories are used for both. This in fact is so (28% vs. 18%).

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RATIONAL CHOICE AND REBELLIOUS COLLECTIVE ACTION

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A basic problem for a rational choice theory of rebellious collective action is to explain why average citizens would participate in such behavior, since they have nothing to gain (they will receive benefits of successful rebellion, in terms of public goods, regardless of whether they take part or not), but much to lose (rebellious behavior may be quite costly). According to the conventional private interest or "by-product" theory, the incentive to participate must come from the expectation of receiving selective benefits; but since average citizens in a general case cannot expect substantial private material rewards, the relevant selective benefits must be psychological in nature. In contrast to the model of private interest theory, a public goods model is proposed, stipulating that the value of rebellion in terms of public goods can be a relevant incentive for participation. Using data from surveys conducted in New York City and Hamburg, West Germany, we investigate the relationship between participation in rebellious political behavior and measures of the incentives of public goods and private interest. The results do not support predictions of the private interest model in regard to nonmaterial selective incentives. Hypotheses of the public goods model are supported.

Consider the following situation: An individual, for whatever reasons, holds a very negative opinion of the political system under which he or she is governed, and there exists an opportunity to participate in a social movement that seeks to effect political change by means of rebellious collective action. This individual is faced with a decision between two courses of action: taking part in rebellious behavior, or staying at home while wishing the rebels well. Rational individuals will compare the benefits and costs of participation with those of inactivity, and

choose the course of action in which their expected utility is maximized.

If rebellious collective action is stipulated by definition to refer to behavior by nonelites that (1) deviates from legal norms regarding acceptable forms of political participation, and (2) has broad political objectives of changing governmental structure, policy, or personnel, then it can be shown (Salert, 1976) that, under conventional assumptions, the rational choice of an average citizen in the general case is to be an inactive "free rider," reaping the public benefits of rebellious collective action should it be

successful, while avoiding the private costs. However, history of course shows that average citizens do take part in rebellious collective action, sometimes in quite large numbers. Therefore, from a conventional rational choice perspective, most participants in instances of rebellious collective behavior would appear to be acting nonrationally.

In this paper we review the motivational assumptions of a rational choice explanation of rebellious collective action, propose a public goods alternative to the conventional private interest model, and conduct empirical tests of the public goods argument versus the private interest argument by means of survey research.

Rational Choice Models of Rebellion

A simple benefit-cost model of the expected utility of participation versus inactivity may be stated as:

$$E(R) = (p_n + p_i)V - E(C_r) \quad (1)$$

$$E(I) = p_n V - E(C_i), \quad (1a)$$

where $E(R)$ is the expected utility of rebellious collective action; V is the value of rebellious collective action in terms of public goods; p_n is the probability of success of rebellious collective action given that the individual is neutral; p_i is the individual's estimation of the probability that participating in rebellious collective action will make a difference in the likelihood that public goods will be the result; $E(C_r)$ is expected private costs to the individual of participating in rebellious collective action; $E(I)$ is the expected utility of inactivity; and $E(C_i)$ is expected private costs to the individual of inactivity.

Now for a person who is not a leader of a social movement, p_i will be approximately equal to zero, since the participation of a single average citizen will have a negligible impact on the likelihood of suc-

cess of rebellious collective action. Given that $p_i = 0$, the model reduces to

$$E(R) = p_n V - E(C_r) \quad (2)$$

$$E(I) = p_n V - E(C_i). \quad (2a)$$

Under this condition the public goods value of rebellious collective action, V , cannot be an incentive to participate. Whether the outcome of successful rebellious collective action is a change in public policy or a transformation of the entire governmental structure (a revolution), this good will be provided to all citizens regardless of their participation. Consequently, unless the costs of inactivity exceed the costs of participation, the rational choice for any average citizen is to be an inactive free rider. The simple benefit-cost model thus reduces to the postulate that average citizens participate rationally in rebellious collective action only under the pressure of coercion.

A coercion model of rebellious collective action is implausible, however, since most dissident groups do not have sufficient resources to force very many average citizens to join the movement against their will. In the special case of a dissident group that controls territory, coercion could be an incentive; but in order to control territory the group must be relatively large, and the coercion model cannot explain how the movement initially attracted enough members to become relatively large. Therefore, if it is reasonable to assume that $E(C_i)$ will be approximately equal to zero in the general case, the simple benefit-cost model reduces further to

$$E(R) = p_n V - E(C_r) \quad (3)$$

$$E(I) = p_n V. \quad (3a)$$

According to this specification of an average citizen's decision calculus, in the general case there can be no incentive for such individuals, if they are rational, ever

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to participate in rebellious collective action.

In order to explain why it might be rational for an average citizen to take part in rebellious collective action, Olson (1971), Tullock (1971), and Silver (1974) introduce the factor of selective incentives: private personal rewards that the individual can expect to receive only by participating. Olson and Tullock emphasize material inducements such as power and status rewards, as well as financial gain resulting from direct payments or looting. Tullock also includes an "entertainment" motive—enjoyment of the adventure of rebellious collective action for its own sake. And Silver (1974, pp. 64–65), in addition to private material rewards, expands Tullock's entertainment motive to include, under the rubric of "psychic income," "the individual's sense of duty to class, country, democratic institutions, the law, race, humanity, the rulers, God, or a revolutionary brotherhood as well as his taste for conspiracy, violence, and adventure." The private interest model of Olson, Tullock, and Silver may be written as:

$$E(R) = E(M) + E(P) - E(C_r), \quad (4)$$

where $E(M)$ is expected private material benefits from rebellious collective action, and $E(P)$ is expected private psychological benefits from such behavior.

Neither Olson nor Tullock attach much causal significance to the $E(P)$ term in equation (4). However, as Salert (1976, p. 44) points out, the problem with a private interest theory which emphasizes the $E(M)$ term is that if an average individual's expectation of receiving material benefits is assumed to be reasonable, power and status incentives must be assumed to be negligible, leaving income rewards as the principal private material benefit. But it is implausible a priori to postulate that most participants in rebellious collective action are either mercenaries or else are taking part in the hope

of personal gain from pillage and plunder. This is why Mueller (1979, p. 146) concludes that "the economic theory of revolution based on the individual maximizing calculus seems much better suited to explaining the coup d'état, where the number of actors is small, the odds calculable, and the stakes seemingly large, than it is at explaining 'grass roots' revolutions."

The dilemma of the private interest theory is that if $E(M)$ and $E(P)$ are negligible in the general case, then individually rational, average citizens will be free riders. And if average citizens choose to be free riders, then rebellious collective action is likely to fail, resulting in a continuation of the status quo. Thus what is individually rational is collectively irrational, since individuals who attach a high public goods value to rebellious collective action and choose to free ride are likely to obtain a less preferred outcome than if they all behave irrationally and participate.

It follows that collectively rational individuals might estimate p_i to be significantly greater than zero because they recognize that free riding is collectively irrational in the case of rebellious collective action, where group size and cohesion can be a factor of critical importance. And if p_i cannot be assumed necessary to be negligible, then the public goods value of rebellious collective action becomes a relevant consideration in the decision calculus of a rational average citizen.

An alternative way of reintroducing the public goods component, V , into a rational choice model is to stipulate an additional p term, derived from social learning theory. Bandura (1973, p. 206) states that vicarious reinforcement—perception of positive outcomes accruing to others—is a powerful source of motivation, and that "observational incentives play an especially important role in social activism, for here the chances of quick success are poor, but protest behavior is

partly sustained by the long-range attainments of groups that have persevered in their efforts." The individual responding to contingencies of vicarious group reinforcement can be likened to a "calculating Kantian" for whom, as Hardin (1978, p. 5) argues, "the issue will not be whether he himself benefits more from his own contribution than the latter costs him (as in Olson's logic), but whether he and the group of like-minded activists benefit more from their group actions than those actions cost." This introduces the possibility that the influence of the group on the provision of public goods, denoted p_g , may play a role in the decision calculus by which a rational individual chooses whether to participate in rebellious collective action.

Returning to the simple benefit-cost model of equation (3), rejection of the assumption that $p_i = 0$ and introduction of group influence on the provision of public goods, p_g , results in the following specification:

$$E(R) = (p_n + p_g + p_i)V - E(C_r) \quad (5)$$

$$E(I) = p_n V. \quad (5a)$$

According to utility theory, R occurs if $E(R)$ is greater than $E(I)$; that is, if $[E(R) - E(I)] > 0$. If $E(R) - E(I)$ is replaced by the right-hand sides of equations (5) and (5a), we obtain

$$[(p_n + p_g + p_i)V - p_n V - E(C_r)] > 0. \quad (6)$$

Solving the brackets, we obtain

$$[p_n V + p_g V + p_i V - p_n V - E(C_r)] > 0. \quad (7)$$

Since $p_n V$ is irrelevant, we get as a condition for performing R

$$[(p_g + p_i)V - E(C_r)] > 0. \quad (8)$$

Thus, the higher the value of $(p_g + p_i)V$ and the lower the value of $E(C_r)$, the more often R is expected to occur in a popula-

tion. This is a model for a public goods theory of rebellious collective action.

Now let us expand the model to include selective incentives. We assume that for average citizens in the general case, expected private material benefits, $E(M)$, are negligible. This leaves expected private psychological benefits. Tullock's entertainment motive is one example of intrinsic psychological gratification derived from rebellious collective action. Silver's shopping list of "psychic income" is essentially a residual factor that encompasses anything conceivably intangible. We focus more specifically on a class of "soft" nonmaterial selective incentives that entail social rewards (see Opp, 1986). In particular, individuals may expect to receive (1) affiliation rewards such as meeting new friends and feeling solidarity with a group, and (2) psychological gratification from conforming to the expectations of reference persons about how they should behave, if such expectations are subjectively important. The full model, including public goods and soft selective incentives may be written as

$$E(R) = (p_g + p_i)V + E(F) + E(A) + E(O) - E(C_r), \quad (9)$$

where $E(F)$ is the expected entertainment or "fun" value of participation in rebellious collective action; $E(A)$ is the expected social affiliation value of participation; and $E(O)$ is the expected value of conforming to behavioral norms of important others.

Research Design

Empirical evaluation of rational choice models of rebellious collective action requires a research design in which the individual is the unit of analysis. Experimentation under controlled laboratory conditions would be the most appropriate method for testing rational choice hypotheses, but, since rebellious collec-

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tive action is illegal and may involve the use of violence, an experimental design is not feasible due to problems of verisimilitude and ethical constraints. Thus the realistic alternatives are either to engage in armchair speculation using anecdotal evidence (e.g., Tullock, 1971), or to test hypotheses by means of survey research.

The disadvantage of survey research is that the context of particular choice situations is uncontrolled. An interview schedule can be designed to collect data on respondents' current assessments of the value of public goods, their current estimation of individual and group influence on the provision of public goods, their current expectation of receiving soft rewards from rebellious collective action, and their current expectation of the cost of such behavior. However, it must be assumed that the values and expectations measured at the time of the interview are applicable to past behavioral decisions and future decision potentials, regardless of variation in the context of the choice situation. This is a well-known and much-debated general problem inherent to survey research on the relationship between attitudes and behavior, for which there is no satisfactory solution. Therefore, while acknowledging that survey-based tests of the relationship between attitudes and behavior never can be decisive, due to the problem of uncontrolled situational factors, we use the survey method on the grounds that, in the absence of a more feasible alternative, survey evidence is better than no evidence.

Assuming that one does not reject the survey method *per se*, the study of rebellious collective action by means of survey research still must overcome two basic obstacles. First is the problem of variation. Acts of rebellious political behavior are rare events in societies not actually undergoing a mass rebellion, yet the population chosen for the survey must be one in which rebellious behavior has

occurred with sufficient frequency to permit statistical analysis. Second, the environment of the survey must be conducive to free expression.

The structural condition most conducive to freedom of expression is the presence of a democratic political system. The frequency of rebellious collective action has been comparatively low, however, in most contemporary democracies. If one is effectively restricted to democratic nations, the United States and West Germany are appropriate sites because of the relatively high levels of political protest that occurred in the former country from the mid-1960s until the mid-1970s, and in the latter country from the late 1960s, with ebbs and flows, until the present.

Appropriate populations to study within the United States are residents of urban areas, especially nonwhites, and students and faculty at large universities. The general public of New York City and students and faculty at Columbia University and New York University (NYU) were considered to be suitable instances of city and campus because of their past history of comparatively frequent political protest. Our American data were collected by Response Analysis Corporation of Princeton, New Jersey, a survey research firm with expertise in designing and implementing surveys on sensitive topics (e.g., drug use, violence in the home). During the spring and summer of 1978 personal interviews were conducted with a probability sample of 778 adults residing in the five boroughs of New York City;¹ also, a random sample of 240 undergraduates, graduate students, and faculty in the College of Liberal Arts at Columbia and NYU completed a supervised, self-administered version of the same questionnaire in May and June of that year (for complete details of the sampling procedure see Muller, Jukam, and Seligson, 1982).

In the Federal Republic of Germany an

appropriate group to study is opponents of nuclear power. Especially since 1974, numerous legal and illegal protest actions have occurred against particular atomic power stations and against the utilization of nuclear energy in general.

We expected opponents of nuclear power to live mainly in large cities and in the vicinity of atomic power stations. The sampling sites selected were Eimsbüttel, a district of Hamburg where many counter-culture people reside, and Geesthacht, a small town near Hamburg with an atomic power station in the neighborhood. A random sample was drawn from the populations of the two locations. If the respondent expressed a positive general attitude toward nuclear energy, only personal background data were collected. Full interviews were conducted with 187 opponents of nuclear energy, who were asked to give addresses of other opponents of nuclear power. The additional snowball sample consisted of 211 respondents. Differences between the random and snowball samples with respect to the demographic characteristics of the respondents were not sufficiently large to warrant separate analyses (for complete details of the sampling procedure and the analyses of the subsamples see Opp et al., 1984).

The Hamburg interviews ($N=398$) were conducted by students from the University of Hamburg who were carefully trained. To test for the existence of agreement bias, each variable used in the following analysis was correlated with a social desirability scale. Bivariate correlations were all within the range of $\pm .10$. Also, no significant correlations were found between properties of the interviewers (e.g., gender, attitude toward nuclear energy) and responses of the interviewees.

Measurement of the Dependent Variable: Rebellious Collective Action

The concept of rebellious collective

action entails acts of aggressive political participation (see Muller, 1979, pp. 4-7). Aggressive political participation in the New York City study is measured following a procedure developed by Muller (1979, pp. 37-68). Respondents were questioned about their past performance of and future intention to perform a series of behaviors, legal and illegal, described as actions that people may take to accomplish their political goals and objectives.² The set of aggressive behaviors includes (1) participating in an unofficial or "wild-cat" strike, (2) participating in a group that refuses to pay taxes, (3) seizing factories, offices, or other buildings, (4) participating in fights with police or other demonstrators, (5) participating in a group that wants to overthrow the government by violent means. A composite Aggressive Participation Potential scale (R_p) was constructed by summing participation weighted by intention across the five behaviors.³ A simple Aggressive Behavior index (R_b) is the sum of the number of aggressive behaviors performed in the past by the respondent.

The correlation between Aggressive Participation Potential and Aggressive Behavior is $r = .72$. Weighting participation by intention thus produces a somewhat different measure than the index of past participation. The principal difference is that some respondents who have not previously taken part in aggressive activity express intention to perform one or more of the behaviors in the future, and many of these respondents receive intermediate scores on Aggressive Participation Potential, despite their zero score on Aggressive Behavior.

The range of the Aggressive Participation Potential scale is 0-21,⁴ with a mean of 2.2 and a standard deviation of 2.7. Thirty-three percent of the respondents score zero on Aggressive Participation Potential. In comparison, the range of the Aggressive Behavior index is only 0-4, and fully 87.6% of the respondents receive a score of zero on this variable.

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Because of the restricted range and extreme skewness of the index of past behavior, explanatory variables can be expected, *ceteris paribus*, to correlate less strongly with it than with the measure of participation potential. However, the pattern of relationships should not be affected. Inclusion of both indicators of rebellious collective action thus affords a useful consistency check in the testing of hypotheses. In particular, a hypothesis that holds for the prospective measure of aggressive participation potential at the time of the interview, but not for the retrospective index of previous aggressive participation, will be suspect in regard to its validity as a causal explanation.

In the Hamburg survey the referents of aggressive participation are (1) participation in a demonstration which is forbidden, (2) engaging in resistance if the police attack demonstrators, (3) breaking through barricades and the like during demonstrations, (4) painting slogans against nuclear plants and the like on walls of houses, (5) participation in an occupation of a construction site for nuclear power plants, (6) attacks and sabotage against nuclear power plants, and (7) attacks on individuals responsible for the construction of nuclear power plants. Aggressive Participation Potential and Aggressive Behavior variables were constructed according to the same procedure as that used in the New York City study.⁵ The correlation between Aggressive Participation Potential and Aggressive Behavior in the Hamburg study is $r = .88$.

Measurement of the Independent Variables

The Hamburg interview schedule, designed in 1981 and administered in 1982, contains a more complete set of measures of the independent variables than the New York City interview schedule. The New York City interview

schedule is limited to an indirect indicator of p_i , the influence of the individual on the provision of public goods; and measures of p_g , the influence of the group on the provision of public goods, and V , the public goods value of rebellious collective action. The Hamburg interview schedule includes measures of p_i and V , as well as measures of $E(F)$, the entertainment value of rebellious collective action, $E(A)$, the social affiliation benefit of such behavior, $E(O)$, the benefit of conforming to behavioral norms of important others, and $E(C_r)$, the costs of performing rebellious collective action.

The public goods value of rebellious collective action. A person who is proud and respectful of the political institutions under which he or she is governed, and who believes that they operate to promote justice and protect the basic rights of citizens, would be unlikely to consider acts of rebellious behavior as being in the public interest—indeed, such a loyal citizen would be likely to regard change resulting from rebellious behavior as a public “bad.” By contrast, a person who is fundamentally alienated from the existing government and political institutions would be likely to regard change resulting from rebellious behavior as a public good. Thus a very basic indicator of the public goods value of rebellious collective action is the extent to which an individual is hostile to or alienated from the political system in general. In addition, for opponents of nuclear power, the utilization of nuclear energy is a public bad and less or no utilization is a public good.

In the New York City and Hamburg surveys, an indicator of the public goods value of rebellion, denoted V_s , is measured by responses to a series of questions about the extent to which the individual supports the political system.⁶ In the Hamburg survey, an indicator of the extent to which the existing utilization of nuclear energy is considered to be a

public bad, denoted V_n , is measured by yes-no responses to statements such as "I personally feel threatened by atomic power stations" and "to be sure, I am against atomic power stations, but they really do not concern me that much."

Perceived influence on the provision of public goods. Respondents in the Hamburg survey were presented with a battery of questions designed to measure the p_i term, perceived influence of the individual on the provision of public goods, that included items such as "After all, it is superfluous that I am active against the construction of atomic power stations because I have no influence," and "Everyone who is active against atomic power stations makes a small contribution."⁸ The Personal Influence scale, with a range of 0-1, has a mean of .73; thus, the average respondent in the Hamburg study estimates that he or she has considerable personal influence on the provision of public goods.

The New York City interview schedule did not include a direct measure of the perceived influence of the individual on the provision of public goods. As an indirect indicator of p_i we used a question that measures perceived importance of participation in general, denoted p_i^* .⁹ We assume that individually rational respondents will tend to believe that participation by people like themselves is unimportant because the participation of a single person makes little or no difference in the probability of the success of collective political behavior, except in the case of very small groups. By contrast, we assume that collectively rational respondents, who are cognizant of the dilemma arising from individually rational free riding, will tend to believe that participation by people like themselves is indeed important. The mean of the Importance of Participation variable is .65 on a scale of 0-1.

The influence of the group on the provi-

sion of public goods—the p_g term—is measured in the New York City survey by a question about the extent to which dissident groups are perceived to have helped or hurt their cause by engaging in rebellious collective action. Six rebellious actions were evaluated in regard to whether they (a) had definitely helped the cause of the groups involved, (b) had helped the cause somewhat, (c) had neither helped nor hurt the cause, (d) had hurt the cause somewhat, or (e) definitely had hurt the cause.¹⁰ The Efficacy of Group Aggression scale constructed from these items, when transformed to a range of 0-1, has a mean of .25. Thus, the average New York City respondent estimates that aggressive tactics by dissident groups in the past have not enhanced the provision of public goods.

The entertainment motive. The F term is measured by three items with which a respondent could more or less agree: (1) "If I protest against the construction of atomic power stations, this activity is fun for me"; (2) "although I am an opponent of nuclear energy, I find it somehow unpleasant being active against atomic power stations"; and (3) "I feel inhibited to show that I am against atomic power stations." A factor analysis (principal components) reveals unidimensionality (the first component explains 60% of the variance). We did not seek to determine a subjective probability of receiving entertainment from rebellious behavior because we assumed that respondents already know with certainty what entertainment value such behavior has for them.

Behavioral norms of important others. Measurement of the $E(O)$ term entailed a complex procedure. Respondents first were asked to state what most people whose opinion is important to them (like friends and relatives) think about their participation in the anti-nuclear move-

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ment. The possible responses were that such people (1) consider participation in the movement to be rather good (positive reference persons); (2) consider participation in the movement to be rather bad (negative reference persons); (3) hold differing opinions (mixed reference persons); and (4) are indifferent. Respondents choosing the positive or negative options were asked how important the opinions of the reference persons are, using a five-point scale ranging from "does not matter at all" to "matters very much." They were then asked to state whether, among the persons whose opinions are important, there are also persons who hold the opposite opinion. If the response to this question was affirmative, the importance question was repeated in regard to reference persons with the opposite opinion. If respondents chose the option of mixed reference persons they were asked how much they care about conforming to the opinion of each of the two sets of reference persons (positive and negative).

From the answers to this battery of questions we constructed a scale measuring the utility of following the expectations of important others with regard to being active. For respondents indicating only reference persons who expect activity (positive others), the more these expectations mattered to them, the higher their scores were. For respondents indicating only reference persons who expect inactivity (negative others), the less these opinions mattered, the higher their scores were. If respondents indicated mixed expectations, we first assigned scores for following the expectations of positive reference persons, and then for following the expectations of negative reference persons; the more respondents cared about following expectations of being active, and the less they cared about following expectations of being inactive, the higher their scores were. The second score was then subtracted from the first score.

Finally, we weighted the utility of

following the expectations of important others by the respondent's subjective probability that the reference persons hold the perceived expectations. This was determined by asking how certain respondents were (in regard to five categories of certainty scored 0, .25, .5, .75, and 1.0) that the reference persons in question are positive, negative, hold mixed opinions, or are indifferent.

Social affiliation rewards and costs of rebellious collective action. Respondents were presented with a list containing seven positive reactions and seven negative reactions of other persons or institutions that might be incentives or disincentives for participation in the anti-nuclear movement. The positive sanctions entailed social rewards such as receiving social approval from other opponents of nuclear power, feeling solidarity with other opponents of nuclear power, and getting to know interesting people. The negative sanctions entailed costs such as being labeled "leftist" or "crazy," being injured by the police, and *Berufsverbot* (prohibition from employment in the public sector). Respondents were asked, for each of the 14 sanctions, first to evaluate them on a five-point scale of very bad to very good, scored from -1 to +1; and second, to state the likelihood that they would occur (using the five-point certainty scale mentioned above). The utility of each sanction then was multiplied by the subjective probability of its occurrence.

A factor analysis (principal components) of the expected utilities of positive and negative sanctions yielded four factors with eigenvalues greater than 1. Most of the positive sanctions loaded highly on the first factor and most of the negative sanctions loaded highly on the second factor. Another factor analysis was performed, limiting the number of factors to two. Under this constraint all of the positive sanctions loaded on the first factor

and all of the negative sanctions loaded on the second factor. An Expected Utility of Positive Sanctions scale, the indicator of the $E(A)$ term, was constructed from the factor scores of the first factor, and an Expected Utility of Negative Sanctions scale, the indicator of the $E(C)$ term, was constructed from the factor scores of the second factor. These variables are standardized to have a mean of 0 and a standard deviation of 1. Since most respondents assign a positive utility to positive sanctions (i.e., social rewards), scores in the high range of the Expected Utility of Positive Sanctions scale indicate a subjective probability that positive sanctions are likely to occur, whereas scores in the low range indicate a subjective probability that positive sanctions are unlikely to occur. By contrast, most respondents assign a negative utility to negative sanctions (i.e., costs), so high scores on the Expected Utility of Negative Sanctions scale (which are mostly around 0) indicate a subjective probability that negative sanctions are unlikely to occur, whereas low scores (which are negative) indicate a subjective probability that negative sanctions are likely to occur. Thus it should be kept in mind that if negative sanctions are a disincentive for performance of rebellious political behavior, there should be a positive correlation between the Expected Utility of Negative Sanctions scale and the Aggressive Participation Potential and Aggressive Behavior variables.

Results

The private interest model predicts a lack of association between rebellious collective action and its public goods value because of individually rational free riding. People who attach high public goods value to rebellious collective action are expected to realize that they will receive the public goods of successful rebellion regardless of whether they participate or not; therefore, they should be

no more likely to score high on indicators of R than people who attach low public goods value to rebellious collective action.

The public goods model allows for the possibility that group rather than individual influence on the provision of public goods may be a relevant factor in a rational citizen's decision calculus, and that collectively rational individuals also may estimate individual influence to be non-negligible because they believe individually rationally free riding will reduce the likelihood of success. Empirically, the expectation of the public goods model is to observe nontrivial correlations between R and V as weighted by p_i and/or p_g ; moreover, weighting V by p_i and/or p_g should improve the correlation in comparison to V alone.

The predictions of the private interest and public goods theories were tested across four subpopulations of the New York City survey: whites and nonwhites from the general public and from the universities. We controlled for these subpopulations in order to determine if results hold across individuals who (1) have differing objective social background characteristics (white versus nonwhite), and (2) reside in differing community contexts (general public versus university) that might be more or less conducive to rebellious collective action.

Correlations between the two indicators of rebellious collective action, the Aggressive Participation Potential scale, denoted R_p , and the Aggressive Behavior index, denoted R_b , and the public goods component variables and product terms are listed in Table 1.¹¹ Comparison of the fourth row with the first row shows that the correlations between $(p_g + p_i^*)V_s$ and R_p and R_b exceed those between V_s and R_p and R_b in every case except that of R_b among whites in the university context, where the correlation is unchanged. Since p_i^* , the Importance of Participation variable, is only an indirect indicator of

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Table 1. Correlations between Rebellious Collective Action and (1) The Public Goods Value of Rebellion and (2) Influence on Provision of Public Goods: New York City Sample

Scales		General Public				University			
		Whites		Nonwhites		Whites		Nonwhites	
		Aggressive Participa- tion Potential (R_p)	Aggressive Behavior Index (R_b)	Aggressive Participa- tion Potential (R_p)	Aggressive Behavior Index (R_b)	Aggressive Participa- tion Potential (R_p)	Aggressive Behavior Index (R_b)	Aggressive Participa- tion Potential (R_p)	Aggressive Behavior Index (R_b)
Political Support/ Alienation	V_s :	.36	.24	.40	.19	.40	.26	.72	.63
Importance of Participation	p_i^* :	.10	.02*	.13	.10	.08*	.05*	.46	.32
Efficacy of Group Aggression	p_g :	.31	.21	.17	.06*	.46	.20	.18*	.08*
Product Terms ($p_g + p_i^*$) V_s :		.38	.26	.46	.24	.44	.26	.77	.66
	$p_g V_s$:	.39	.30	.42	.26	.45	.26	.65	.52
Number of Cases		349	361	316	327	202	207	23	24

*Not significant at the .05 level (for table entries).

individual influence on the provision of public goods, the fifth row gives the product term for group influence only. Comparison of the fifth row with the first row shows that for the general public and for whites in the university context, the correlations between $p_g V_s$ and R_p and R_b exceed those between V_s and R_p and R_b in every case except one, R_b among whites in the university context, where the correlation is unchanged. For the small subsample of nonwhites in the university context, weighting V_s by p_g reduces the correlation with R_p and R_b . In sum, in line with the specification of the public goods model, weighting the public goods value of rebellious collective action by individual and group influence on the provision of public goods results in nontrivial correlations with indicators of participation in rebellious behavior; and the magnitude of these correlations is usually greater than that observed for the unweighted public goods value, a finding consistent with general rational choice theory.

The results from the Hamburg survey are presented in Table 2, where we have two public goods variables, V_s and V_n ,¹² and a direct measure of the p_i term. The correlations between the public goods variables and the indicators of rebellious collective action are nontrivial and, as the public goods model specifies, multiplying the public goods variables by perceived individual influence on provision of the public goods again raises the magnitude of the correlations.

The predictions of the public goods theory thus are supported cross-nationally as well as across subpopulations of New York City. Weighting the public goods value of rebellious collective action by perceived individual and/or group influence on the provision of public goods enhances (consistently but not dramatically) the correlations with current potential for performing rebellious collective action and past performance. This result is in accordance with an expected utility model of decision making, and lends empirical support to our

Table 2. Correlations between Rebellious Collective Action and
(1) The Public Goods Value of Rebellion and (2) Influence on Provision of
Public Goods: Hamburg Sample

Scales		Aggressive Participation Potential (R_p)	Aggressive Behavior Index (R_b)
Political Support/Alienation,	V_s :	.47	.43
Discontent with Nuclear Energy,	V_n :	.33	.29
Personal Influence,	p_i :	.27	.23
Product Terms,	$p_i V_s$:	.49	.45
	$p_i V_n$:	.39	.35
Number of Cases		398	398

contention that there is indeed a p in such participation.¹³

The data of Tables 1 and 2 indicate that public goods are an incentive for performing rebellious political behavior. Now we turn to the question of whether selective soft incentives and expected costs also are determinants of such behavior. In this regard we focused on the following selective incentives: F , intrinsic psychological gratification derived from rebellious collective action because it is "fun"; $E(O)$, the expected reward of performing rebellious behavior because it conforms to behavioral norms of important reference persons; and $E(A)$, expected social affiliation benefits of rebellious behavior (positively valued reactions of the social environment). We also take into account the disincentive of $E(C_r)$, the expected value of costs (negatively valued reactions of the social environment, especially the police). Since the New York City survey did not include measures of selective incentives and costs, we use only data from the Hamburg survey.

The first equation of Table 3 shows the ordinary least squares (OLS) regression of current potential for performing rebellious political behavior against (1) the public goods variables, (2) the indicators of soft selective incentives, and (3) the expected utility of negative sanctions (costs). As anticipated from the bivariate

correlations, the public goods variables, $p_i V_n$ and $p_i V_s$, are estimated to have significant positive effects on R_p . With respect to selective incentives, the entertainment value of rebellious collective action, F , and expected positive sanctions in the form of social affiliation rewards, $E(A)$, are statistically nonsignificant; only the expected value of conforming to behavioral norms of important others, $E(O)$, shows a small (in terms of the standardized coefficient) but statistically significant effect. Surprisingly, the expected utility of negative sanctions, $E(C_r)$, is estimated to have a significant negative instead of positive effect. Since costs generally are negatively valued, this means that respondents who attach a relatively high probability to the occurrence of negative sanctions are somewhat more likely to have a high rebellious behavior potential than those who attach a low probability to the occurrence of negative sanctions.

When the explanatory variables are compared in regard to their relative importance, the dominant predictor of potential for performing rebellious political behavior is the public goods value of rebellion as indexed by generalized hostility toward the political system weighted by perceived personal influence on the provision of public goods. This also is the case in regard to the equations

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Table 3. Rebellious Collective Action Regressed Against Rational Choice Variables: Hamburg Survey

Equations	Intercept	$p_i V_n$	$p_i V_s$	F	$E(O)$	$E(A)$	$E(C_r)$
(3.1) $R_p =$	5.24	0.70*	0.02**	1.02	2.90*	0.25	-1.22**
		(2.26) ^a	(5.00)	(0.80)	(2.15)	(0.57)	(-3.13)
$\hat{R}^2 = .28$		0.12 ^b	0.31	0.04	0.10	0.03	-0.14
(3.2) $R_b =$	-0.45	0.07	0.003**	-0.01	0.29	0.08	-0.11
		(1.75)	(5.00)	(-0.05)	(1.47)	(1.33)	(-1.89)
$\hat{R}^2 = .22$		0.09	0.32	0.00	0.07	0.07	-0.09
(3.2a) $R_b =$	-0.54	0.09*	0.003**				-0.12*
		(2.09)	(6.25)				(-2.05)
$\hat{R}^2 = .21$		0.12	0.35				-0.09

Number of cases = 398

Note: R_p : Aggressive Participation Potential
 R_b : Aggressive Behavior
 V_n : Discontent with Nuclear Energy
 V_s : Political Support/Alienation
 p_i : Personal Influence
 F : Entertainment Value
 $E(O)$: Expected Behavioral Norms of Important Others
 $E(A)$: Expected Social Affiliation Rewards (positive sanctions)
 $E(C_r)$: Expected Costs (negative sanctions)

^at-ratio in parentheses.

^bStandardized regression coefficients in italics.

* $p < .05$

** $p < .01$

for past performance of rebellious political behavior, R_b , where the standardized regression coefficients of $p_i V_s$ are .32 and .35, a magnitude similar to the estimate obtained for future performance in equation (3.1). Indeed, in equation (3.2), $p_i V_s$ is the only statistically significant explanatory variable. The indicators of $p_i V_n$ and $E(C_r)$ are of borderline significance in equation (3.2), however, and when the nonsignificant soft selective incentives variables are trimmed from the prediction equation, (3.2a) shows that $p_i V_n$ and $E(C_r)$ achieve significance at the .05 level, although the standardized coefficient of $E(C_r)$ is of trivial magnitude (less than .1). Thus the multivariate analysis shows that, in contrast to conventional rational choice models of rebellion, the public

goods variables, not selective incentives, appear to be the most important incentives for performing rebellious political behavior. Expected costs do not appear to be a disincentive, and they may even have some weak incentive value. This latter result is inconsistent with rational choice theory in general.

Conclusion

A fundamental problem for a rational choice theory of rebellious collective action is to explain why average citizens might choose to take part in such behavior. According to the private interest theory of collective action in general, formulated by Olson (1971) and applied specifically to the case of politically rebel-

lious behavior by Tullock (1971), the public goods value of rebellion cannot be an incentive to participate because of free riding. Consequently, the private interest theory predicts that the rational average citizen will participate only if coerced to do so or if private material rewards are offered, such as direct payment or proceeds of looting. The coercion and material gain factors, however, can be incentives for average citizens only under special circumstances, such as when a dissident group already is sufficiently powerful to control territory or when rebellious collective action takes the form of land seizures.

The dilemma of the private interest theory in regard to behavior of average citizens is recognized explicitly by Salert (1976, p. 49), who concludes that "the problem with this theory stems from its relative neglect of psychological factors or from the conception of rationality on which it is based, or both." In this paper we pursue the two lines of investigation suggested by Salert as ways of developing a rational choice theory of rebellion: modification of core assumptions and more precise specification of the nature of psychological incentives.

In contrast to the private interest theory, we assume that average citizens may adopt a collectivist conception of rationality because they recognize that what is individually rational is collectively irrational—that if people like themselves were individually rational free riders, the likelihood of the success of rebellious collective action would be very small, and that, therefore, it is collectively rational for all to participate, even though the objective probability of a single individual influencing the outcome is negligible. Thus we reject one of the core assumptions of the private interest theory, namely, that the public goods value of rebellious collective action necessarily drops out of an average citizen's utility calculus.¹⁴

According to our model for a public goods theory, two perceived influence variables are stipulated as relevant: (1) the subjective probability of individual influence on the provision of public goods, and (2) the influence of the group as an undifferentiated whole. In regard specifically to the latter, we postulate that the perceived influence of the group as a whole will be a function of observation of the success or failure of dissident groups in the past: if dissident groups are perceived to have been generally successful, then perceived group influence on the provision of public goods will be high; if dissident groups are perceived to have been generally unsuccessful in the past, then perceived group influence on the provision of public goods will be low.

Postulation of relevant perceived influence variables allows us to introduce the multiplicative interaction of influence and public goods incentive into a model of an average citizen's decision calculus. With respect to operational definition of the public goods incentive, we expect that it will be a function in general terms of a citizen's overall evaluation of the political system—"diffuse support" in the terminology of Easton (1965); "legitimacy" in the terminology of Lipset (1963); "system affect" in the terminology of Almond and Verba (1963)—and a function in specific terms of a citizen's evaluation of particular governmental policies—an important aspect, in the respective terminology of these scholars, of "specific support," "effectiveness," and "incumbent affect." The weakness of previous theorizing about determinants of rebellious collective action on the basis of diffuse/specific support, legitimacy/effectiveness, or system/incumbent affect is that it is individually rational to free ride instead of rebel, regardless of the extent to which diffuse/specific support, legitimacy/effectiveness, or system/incumbent affect are low or negative. Our public goods theory provides a plausible causal rationale for

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linking such traditional political support variables, as indicators of preferences for public goods, to the decisions of average citizens about whether or not to take part in rebellious collective action.

Apart from public goods incentives, we also take into account the expected utility of a variety of nonmaterial selective incentives, as well as expected costs. Olson (1971) is skeptical about the role of nonmaterial rewards as a determinant of collective action in general. Tullock (1971) argues that although a particular kind of nonmaterial selective incentive, the entertainment value of rebellion, is an important motive of "pseudo-rebellious" behavior engaged in by students in advanced democracies, the entertainment motive is not an important incentive in "serious" rebellion. Silver (1974) expands upon the entertainment motive to include a long but vaguely defined list of "psychic income." We measure the entertainment value of rebellious political behavior, as well as two kinds of psychic income: expected benefits of conforming to behavioral expectations of important others, and expected social affiliation rewards.

The principal results of our tests of rational choice models of rebellious collective action are, first, that selective incentives appear for the most part to be irrelevant. This runs counter to the spirit of Olson's general theory, which, as Barry (1978, pp. 23-46) has argued, may have a somewhat narrow range of applicability; and it contradicts directly the arguments of Tullock and Silver in regard to private psychological gratification. Second, we find that public goods incentives—the variables assumed to be irrelevant in the private interest or "by-product" models of Olson, Tullock, and Silver—consistently are estimated to have significant impact on participation in rebellious collective action. These variables are components of a public goods model that is predicated on the assumption that, in regard to an aver-

age citizen's choice of whether or not to rebel, considerations of what is collectively rational can override the individually rational logic of the private interest theory.

A finding that goes against the grain of rational choice theory is the observation of a small but statistically significant negative relationship between the expected utility of negative sanctions and rebellious collective action. Respondents who believe that rebellious behavior is likely to be costly show a somewhat greater tendency to participate than those who believe that it is unlikely to be costly. The solution of this anomaly is an important priority of future research on the formulation and testing of rational choice theories of rebellion. One possibility is that the unexpected sign of the costs variable reflects the presence of a "martyr" syndrome, which would need to be measured independently and controlled for. Alternatively, high expected costs may be closely intertwined with a high public goods value of rebellion, stemming from perception of the existing regime as extremely repressive. If so, this raises the question of how to separate costs from public goods, theoretically and empirically.

Notes

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1. Of 2425 potentially eligible households, 928 refused to give the information necessary to determine eligibility. Of the remaining 1297 known eligible households, the completion rate was 60%. Of all potentially eligible households, the completion rate was 32%. Response Analysis developed a complex weighting procedure that can be used to compensate for the problem of low response rate. We do not use the weighting procedure, however, since our interest

is in testing general hypotheses rather than describing distributions.

2. Respondents were asked first whether they approved of each behavior, and second, to estimate how large a percentage of citizens in the United States would approve of each behavior. These were intended principally as "warm-up" questions. Respondents then were asked to report their intention to perform each behavior in the future ("would do; might do, depending on the circumstances; would not do") and whether they had performed the behavior in the past. Special precaution was taken to ensure anonymity. For the behavioral intention question a deck of preshuffled cards (each listing a particular behavior) was given to the respondent, who was instructed to circle the appropriate response option on each card and place it in an envelope, which then was sealed in his or her presence upon completion of the series. This procedure was repeated for the question about actual participation.

3. The participation potential variables are defined operationally as the product of behavior and intention, where behavior is scored 1 for lack of participation and 2 for participation, and intention is scored 1 for negative intention, 2 for conditionally positive intention, and 3 for unconditionally positive intention. Very little nonresponse was observed (only 3% of the respondents could not be scored on all five variables). When subjected to a factor analysis (principal components) the five participation potential variables load highly on a single factor, indicative of unidimensionality.

4. A constant equal to the number of items was subtracted from the total score in order to set the origin at zero.

5. As in the New York City survey, respondents in the Hamburg survey were presented with a list of legal and illegal behaviors (23 in all), described as actions people might take to protest against the construction of nuclear power plants. For each behavior respondents were asked to report whether they had engaged in it or not. Respondents also were asked about their intention to perform the action in the future, using five response categories ranging from "in no case" to "quite certain." The participation potential variables were constructed by multiplying past behavior (scored 1 for "have not done" and 2 for "have done") by future intention (scored from 1 to 5, where a high score means that the behavior will be performed with a high probability in the future). There were no missing values on the question about past behavior, but because of missing data on the intention question, only 85% of the respondents could be scored on the behavior times intention product variables. This can be explained in part by interviewer errors, as some of the interviewers wrongly assumed that they should ask the intention questions only for those actions previously performed by a respondent. For each intention variable

the missing value was replaced by the mean. A factor analysis of the participation potential variables indicates unidimensionality.

6. Respondents in the New York City survey were asked to state the extent to which they (1) have respect for the political institutions in the United States, (2) think that the courts in the United States guarantee a fair trial, (3) feel that the basic rights of citizens are well protected by the political system, (4) are proud to live under the political system, (5) feel that the system of government is the best possible political system, and (6) feel that they should support the system of government. The response continuum was a seven-point scale. The average inter-item correlation was .57, and scale reliability (Alpha) was .89. Scores were reversed and summed to form a Political Support/Alienation scale (V_2). Respondents in the Hamburg survey were asked to respond on a five-point agree-disagree scale to the following statements: (1) "At present, I feel very critical of our political system"; (2) "In general, one can rely on the federal government to do the right thing"; (3) "The courts in the Federal Republic guarantee everyone a fair trial regardless of whether they are rich or poor, educated or uneducated"; (4) "I find it very alarming that the basic rights of citizens are so little respected in our political system"; (5) "Looking back, the leading politicians in the Federal Republic have always had good intentions"; (6) "I have great respect and affection for the political institutions in the Federal Republic." The average inter-item correlation was .40 and scale reliability was .80. Items were scored in the direction of negative evaluation and summed.

7. The series also included the following: "The existence of atomic power stations is a catastrophe for me"; "I am really afraid of atomic power stations"; "Sometimes I think about atomic power stations, but they do not play an important role in my life"; "It is sometimes difficult for me to fall asleep if I think about the problem of nuclear energy"; and "I am worried about the existence of nuclear power stations." A response was scored 1 if it indicated discontent with the existing utilization of nuclear energy; otherwise a score of 0 was assigned. The mean inter-item correlation was .26 and scale reliability was .71. Responses were summed across items to construct a Discontent with Nuclear Energy scale (V_3).

8. The series also included the following: "The anti-nuclear movement would lose influence if I were no longer active";* "The activity of a single person against the construction of atomic power stations cannot prevent the development of nuclear energy"; "The question of whether the anti-nuclear movement is successful or not because of my participation never occurs to me";* "I don't believe that my activity against atomic power stations has any weight." A five-point agree-disagree response format was used, scored to range from low to high per-

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ceived personal influence. A factor analysis (principal components) yielded two factors with eigenvalues greater than 1. The starred items show low loadings on the first factor. A Personal Influence scale (p_i) was constructed from the factor scores of the first factor. Scale scores were transformed to a range of 0-1 according to the formula: $X_i' = (X_i - X_{min}) / (X_{max} - X_{min})$, where X_i is the original scale value and X_i' is the transformed value.

9. Respondents were asked: "In your opinion, how important is it for people like yourself to be involved in politics today? Is it very important, somewhat important, somewhat unimportant, or very unimportant?" Responses were scored in reverse order, 0, .33, .67, and 1.

10. The six actions were (1) disrupting lectures and meetings, (2) staging illegal demonstrations or strikes, (3) obstructing traffic or preventing free access to schools, factories, or other buildings, (4) damaging or destroying rooms or furniture in universities, (5) taking over public buildings and fighting with the police, and (6) taking up guerrilla tactics such as bombing buildings, holding hostages, and hijacking airplanes. The mean inter-item correlation between responses (on a five-point scale scored from "hurt" to "helped") is .52, and scale reliability is .87. An Efficacy of Group Aggression scale (p_g) was constructed by summing scores across items and transforming them to range between 0 and 1.

11. Scores on the Political Support/Alienation scale are squared because the hypothesis of a positively accelerated function is more plausible theoretically than that of a linear function (see the discussion in Muller, 1979, pp. 83-88).

12. Scores on the Political Support/Alienation scale are squared; scores on the Discontent with Nuclear Energy scale are not squared because this transformation does not improve the correlation with R_p and R_b , perhaps because of the restricted range of the Discontent with Nuclear Energy scale, which is limited to eight values between 0 and 7.

13. Mixed evidence on the question of whether there is a p in conventional political participation (e.g., voting, campaign activity) is reported by Aldrich (1976); however, the indicators of p used by him are indirect.

14. Mason (1984) also rejects this assumption on the grounds that certain kinds of public goods—i.e., those that are inclusive and non-inferior—are not subject to free-riding. An implicit assumption of Mason's argument is that the influence of the individual or the group on the provision of public goods will not be negligible in the case of inclusive and non-inferior public goods.

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REFORMULATING THE CUBE LAW FOR PROPORTIONAL REPRESENTATION ELECTIONS

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The cube law was proposed around 1910 to express the conversion of a party's vote shares into its seat share in two-party plurality elections with single-seat districts. This article develops predictive seat-vote equations for a much wider range of elections, including those involving many parties, single- and multi-seat districts, and diverse seat allocation rules such as plurality and list proportional representation (PR). Without any statistical curve fitting based on the seat and vote shares themselves, the basic features of the conversion are predicted using exogenous parameters: magnitude and number of districts, number of parties, and total size of the electorate and of the assembly. The link between the proposed equations and the original cube law is explicated. Using an existing data base, the fit of the predictive model is examined. On balance, this model accounts well for the conversion of votes to seats, and for the deviation from proportionality in PR systems.

A fundamental issue for democracies is the extent to which elections reflect the popular will. One important aspect of that concern is the degree to which voter preferences for different parties and candidates are reflected in electoral outcomes. The way in which this question is commonly studied is by looking at the relationship between the aggregate vote share for candidates of a given party (or other grouping) and the aggregate seat share received by candidates of that party (Rae, 1971).

Some electoral systems expressly aim at proportional representation (PR), that is, seat shares equal to vote shares. However, the so-called PR systems differ widely among themselves in the degree to which they deviate from ideal PR, largely because different district magnitudes (i.e.,

the number of seats per district) are used, and different numbers of parties have evolved. Discussion of electoral reform in such countries often revolves not around the principle of PR, but around whether and how to make the system somewhat more or less proportional. However, such discussion has not been guided by any general quantitative rule predicting the specific degree of deviation from PR in the systems existing or proposed.

For plurality elections, it is well known that proportionality between vote shares and seat shares cannot be expected. In this case, however, a votes-to-seats conversion rule has been proposed, the so-called cube law. Devising a comparable expression for the PR elections would have considerable practical importance in guiding the discussions of electoral reform. A

more general expression accounting for votes-to-seats conversion in both plurality and PR elections would also be of theoretical significance in unifying the field of electoral systems, and in casting new light on the functioning of each particular system through quantitative comparison with others.

The purpose of this article is to develop and test a set of predictive equations that use no curve fitting based on the seat and vote shares themselves. Starting out as a reformulation and expansion of the cube law, these new seat-vote equations express the basic features of conversion of vote shares into seat shares for a number of electoral rules, including plurality, "list PR" elections, and the Japanese single nontransferable vote.

The Cube Law of Elections

For two-party contests in single-seat ("single-member") districts using plurality, the *cube law of elections*, first formulated around 1910 (see Kendall and Stuart, 1950) was proposed as a predictor of the expected seat-vote relationship. The cube law expresses the empirical observation that cubing the ratio of votes (v_K/v_L) received by any two parties K and L was seen to approximate the ratio of assembly seats (s_K/s_L) they win:

$$s_K/s_L = (v_K/v_L)^3. \quad (1)$$

What this means is that any advantage a party has in terms of votes is magnified at the seats level, and in a very specific way.

The general form taken by the cube law is unusual in political science (though quite usual in some other sciences), in that it consists of an equation connecting well-defined quantities, without any adjustable parameters subject to statistical curve fitting. Given its empirical nature, it might more properly be called the "cube rule," but the traditional designation will be kept in this article. The cube law is meant to apply only to electoral systems of the

Anglo-Saxon type that use one-seat districts and the plurality ("first past the post") allocation rule. In particular, it is not expected to apply to PR systems in multi-seat districts. The latter try to achieve something close to $s_K/s_L = v_K/v_L$, thus replacing the power index 3 in equation (1) with 1.

Even some Anglo-Saxon elections deviate considerably from the cube law (Tufte, 1973). They involve partisan gerrymander, which biases the pattern in favor of one party, and bipartisan gerrymander, which profits incumbents in both major parties. Despite such occurrences the cube law might still be a useful simple base-line rule. It could supply a framework to which various correctives could gradually be added—e.g., some bias or gerrymander coefficients. (For a recent overview of measures of bias see Grofman, 1983.)

The predictive power of the cube law is more seriously threatened by observations that in many cases a power index other than 3 gives a better fit. Even though the cube law was initially formulated for Great Britain, some recent work suggests that a power index of 2.5 might fit British elections better (Laakso, 1979). Further analysis (Schrodt, 1980) underlines the difficulty of establishing the proper statistical format for testing such a nonlinear expression.

Previous Generalizations of the Cube Law

A number of ingenious theoretical explanations for the existence of the cube law have been offered. Most of them (see appendix 1) are so specific that they have not offered avenues for testable predictions beyond the Anglo-Saxon parliamentary elections. However, there is one previous line of approach which has yielded testable predictions for some other elections (such as the U.S. Electoral College), but always within the plurality frame-

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work. This approach will be reviewed here, because it will supply the starting point for further generalization to multi-seat districts, to be presented in the next section.

The properties of the following equation were studied by Theil (1969):

$$s_K = v_K^n / \sum v_i^n, \quad (2)$$

where s_K and v_K are one particular party's vote and seat shares, respectively, n is a constant, and the summation is over the vote shares of all parties. This equation expresses the seat share of one specific party, K , in terms of the vote shares of K and all other parties. When dividing equation (2) by the analogous equation for party L , the summation term cancels out, and one obtains

$$s_K/s_L = (v_K/v_L)^n. \quad (3)$$

This form would include, as special cases, the cube law (for $n=3$) and perfect PR (for $n=1$). Equation (2), in turn, can be derived from equation (3), which means that the two forms are mathematically equivalent. In the case of the cube law, the summational form was used by Qualter (1968) for Canadian elections.

Theil (1969) showed that if any equation connecting s_K/s_L to v_K/v_L exists at all, it has to be of the general form given above. The reason is that the sum of the seat shares of all parties must amount to unity, or 100%. Summing the seat shares s_i for all parties, using equation (2), does yield such a result. This could not be the case for any other functional relation between s_K/s_L and v_K/v_L , when the system has three or more parties, according to Theil's mathematical proof. What this means is that equation (3) is the only possible expression connecting seat and vote ratios that does not produce inconsistencies.

At this stage the index n could have any value, and the question remains why it should be equal to 3 in the case of national assembly elections. It has been proposed

(Taagepera, 1973) that n depends on the logarithms of the total number of votes (V) and the total number of districts (D):

$$n = \log V / \log D, \quad (4)$$

provided that all seats in the same district are allocated to the party with plurality of votes.

The broad underlying reason is as follows. When one considers the ideal extreme cases $D=V$ and $D=1$, it becomes apparent that the numerical value of n should be affected by the relative number of votes and districts. If districts are made so numerous that every voter is a separate district ($D=V$), then perfect PR should prevail by definition, so that $n=1$. If, on the other hand, the number of districts is reduced to one (as in direct presidential elections), then only one party can win a seat, and the resulting seat ratio 0:1 emerges from equation (3) only if n tends to infinity. Assuming quasi continuity in the intervening range, one would expect that n gradually increases from 1 to infinity as D gradually decreases from V to 1.

Equation (4) expresses this general trend and also satisfies the boundary conditions at $D=1$ and $D=V$. The theoretical/mathematical justification for the logarithmic format is reviewed in appendix 2. The striking aspect of equation (4), however, is that it does agree with actual elections, and not only in the cases where the cube law might apply, but also in U.S. Electoral College elections and certain trade union elections data.

For most national assembly elections, the ratio $\log V / \log S$, where S is the number of assembly seats, happens to be close to 3, regardless of electoral rules used (see Table 1 for some examples, disregarding for the moment the last three columns, M , N , and n). In other words, a cube root relationship tends to exist worldwide between assembly size and the voting population, and a rational model for such a relation has been proposed by

Table 1. Characteristics of Electoral Systems Selected for Model Testing

Country, Period and Number of Elections	Votes (V, millions)	Seats ($S=DM$)	$\frac{\log V}{\log S}$	District Magnitude (M)	Effective Parties (N)	n
U.S. House 1950-70 (11)	54.9	436	2.93	1	2.0	2.93
Canada 1963-74 (5)	8.3	264	2.86	1	3.1	2.86
Japan 1963-76 (5)	47.5	486	2.86	4	3.3	1.30
Austria 1945-70 (8)	4.4	165	3.00	6	2.5	1.20
Switzerland 1947-75 (8)	1.0	200	2.61	8	5.4	1.13
Italy 1958-76 (5)	30.2	630	2.67	19	3.9	1.05
Finland 1962-75 (5)	2.5	200	2.78	15	5.8	1.07
Netherlands 1963-77 (5)	6.3	150	3.12	150	5.4	1.01

Sources: Median V, S, and N calculated from data in Nohlen (1978, pp. 384-415), except for the U.S. (Vick, 1979, based on Cox, 1972). Average M is calculated or estimated from information in various parts of Nohlen (1978). Equation (5) is used to calculate n . The number D of districts resulting from $S=DM$ may differ slightly from the actual number when M is not the same for all districts.

Taagepera (1972). In the case of single-seat districts (where $D=S$) this means that the value of n resulting from equation (4) is close to 3. The cube law now appears as the result of combining the "cube root law of assembly sizes" with the equations (3) and (4).

While the cube law is thus explained in terms of more basic rules and thus becomes more credible, it also is subject to a new unexpected modification: instead of being exactly 3 in all cases, the exponent is now expected to vary somewhat from country to country (and also over time within the same country). For New Zealand, equation (4) yields values around $n=3.2$, while for Britain it yields $n=2.6$, with intermediate values for Canada (2.8) and the U.S. (2.9).

Besides casting such new light on the cube law, equations (3) and (4) also lead to predictions regarding the U.S. electoral college, where $D=51$ (and less in earlier times), given that all delegate seats of a state go to the same party. Instead of a power index close to 3, equation (4) now yields an average of about 5, and this is indeed close to the observed average swing ratio, which is clearly higher than predicted by the cube law (Taagepera, 1973). Certain trade union elections

studied by Coleman (1964, p. 347) also fit the pattern, but there the calculated (and observed) n is much less than 3: namely, around 1.5 (Taagepera, 1973). As observed earlier, equation (4) also applies to direct presidential elections, where the entire country becomes a single one-seat district.

In sum, the combination of equations (3) and (4) can express and predict the broad features of Anglo-Saxon assembly elections, of U.S. Electoral College elections, and of direct presidential elections. The fit to any particular election results may be far from the best statistical fit, but the latter lack any predictive ability regarding other countries.

Apart from direct testing or theoretical proof, a model's credibility increases if it leads to unanticipated consequences that can be observed. The application to U.S. Electoral College and direct presidential elections is of that type, since both fall clearly outside the scope of the original cube law. The usefulness of a model is also enhanced if it ties together several previously isolated phenomena. Combining the aforementioned "cube root rule of assembly sizes" (as proposed by Taagepera, 1972) and the seat-vote relationships in equations (3) and (4) to yield the cube

law of elections is such an extension.

Like the cube law, the generalized seat-vote relationship thus developed is meant to apply only to systems using the plurality allocation rule. We will now proceed to previously uncharted ground, and extend the seat-vote equations to multi-seat districts with PR allocation rules. It is sometimes thought that such an extension is unnecessary, because "PR is PR." However, actual PR systems deviate from the ideal PR, and they do so to varying degrees. It will be seen that our extended model is able to account for the basic differences between them.

Seat-Vote Equations for Multi-Seat Districts with Multiple Parties

Further extension of the model of seat-vote relations to include PR elections in multi-seat districts first requires the introduction of district magnitude, M , i.e., the number of seats allocated in the same electoral district using some PR or other non-plurality formula. On grounds that will be discussed shortly, it is proposed that magnitude affects the power exponent n and requires the following modification of equation (4):

$$n = (\log V / \log DM)^{1/M}. \quad (5)$$

In the case of single-seat districts ($M=1$) this equation reduces to the earlier one, as it should. In the case of very large districts (such as Netherlands, where the whole country is a single electoral district of $M=150$) n tends toward unity, reflecting near-perfect PR. For low-magnitude, multi-seat districts (such as Japan, where $M=4$), the small party penalty inherent in the cube law is reduced to a considerable extent, but the system still is a long way from perfect PR.

The way M is introduced is chosen so as to satisfy the boundary conditions for the

extreme cases of $M=1$ (cube law) and M tending toward infinity (perfect PR), but beyond this it is purely empirical. It is justified to the extent that it correctly predicts the broad patterns of the seat-vote relationship for a wide range of district magnitudes. The equation would be expected to apply best to systems with a simple electoral setup: having all districts be the same magnitude and using a simple allocation rule such as d'Hondt or Quota. The complexities actual electoral systems can exhibit are almost limitless (for an overview, see e.g. Grofman, 1975, or Lijphart, 1984a). Thresholds, nationwide adjustment seats, use of second preferences (through ranking on the ballot or through a second round of voting), high variance in district magnitude, and other such features are not taken into account by equation (5). Since such features should be expected to reduce the predictive power of our model, if it is nonetheless able to account well for the basic features of votes-to-seats conversion, our confidence in the model's usefulness would be greatly enhanced.

Used in conjunction with equation (2), equation (5) would enable us to calculate the seat shares s_K for any given value of v_K , except for one remaining problem: the outcome does not depend only on the given party's share of votes, but also on how the remaining votes are distributed among the other parties, through the summation in the denominator of equation (2). In plurality elections, in particular, votes distribution among other parties matters greatly. A vote share of 40% can spell disaster if the party is facing a single opponent, or landslide victory if it is facing two opponents of equal size (as Thatcher did in 1983).

The main issue here is the number of serious opponents one faces. To operationalize "serious" we will use the "effective number of parties," as defined by Laakso and Taagepera (1979), and recently used by Lijphart (1984b) to establish

major conclusions regarding the functioning of democracies:

$$N = 1/\Sigma v_i^2, \quad (6)$$

where the summation is over all parties in the given elections.¹ While N changes from election to election, the changes are usually observed to be minor, so that the average N over a long period can be used (Laakso and Taagepera, 1979; Lijphart, 1984b). Using such an average value, we shall approximate the denominator in equation (2) by assuming that every party K faces $N-1$ other parties of equal size in terms of votes. This gives us, for the denominator of equation (2),

$$\Sigma v_i^n = v_K^n + (N-1)^{1-n}(1-v_K)^n. \quad (7)$$

This seemingly complex expression actually represents a simplification, in the sense that instead of having to know the vote shares of all the parties, one only needs to know N and the vote share v_K of the single real or hypothetical party under consideration. In sum, the seat-vote equations for single-seat plurality and multi-seat PR elections will be tested in the form

$$s_K = v_K^n / [v_K^n + (N-1)^{1-n}(1-v_K)^n], \quad (8)$$

where n is given by

$$n = (\log V / \log DM)^{1/M}. \quad (5)$$

We can now calculate the seat share for any given vote share, provided that N and n are known. The latter in turn depends on the average district magnitude, the number of districts, and the number of votes in the nation. Thus we are predicting the shape of the relationship between seats and votes in terms of one factor (V) independent of the electoral rules, two factors stipulated in the electoral rules (M and D), and a factor (N) which summarizes the long-term votes distribution. If those few factors exogenous to the election results of the given year enable us to account for the general pattern of votes-

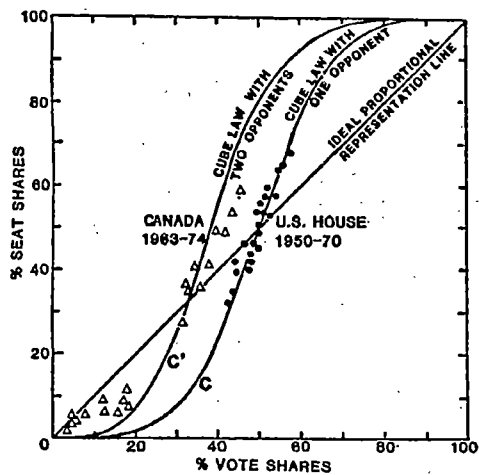
to-seats conversion in a wide range of multi-seat PR and single-seat plurality systems, we would be doing well, indeed.

Graphical Analysis of the Seats-Votes Model

The commonsense way to graph the seat-vote relation for various parties is simply to plot the seat share of party K (s_K) versus its share of votes (v_K), as shown in Figure 1 for Canada and the U.S. However, this way of graphing results in an overcrowding of points at the lower left and a lack of sensitivity regarding even major deviations from PR. Curves C and C' express the theoretical outcomes calculated from equation (8) for the U.S. House ($N = 2.0$) and for Canada ($N = 3.1$), respectively. Given that n is close to 3 in both cases (see Table 1), these curves essentially express the outcome of the traditional cube law for parties facing a single opponent or two opponents of equal size, respectively. Although the cube law pattern is a standard example of a gross deviation from PR, the curves C and C' do not visually seem to deviate that much from the line of perfect PR. Most actual data points for elections world-wide are squeezed in between the curves shown and the ideal PR line. Even stark deviations from PR would be deemphasized both in visual inspection and in statistical testing. Thus the commonsense way to display seats-votes data is not very good.

The crowding and loss of discrimination are avoided if, instead of s_K , one plots the "advantage ratio" $A_K = s_K / v_K$ versus v_K , a format used earlier by Taagepera and Laakso (1980). The outcomes for Canada and the U.S. are shown in Figure 2. The perfect PR and cube law curves now become very distinct, and considerable detail emerges for the small Canadian parties. In the first plot (s vs. v) they all seem to fall close to the PR line. In the second plot (A vs. v) some visibly fall

Figure 1. A Non-Optimal Way to Plot Electoral Data: Seat Shares vs. Vote Shares



very much short of PR, while in one case there is considerable overrepresentation. Note that such “proportionality profiles” have one point for every party in every election.

To demonstrate how widely disparate proportionality profiles can be engendered by equation (8), depending on district magnitude, a sampling of theoretical curves is shown in Figure 3.² These curves show that, in comparison with PR rules, the plurality rule ($M=1$) is expected to give a greater relative advantage to large parties, as has long been observed empirically. They further show that within the PR systems the large party bonus should decrease as M increases, a point previously noted by Sartori (1968) among others.

Graphical Testing of the Seat-Votes Model

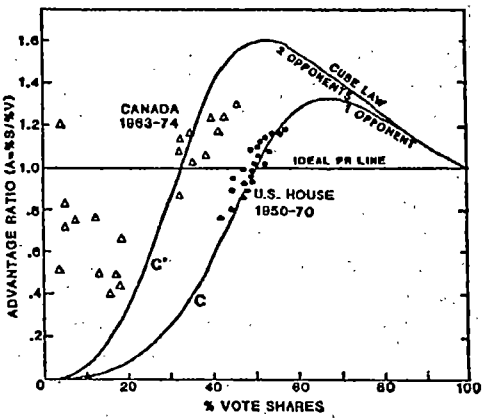
While one might eventually want to test the fit of the proposed equations for all available electoral data, all that is really needed is to look at a data base that ade-

quately covers the entire observed range of values of both district magnitude ($M=1$ to 150) and the number of parties ($N=2$ to 6). To reduce the possibility of picking only the cases which best agree with the model, the selection of countries and periods was based on two previously existing works: Lijphart's *Democracies* (1984b) and Nohlen's *Wahlsysteme der Welt* (1978).

Case Selection

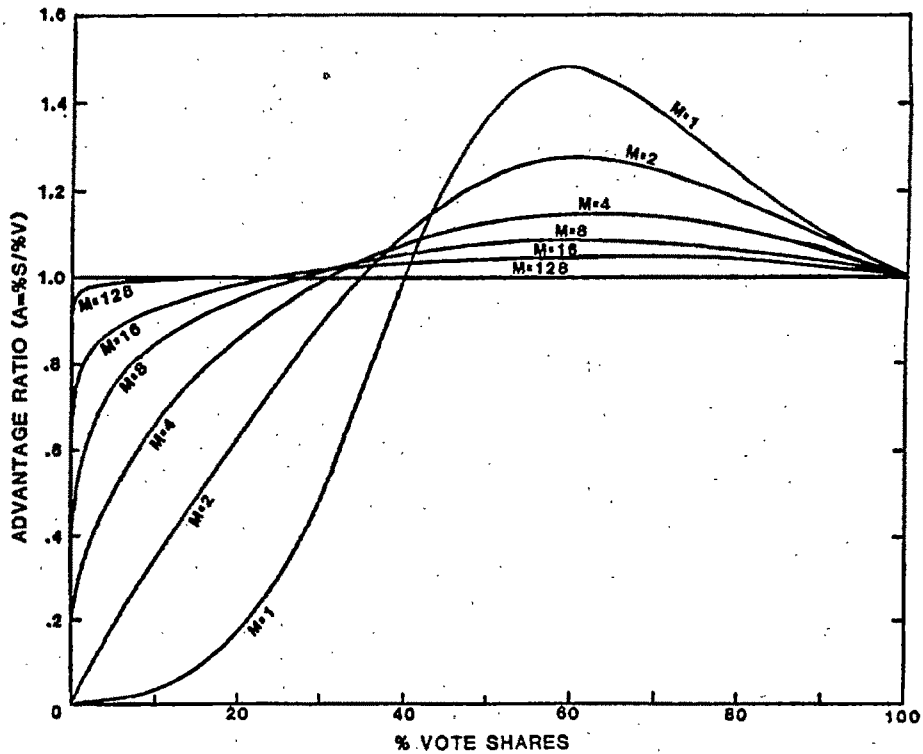
All electoral systems among Lijphart's (1984b) list of 21 stable democracies were considered, discarding those which involve extensive nationwide or regional adjustment seats, rather high thresholds, a second round, use of second preferences, or mixing of several allocation rules. Among the remaining 14 cases, those with the lowest and the highest effective number of parties were chosen for different ranges of district magnitude.³ For the 8 cases thus selected (see Table 1), the election data to be used were chosen on the basis of elections tabulated in the

Figure 2. Proportionality Profiles: Advantage Ratios vs. Vote Shares



Note: See Table 1 for data sources and parameter values of the predicted curves shown in Figures 1 and 2.

Figure 3. Predicted Proportionality Profiles at Various District Magnitudes (M)



Note: These profiles have been calculated using equation (8), with $n=3^{1/2M}$ and $N=1.25(2+\log m)$.

appendix of Nohlen (1978).⁴ Average magnitudes were calculated or estimated from information in Nohlen, and rounded off to integers. The average n and N were calculated using equations (5) and (6). The expected seat shares at various vote shares were calculated using equation (8), and the resulting theoretical proportionality profiles were graphed. Figures 2 and 4 show the actual results for all parties in all the elections considered, plotted superimposed to the theoretical curves.⁵

In assessing the fit of the model to the data from the eight countries, it should be kept in mind that this is *not* a curve fitting with four coefficients, the values of which

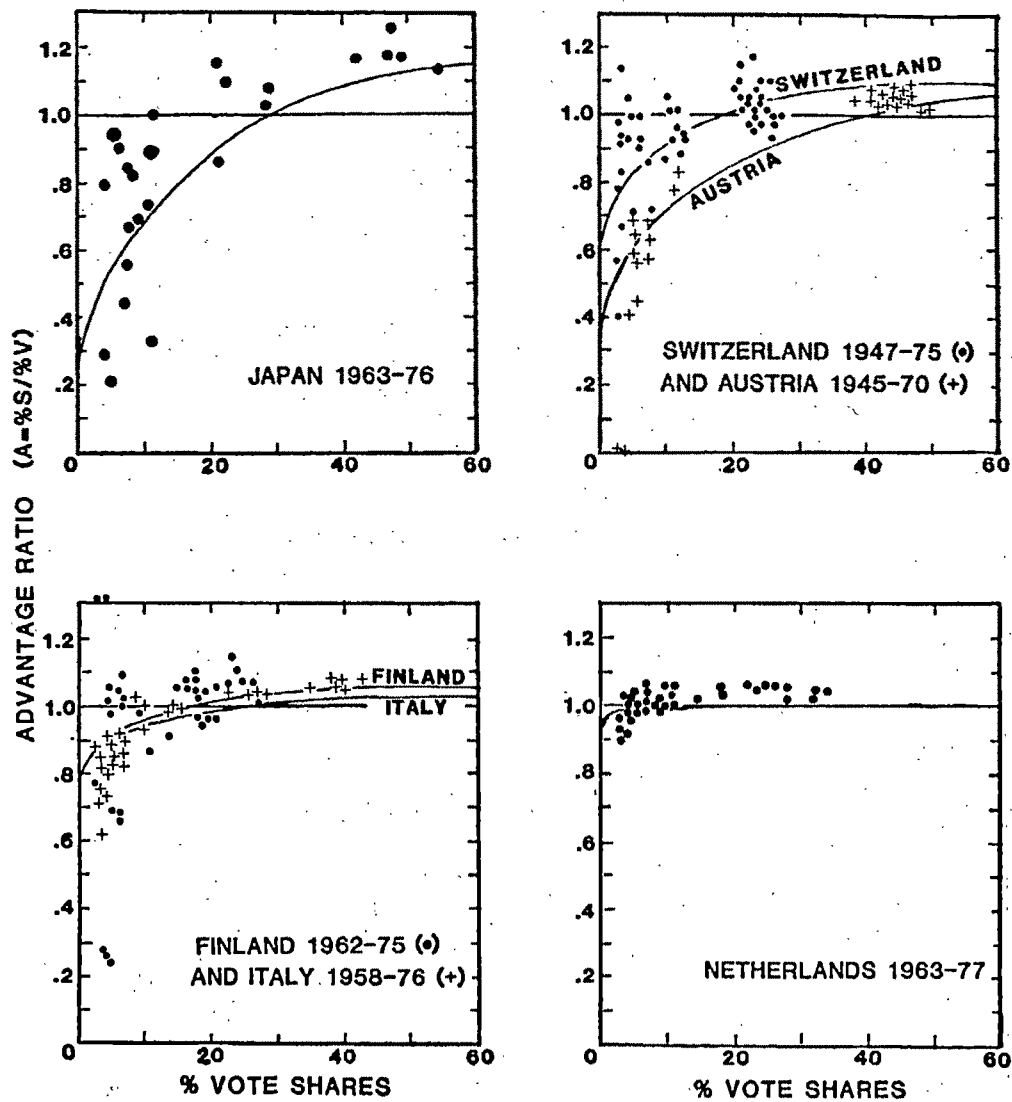
can be adjusted at will to achieve the best fit. The values of M , N , V , and D are all predetermined by the political system. There are no adjustable fudge factors. This also means that there is no need for an "after-the-fact test of the fitted model" with a different data set, because there was no fitted model to start with: all coefficients are exogenous to the data on seats and votes.

Country Results

United States. Among the single-seat systems, the U.S. House offers the lowest number of parties ($N=2.0$). The fit of the U.S. data to the calculated curve in Figure

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Figure 4. Predicted and Actual Proportionality Profiles for Elections with Multi-Seat Districts



Note: See Table 1 for data sources and parameter values used.

2 (which closely agrees with the classical cube law) is visibly close to the best possible fit.

Canada. Among the single-seat systems, Canada has the largest effective number of parties ($N=3.1$). While in the less sensitive s vs. v plot in Figure 1 the Canadian data may look close to the calculated curve, the more sensitive proportionality profile (A vs. v plot) in Figure 2 reveals considerable deviation, especially for the smaller parties. Like the classical cube law, the present refined form correctly predicts very few or no seats for small nationwide parties, such as the British Liberals, but it cannot account for regionally based parties such as the Scottish Nationalists, for whose success the nationwide vote share is not relevant. In such cases A actually tends to decrease with increasing vote shares, up to about 15% of the votes, reflecting the unsuccessful attempts of some regionally based parties to go nationwide. Canada offers one of the most marked cases of such regional variety.

Japan. Figure 4 shows various patterns for multi-seat districts. Japan is the only case available with a low M and a simple allocation rule, and even there the rule is rather unusual: the single non-transferable vote. The calculated curve tends to fall below the actual points, but it correctly predicts the general pattern: (1) The penalty for nonregional small parties is less than in the case of single-seat districts, but is still considerable. (2) The bonus for the large parties is less than in the case of single-seat districts, but it is still higher than in any other multi-seat system (as we will see). (3) The shift from penalty to bonus occurs at lower vote share levels (less than 30%) than is the case for single-seat districts (32–45%). As in the case of nationwide Canadian parties, the general shape of the proportionality of profile is well predicted for Japan, too.

Austria and Switzerland. The next panel in Figure 4 shows the calculated curves and election data for two countries with medium district magnitudes in which the number of parties differs widely: Austria ($N=2.5$) and Switzerland ($N=5.4$).⁶ At a given percentage of votes, the calculated curves predict higher values of A in the case of Switzerland. This is confirmed by electoral results. The imperfect detailed fit should not detract from this main fact: as N changes (at essentially constant M) the direction and the approximate amount of change in A are correctly predicted. Furthermore, the maximum advantage ratio for large parties is predicted to be less than that for Japan, because of larger district magnitude, and this is also borne out. The small-party penalty is correctly predicted to be about equal in Austria and Japan, but less heavy in Switzerland. The shift from small-party penalty to large-party bonus occurs in Switzerland at a lower percentage of votes (around 20%) than in Japan, as predicted by the curves; in Austria's case the lack of parties with vote shares between 20% and 35% leaves this question open.

Finland and Italy. These countries have very large district magnitudes (short of a single countrywide district).⁷ Although these countries differ considerably in the number of their parties, the calculated curves predict that at such a large magnitude N makes hardly any difference. If anything, the Italian data points would be expected to lie slightly lower than the Finnish, and on the average this is the case. Compared with Switzerland, the calculated curves for Finland and Italy predict a slight further reduction in small-party penalty, but this small difference cannot be ascertained with the actual data, given their wide random scatter. The prediction of a slightly lower large-party bonus, compared to that of Austria and Switzerland, is neither confirmed nor disconfirmed, given the scatter in data

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points. The predicted curves are by now so close to the ideal PR line that distinctions cannot be made regarding the break-even point. Once again the predicted curves tend to lie slightly too low for the larger parties.

The Netherlands. For the huge magnitude ($M=150$) of the Netherlands' countrywide district, the calculated curve is practically indistinguishable from the ideal PR line. The actual data confirm this linearity, but the best-fitting horizontal line lies at $A = 1.03$ rather than $A = 1.00$. (This is compensated by heavy penalty on parties with less than 2.5% of the votes, which are not shown in the plot.) The prediction of an even lesser large-party bonus and small-party penalty than in the case of Finland and Italy is borne out, but the difference is small, as expected from the model, and could be swamped by random noise.

Statistical Considerations

The degree of agreement of the predicted curves with the actual data readily meets the coarse but hard-to-fool test called eye-balling. This is true, in particular, regarding predictions about what would happen when district magnitude is increased, or when, at the same M , the number of parties is increased. A more formal statistical analysis will be given next.

For each country, the data were grouped by vote share brackets of 10%—i.e., up to 9.9%, from 10.0% to 19.9%, etc. Within each bracket the median vote share and the median advantage ratio A were determined. The theoretical advantage ratio (designated in this section as A') was also calculated on the basis of the median vote share, using equation (8) and the appropriate country parameters (M , D , V , and N). If the model fitted, one would expect not only a high correlation between the calculated A' and the actual

value of A , but also a best-fit line close to $A = 1.0 A' + 0.0$. This is so, indeed, especially in the case of multi-seat districts. For the 24 number pairs (A and A') generated by the 6 multi-seat systems, the predicted A' ranges from 0.6 to 1.1; the best-fit line is very close to $A = A'$, namely $A = .92A' + .13$, and $r^2 = .87$.

The goodness of this fit is highlighted when one considers the best empirical predictions based on Rae's (1971, p. 89) linear fit of the percentage of seats to the percentage of votes for a large number of PR elections ($s = 1.07v - .84\%$).⁸ This equation yields advantage ratio predictions which are not only rather widely scattered ($r^2 = .54$), but also congregate along a best-fit line with a slope deviating from unity: $A = 1.65A' - .66$. Using the best linear fit of seats to votes in our actual testing sample for a kind of "post-diction" offers no improvement: $A = .55A' - .46$; $r^2 = .44$. For the multi-seat districts, our model performs vastly better than the empirical approach.

When including the 6 number pairs generated by the two single-seat systems in our sample, the goodness of fit is reduced by the aforementioned small, regionally based Canadian parties. The range of predicted values A' widens (from 0.02 to 1.4), the best-fit line for our model becomes $A = .54A' + .48$, and r^2 is .75. Small but nationwide parties in single-seat districts (e.g., in the U.K. or New Zealand) would follow the model, but the U.S. has no third parties with more than 2.5% of votes. This leaves Canada as the only plurality system in our sample which has small parties, putting the model to the severest possible test. Nonetheless, our model still does appreciably better than empirical predictions based on Rae's (1971, p. 70) fit of a large number of elections with single- and multi-seat districts ($s = 1.13v - 2.38\%$). The latter yields a correlation between expected and actual values of A of only $r^2 = .32$. The best fit of the percentage of seats to the percentage

of votes for our present sample ($s = 1.15v - 2.45\%$) is very close to Rae's, and its "postdicted" advantage ratio values have the same limited correlation ($r^2 = .32$) with the actual A .

The elections held after the compilation of the Nohlen (1978) data are too few to offer a sufficient data base for a separate testing of the model. If, however, 13 later elections in the same countries are included with the previous data base of 52 elections, the outcome remains essentially the same, with $r^2 = .70$ for predictions based on our model and applied to all countries, including Canada.⁹

Conclusions

The qualitative predictions of the seats-votes model expressed in equation (8) are confirmed in the cases where large differences in proportionality patterns were expected. The deviations from proportionality predicted by our model agree with the actual ones much better than is the case for predictions based on any other approach (short of country-by-country curve fitting): $r^2 = .87$ for our model vs. $.54$ for the best alternative approach in the case of PR in multi-seat districts, and $r^2 = .75$ for our model vs. $.32$ when including plurality elections. Of course, better empirical fits could easily be found for each country separately, but such post-fact curve fitting would be something quite different from the approach used here, since it would be of no use in predicting outcomes in countries apart from the one for which it was established. The major achievement of the present approach is to enable one to make baseline predictions for almost any electoral system for which the number and magnitude of districts and the number of voters and parties are well definable and known.

The problem of expressing the relationship between seats and votes has of course not been fully solved. There are electoral

rules that defy any systematics, such as the stipulation that the opposition parties to the PRI are to receive 25% of the seats in the Mexican national assembly, regardless of their percentage of votes. Other phenomena, such as vote size thresholds and mixing of several allocation rules, could in principle be taken into account by developing appropriate correction terms to the model presented here. The same applies to the effect of PR allocation rules (such as d'Hondt and Largest Remainder) at the same district magnitude and number of parties, although their effect is small compared to that of M and N .

How does the model presented agree with countries and periods other than the ones tested in this article? Given our method for selecting the sample countries, other systems with simple allocation rules should fit equally well. Visual inspection of previously published West European proportionality profiles (Taagepera and Laakso, 1980) confirms this expectation. The partly theory-based and partly empirical equations presented here clearly offer sufficient potential to warrant further testing with all electoral data available. They have predictive power, and by the same token are clearly "falsifiable"—an indispensable feature of scientific models and laws. For systems with complex electoral rules they offer a way to tell to which simpler system (if any) such a system is equivalent, as far as the votes-to-seats conversion is concerned.

In sum, with four parameters (M , N , V , and D), which are themselves exogenous to the elections data, we can account very well for seats-votes relationships in a very wide range of electoral and party systems. The form of interaction between these parameters is derived from theoretical considerations about their functional relationships. Given the exogenous nature of these parameters, the proposed seat-vote equations offer considerable interest for "electoral engineering," since the model

can make specific predictions about how the votes-to-seats conversion would change upon, say, a small alteration in average district magnitude. But the main interest is intellectual and conceptual. Regarding the multi-seat districts, the small but significant deviations from PR can now for the first time be "understood" in a sense that includes quantitative prediction. For plurality in single-seat districts the known seats-votes relationships have been clarified by making them part of a broader picture. To an appreciable extent, single-seat and multi-seat electoral systems have up to now been considered distinct species to be studied separately. Unifying the field of electoral systems through equations which apply to both species certainly has theoretical significance.

Appendix 1. Previous Explanations of the Cube Law

A normal distribution of voters for a party over all districts would yield the cube law, provided that the appropriate value is used for variance (Kendall and Stuart, 1950). The general shape of the relationship thus receives an explanation, but the particular exponent value, 3, does not. The suitable value of variance emerges from a contagious binomial model based on a Markov chain approach, provided that people form reciprocal influence clusters of a suitable size (Coleman, 1964, pp. 343-53). The establishment of a tie between cluster size and the outcome of an election is of considerable interest in its own right, but it does not explain why people aggregate in clusters of precisely the right size to yield a cube relationship in national elections.

A Poisson distribution can be used instead of the normal one, and it can be justified in terms of a model of interaction between interparty negotiations and intra-constituency pressures (March, 1957); however, the underlying factors have not

been measured. A LOGIT approach (Theil, 1970) also leads to the desired curve shape; the cube relationship emerges if one arbitrarily assumes that the standard deviation of the vote proportions across districts is around $1/8$. A stochastic model by Quandt (1974) chiefly adds the possibility of bias in favor of one of the parties in a two-party system. The large number of parameters enables the model to account, in retrospect, for a large variety of shapes of the seats-votes relationship, but at the cost of making prediction unfeasible. The Markov scheme has been further elaborated upon by Gudgin and Taylor (1979, pp. 31-53) who may have been unaware of Coleman's work. The cluster sizes that would give rise to the cube law are not independent input data, but again are calculated retroactively from the cube law.

A quite different game theory approach to campaign resource strategy is followed by Sankoff and Mellos (1972); it leads to an exponent of 2 rather than 3. Additional assumptions regarding an unswayable hard core of partisan voters can lead to an exponent of 3—or any other value. Once more we have an interesting connection, but a hard-core percentage calculated from the cube law cannot pass for an explanation of it.

Broadly speaking, the aforementioned approaches tend to show the plausibility rather than the inevitability of the cube law. Some of them enable us to use the exponent 3 to infer what the prevalent cluster size or hard-core percentage would have to be, but they do not explain why the whole magic package comes about. The very number of distribution-oriented models offered (which include all those mentioned, except Sankoff and Mellos) indicates some unease regarding the pre-existing models.

What would constitute an adequate explanation of the cube law? First of all, one would ideally want a model plus some independently known quantities

such that one could plug those pre-given numbers into the model from the one end and have equation (1) pop out from the other end. The variance or standard deviation of the distribution of votes over districts can be independently known (though in practice this is often difficult), and thus models using this basis would satisfy this first condition. As a further desideratum, one would prefer a model which applies to all parties, all vote ranges, and to any number or size of districts. All the aforementioned explanations use a system with two parties only. If they can be expanded to include nationwide third parties such as the British Liberals, this aspect has not been formulated. Models which apply to pure two-party systems are not without merit, but multi-party models including the two-party systems as a special case offer a wider explanation. The approach taken by Theil (1969) leads in that direction.

Appendix 2. Derivation of $n = \log V / \log D^{10}$

In the main text of this article, reasoning through extreme border cases led to the conclusion that n is a function of the total number of voters (V) and of districts (D): $n = n(V, D)$. For the sake of achieving as much generality as possible, we would want the model to apply also in the case of multi-stage elections, such as the voters/electoral college/president sequence, although such elections occur rarely. We are here again reasoning by extreme cases which may or may not occur in practice. But if they were ever to occur, a general model of elections should be able to fit them without running into inconsistencies. If V voters elect D sub-representatives who in turn elect T final representatives, then one would expect equation (3) to apply to the stages V - D and D - T separately, and also to the entire process V - T . Consistency then requires that $n(V, T) = n(V, D)n(D, T)$. We can

rewrite it, provided that $n(D, T)$ is not equal to zero, as

$$n(V, D) = n(V, T) / n(D, T).$$

Now take T to be a standard number of units, against which elections with any other number of units are compared. Then $n(V, T) = n(V, \text{constant}) = f(V)$ only, and similarly, $n(D, T) = f(D)$, so that

$$n(V, D) = f(V) / f(D).$$

The function f must be the same function for V and for D . Also, we must have $f(1) = 0$, if we want to account for presidential elections.

In the absence of any information to the contrary, we have to assume that the degree of clustering of like-minded voters is the same on all levels (i.e., a magnified piece of a political adherence map would look like the original map). When V voters elect D representatives (with V/D voters per one-seat district) we should observe the same decrease in f on the entire country level, where the decrease is from $f(V)$ to $f(D)$, and on the level of a single district, where the decrease is from $f(V/D)$ to $f(1)$. Since $f(1) = 0$, this yields

$$f(V/D) = f(V) - f(D),$$

an identity satisfied only if $f(V) = \log V$. This completes the derivation of

$$n(V, D) = f(V) / f(D) = \log V / \log D.$$

Notes

I would like to thank Philip Converse, Creel Froman, Bernard Grofman, Manfred Holler, Arend Lijphart, Howard Margolis, Frederick Reines, and Matthew Shugart for helpful comments.

1. For four parties of equal size (vote distribution of .25/.25/.25/.25), N is exactly 4, as one would want it to be. For a distribution of .45/.35/.19/.01 we obtain $N=2.77$, which tells us that there are only two major parties plus a third one of somewhat lesser importance. The Rae-Taylor fractionalization index F is connected to N through $N=1/(1-F)$.

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2. For all curves shown in Figure 3 it was assumed that the aforementioned cube root relationship (Taagepera, 1972) between the number of assembly seats and the number of voters applies perfectly, so that equation (5) becomes $n = (3)^{1/M}$. Also, N was given the average value at the given district magnitude reported by Taagepera (1984) for world-wide data: $N = 1.25(2 + \log M)$. Despite these simplifications, the variety of curves obtained is considerable. Even more variety can be expected if one uses the actual values of N , D , and V for particular countries.

3. The following ranges were used: for single-seat districts, $M=1$; for low magnitude, multi-seat districts, $2 < M < 4$; for medium magnitude districts, $5 < M < 9$; for high magnitude districts, $10 < M < 20$; and for very high magnitude districts, $M > 100$. There are no cases with M between 20 and 100. For the low M , Japan is the only case with simple allocation rules available. For the very high M , the number of parties no longer is expected to matter, and the case with the highest M available (the Netherlands) was chosen.

4. The only omission was post-1970 Austria, where the electoral system shifted from $M=6$ to $M=19$. The U.S. House data missing in Nohlen (1978) were added, using approximately the same time span.

5. Parties with less than 2.5% of votes were not plotted. For such very small parties, wide random fluctuations in advantage ratio can be expected for any electoral system, because seats come in integer numbers only. If a party with 1.5% of the votes obtains one seat in an assembly of 200 seats, its advantage ratio is a low 0.67. If it should reach two seats, its A immediately jumps to a very high 1.33.

6. In Austria's case the effective M could be somewhat higher than the value $M=6$ used here, because the seats not allocated by district-wide quota of $q = 100\% / (M+1)$ were allocated (by d'Hondt procedure) in four wider regions (Nohlen, 1978, p. 270). This could involve up to 20% of the total 165 seats—i.e., up to 8 seats per region, which thus would represent another district with $M=8$.

7. Among the high magnitude systems, Sweden and Luxembourg have a slightly lower N than Italy, but Italy was chosen as the low- N case because it is at the upper edge of the range of magnitudes. Like Austria, Italy allocates most of the seats by quota in the districts (with the definition of quota varying over time), but the remainder of votes are pooled for nationwide distribution, with some restrictions on parties which do not get any seats in the districts. The remainder has involved from 20 to 80 seats (Nohlen, 1978, p. 275)—i.e., an average of 50 out of a total of about 630. This is roughly equivalent to having one district of $M=50$, in addition to the regular districts where M varies from 1 to 36. The average value $M=19$ used here may slightly underestimate the effective average magnitude. In Finland,

too, individual district magnitudes vary widely, from 1 to 24. The wider scatter of the Finnish data might be caused by the use of local electoral alliances, which have an unpredictable effect on the fortunes of lesser parties. In Italy the scatter may be reduced by the nationwide distribution of remainder seats.

8. Rae's empirical equation based on worldwide PR data predicts, of course, the same proportionality curve for all countries. Among those predicted by our model, this curve is closest to Finland's. No previous model seems to exist that would predict the shifts in proportionality profile curves from country to country.

9. Predictions of $A = \%S / \%V$ have been tested here, rather than predictions of the percentage of seats, because the percentage of seats is so highly correlated with the percentage of votes ($r^2 = .981$ for our sample) that our model's explanatory performance ($r^2 = .985$) does not stand out, although it outperforms all other approaches. Using $A = \%S / \%V$ instead of $\%S$ eliminates the PR component, thus accentuating the deviation from PR and making for a more stringent test.

10. This derivation is condensed from Taagepera (1973).

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A PARTISAN SCHEMA FOR POLITICAL INFORMATION PROCESSING

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Based on their interest in politics and knowledge of political leaders, individuals are classified into three levels of partisan sophistication: (1) those scoring high in interest and knowledge (partisan schematics), (2) a middle group, and (3) those scoring low (partisan aschematics). In this experimental study, and consistent with findings from cognitive and social psychology, partisan schematics prove better able than partisan aschematics to classify campaign statements as either Republican or Democratic and to recall the policy stands taken by a fictitious congressman. Aschematics, at the other extreme, perform at no better than chance levels in either the recognition or recall of the congressman's policy statements. There are, however, liabilities to sophistication as well: Schematics demonstrate a "consistency bias" in recalling significantly more policy statements that are consistent with the congressman's party identification than are inconsistent with it. This "restructuring" of memory is especially pronounced among sophisticates, and reflects a serious bias in the processing of political information.

Few topics in the literature on political behavior have generated as much research and subsequent controversy as the concept of political sophistication. Virtually every descriptive statement about the political behavior of citizens must be qualified by their "level" of information, and all attempts to model behavior introduce one or another measure of political interest, knowledge, or experience as a control or intervening variable. The reasons for this are well documented: one's prior knowledge about some particular domain influ-

ences what one sees and remembers, and how one interprets reality.

Confronted with a blizzard of facts, figures, and images from which political impressions are formed and judgments made, the individual must of necessity impose some perspective on the world to make it comprehensible. An effective cognitive framework allows the citizen selectively to attend to some stimuli and disregard others, to group together otherwise disparate bits and bytes of information, to store in memory a representation of this information, and then to retrieve it

when making decisions. Such memory structures—whether called “pictures in our heads” (Lippmann, 1922), “conceptual maps” (Axelrod, 1973), “frames” (Minsky, 1975), “scripts” (Schank and Abelson, 1977), or “schemas” (Bartlett, 1932; Neisser, 1976)—provide a “judgmental yardstick” (Converse, 1964) for the organization of information in memory.

The cognitive sciences have made significant progress in recent years using the schema concept to develop and test models of human information processing (Rumelhart and Ortony, 1977; Simon, 1980; Taylor and Crocker, 1981). Rather than review the extensive literature on schema functioning (see Alba and Hasher, 1983; Hastie, 1980, 1985; Wyer and Srull, 1984), let us simply summarize those findings that relate most directly to social cognition (Fiske and Taylor, 1984) and, by implication, to political cognition (Lau and Sears, 1985). Schemas have been found to:

1. provide categories for labeling people, places, events, and processes;
2. facilitate the chunking or grouping of information into larger, more meaningful, and more easily retrievable categories;
3. influence what information will be attended to, encoded, and retrieved from memory;
4. facilitate the recognition, recall, and ease of retrieval of schema-relevant information;
5. enable the individual to make inferences from incomplete data by filling in missing information with schema-consistent best guesses;
6. provide a basis for making more confident decisions and predictions; and
7. influence the weighting of evidence brought to bear in making decisions and evaluating probabilities.

Just as schemas vary in scope and complexity, citizens differ in their ability to

use them effectively (Fiske and Kinder, 1981; Graber, 1984; Kinder, 1983; Lau, Coulam, and Sears, 1983; Thorson and McKeever, 1983). The cognitive literature classifies those individuals who have established knowledge structures about a particular domain as *schematics*, while those who have not developed an elaborated knowledge structure—whether for lack of interest, ability, or experience—are said to be *aschematics* (Markus, 1977). The breadth and depth of one's factual and associational knowledge about a particular sphere of interest is the hallmark of schema theory, and what discriminates the concept *schema* from *belief system* (Abelson, 1979).

Theoretically, whatever the domain, differences between how schematics and aschematics process information should be profound. In political cognition, as in social cognition, knowledge matters—it affects how new information is interpreted and used to guide behavior (Conover and Feldman, 1984; Lodge and Hamill, 1983). In politics as in other human endeavors (Markus and Smith, 1981), expertise is acquired by exposure and practice. From this cognitive perspective, the principal indicators of sophistication are an individual's *domain-specific* interest, experience, and knowledge.

The application of schema theory to political information processing is, unfortunately, neither simple nor direct, as the cognitive sciences have focused almost exclusively on knowledge structures central to the individual and pertinent to daily life. Politics, however, encompasses a much wider domain of information and behavior than is commonly studied in psychology, and for many people—perhaps for all but a handful—the political world has a less perceptible impact on their lives than does, say, their occupation. Given these significant dissimilarities, we cannot be confident that schemas will have the same cognitive consequences on political thinking as do the social

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schemas traditionally studied in psychology. This being the case, the basic question to be asked is, Do the same patterns of facilitation effects and liabilities found in the cognitive literature for how "experts" process information in non-political domains hold for the processing of political information by political sophisticates?

We start with the assumption that there is no single, overarching "schema about politics"—that in all likelihood there are many distinct ways to think about government and politics and oneself as a citizen (Campbell et al., 1960; Conover and Feldman, 1981; Hamill, Lodge, and Blake, 1985; Lau, 1985; Miller, 1985; Stimson, 1975). One could, for instance, see and interpret information about government and politics in terms of political parties and partisan conflicts, or, perhaps, organize one's thinking about politics along ideological lines. Other schemas are possible as well. One could—apparently some people do—think about politics as race against race, or, like Marx, see policy disputes as the expression of class conflicts. Each of these frameworks, as well as others, no doubt, is a potentially viable way of organizing information about government and politics, and would produce distinct patterns of evaluations and action.

By all counts, only a minority of citizens—probably no more than 25%—use the concepts *liberal* and *conservative* to organize their political beliefs along ideological lines (Bennett, 1977; Conover and Feldman, 1981; Kinder, 1983; Knight, 1983; Levitan and Miller, 1979; Nie, Verba, and Petrocik, 1976), even when sophisticated methods are used to reduce measurement error and model issue preferences (Erikson, 1979; Marcus and Converse, 1979; Norpoth and Lodge, 1985).

As suggested in *The American Voter* (Campbell et al., 1960), a simpler and perhaps more readily accessible political schema available to the average citizen

would be one based on the perception of partisan differences (Bastedo and Lodge, 1980; Conover and Feldman, 1985; Gant and Luttbeg, 1984; Lau, 1985; Sharp and Lodge, 1985). For citizens with the conceptual wherewithal to structure contemporary politics this way, there would be meaningful distinctions between the policies that Republican and Democratic leaders prototypically espouse. We begin then by discriminating partisan schematics from aschematics. Given that expertise is acquired through domain-specific interest and experience, our operational measurement of partisan sophistication is an index of the individual's interest in, experience with, and knowledge of the Republican and Democratic parties.

Predictably, *partisan schematics* will demonstrate the same kinds of facilitation and liability effects when processing political information as do schematics in other areas of social cognition when dealing with schema-relevant information. To test for this general effect on political thinking, we investigate two of the most critically important aspects of information processing: the recognition and recall of schema-relevant information. The starting point is categorization, thought to be the most basic act of human information processing and the organizing principle of semantic memory (Rosch, 1975). To deal effectively with the barrage of information impinging on individuals in their everyday lives, categorization provides a method for grouping discrete bits of information into a meaningful cognitive structure that in turn facilitates the retrieval of information from memory, which is then used to guide behavior. These memory processes require that the individual be able initially to categorize and organize incoming information. This leads to the first, most basic hypothesis:

Partisan schematics will more accu-

rately categorize party-relevant information than will aschematics.

Next we examine the effects of a partisan schema on memory per se—here specifically, the recall of policy positions taken by a Republican or Democratic leader. While much of the social cognition research on schema functioning demonstrates facilitory effects on information processing, there are liabilities as well: Schemas generate expectations that have been shown to produce a variety of systematic errors in recognition, recall, and decision making (Taylor and Crocker, 1981). Based on such evidence for the processing of nonpolitical information, we hypothesize that

Schematics will remember more information that is consistent with their partisan schema than will aschematics.

We then test for evidence of a systematic bias in recall such that

Schematics will remember more schema-consistent than schema-inconsistent information.

In addition to exploring these basic effects culled from the cognitive literature, we systematically control for variables of particular importance in political behavior research—here specifically, the impact of party identification, educational level, cognitive ability, and self-identification as a liberal, moderate, or conservative on the processing of political information. While each of these factors is likely to influence the categorization and recall of political information, the individual's level of sophistication is expected to be the more powerful mechanism in guiding the processing of political information.

Method

A nonprobability sample of 603 residents of Long Island, New York, was

interviewed in the fall of 1982.¹ As diagrammed in Table 1, this experimental study proceeded in four stages.

Stage 1: Questionnaire

Embedded in a conventional survey questionnaire were 12 questions drawn from the interview schedules of the University of Michigan's Survey Research Center which we used to measure partisan sophistication: 6 questions tapping interest in parties and elections, and 6 knowledge questions asking the subject to classify political leaders—Barry Goldwater, Tip O'Neill, Harry Truman, Howard Baker, Jack Kemp, and Claiborne Pell—as Republican or Democrat.² Correct answers were given a score of 1, incorrect and "don't know" responses a score of 0. Correlated at $r = .55$, the interest and partisan knowledge indices were combined into an additive index of partisan sophistication, which was then trichotomized to create three levels of expertise: the bottom third labelled *aschematics* ($n = 197$), the top third *schematics* ($n = 190$), and a middle group ($n = 197$).

Stage 2: Categorization of the Policy Statements and Experimental Manipulations

The policy statements used in this study were culled from the 1980 Republican and Democratic party platforms; from major campaign speeches by Reagan, Bush, Carter, and Mondale reprinted in *The New York Times*; and from preelection summaries of each candidate's policy positions published in *Time*, *Newsweek*, *U.S. News and World Report*, and the major Long Island newspaper, *Newsday*. In all, 160 policy positions on the economy, energy, the environment, military spending, and social issues were collected and then rated in a pretest by 80 college students on a seven-point category scale labeled *Republican* at one end and *Demo-*

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Table 1. Experimental Design

Tasks and Procedures	Condition				
	Republican Group	Democrat Group	Importance Republican Group	Importance Democrat Group	Control Condition
<i>Stage 1: Questionnaire</i>					
Political Interest					
Political Activities	Yes	Yes	Yes	Yes	Yes
Demographics					
Recognition of Leaders					
<i>Stage 2: Categorization of Policies</i>					
Partisan Mix of Policies	30 Rep; 10 Dem	30 Dem; 10 Rep	30 Rep; 10 Dem	30 Dem; 10 Rep	50 Rep; 50 Dem
Type of Judgment	Rep v. Dem	Rep v. Dem	Imp v. Unimp	Imp v. Unimp	Rep v. Dem
<i>Stage 3: Distractor Task</i>					
Civil Service					
Vocabulary Test	Yes	Yes	Yes	Yes	Yes
<i>Stage 4: Recognition of Policies</i>					
Partisan Mix of Policies	10 "Old" Rep	10 "Old" Rep	10 "Old" Rep	10 "Old" Rep	None
	10 "Old" Dem	10 "Old" Dem	10 "Old" Dem	10 "Old" Dem	None
	10 "New" Rep	10 "New" Rep	10 "New" Rep	10 "New" Rep	None
	10 "New" Dem	10 "New" Dem	10 "New" Dem	10 "New" Dem	None
<i>Type of Judgment:</i>					
"Did Congressman Williams make this policy statement?"	Yes/No	Yes/No	Yes/No	Yes/No	None
Number of Cases	170	167	84	85	81

crat at the other. The 100 statements judged in the pretest as most characteristic of the Republican or Democratic parties were selected for use in the actual field study.

Experimental Session. Subjects were told they were taking part in a larger project whose aim was to design a campaign booklet giving voters information about the policy positions of their representatives in Congress. Approximately one-seventh of the sample was randomly assigned to the control group ($n = 81$),

which characterized as Republican or Democratic the 100 statements used in all the subsequent experimental conditions and phases of the study. This task served as a reliability check to determine the extent to which the 100 policy statements were consensually defined by the sample as characteristic of the Republican or Democratic parties, and allowed us to determine if a schema effect is evident in the simple categorization of policies. Schematics should, predictably, be more accurate than aschematics in the categorization of the policy statements along the

partisan dimension, *if* our measure of partisan sophistication is valid, and *if* the distinction between Republican and Democratic policy positions represents a meaningful category boundary.

As detailed in Table 1, the remaining 506 subjects were randomly assigned to one of four experimental conditions. Each subject read a short "biography" of a fictitious "Congressman Williams" from New York State, who was described as a life-long partisan and party leader; half the subjects were told he was a Republican, and the other half that he was a Democrat. All experimental subjects then read 40 policy statements attributed to the congressman. For the 254 subjects told that Congressman Williams was a Republican, 30 of the policies had been judged in the pretest as being characteristic of the Republican Party, and 10 were judged inconsistent—that is, selected from the set of policies associated with the Democratic Party. For the 252 subjects told that the congressman was a Democrat, the mix of policies was reversed—75% Democratic, 25% Republican.

An example of a policy consistent with the Republican party label is "Congressman Williams calls for major cuts in federal spending on social programs," whereas an inconsistent Republican policy stand is "Congressman Williams favors federal government programs to create more jobs." To assess possible differences in the perceived importance of the policies, which could well affect the recall of information, subjects in the two other experimental conditions rated each policy statement in terms of its national importance. Neither the Republican/Democratic statements nor the consistent/inconsistent items were found to differ in rated importance.

Stage 3: Vocabulary Test

After reading Congressman Williams' biographical sketch and categorizing his

policy positions, subjects in the control group and in all four experimental groups completed a 38-item multiple-choice version of the 1975 Civil Service Commission Vocabulary Test. In addition to its value as a distractor task between the categorization (Stage 2) and the upcoming recall (Stage 4) phases of this experiment, the obtained test score provides a direct measure of each subject's verbal ability and, given a positive correlation between vocabulary and general I.Q. test scores, a surrogate measure of cognitive ability.

Stage 4: The Recognition of Policy Statements

Following the 10-minute vocabulary test, all experimental subjects participated in an unexpected recognition task. Forty policy statements were presented, half of them "old" statements—that is, policy statements actually made by Congressman Williams in the earlier categorization phase of the study. Of these "old" statements, 10 were selected from the set of policies consistent with the Congressman's party label, and 10 were inconsistent. The other 20 statements were all new items—policies not previously presented in the campaign booklet, 10 of which were consistent with the Congressman's party label, and 10 inconsistent. Experimental subjects were to decide for each statement whether Congressman Williams actually stated this policy position in the campaign booklet, indicating their decision on a seven-point scale ranging from 1, "Definitely Yes, he said that" to 7, "Definitely No, he did *not* say that."

The literature on human information processing predicts that schematics are more likely than aschematics to "remember" information consistent with their schema, whether it was actually present in the message or not. Those policies consistent with Congressman Williams's partisan affiliation and actually presented in the booklet ("old consistent") will, predictably, produce higher levels of accu-

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rate recall by schematics than aschematics. On the other hand, those schema-consistent policy statements never presented in the original message ("new consistent") should produce a relatively high rate of false recall in the memory task. Because schematics are attuned, for example, to the policy positions of the prototypical "Democrat," then even when a Democratic congressman does not actually make a campaign promise supporting, say, increased federal spending for education, schematics may erroneously attribute such schema-consistent statements to him. A consistency bias would be evidenced by subjects attributing "new consistent" policies to the congressman and recalling more of his "old consistent" than "old inconsistent" policy positions. According to the cognitive literature, schematics are especially prone to this type of consistency bias.

Relatedly, schematics should, in all likelihood, be able to reject "new inconsistent" policy statements, since such policy statements do not fit with their expectations of partisanship. The evidence concerning the recognition and recall of inconsistent information is, however, mixed, with some researchers concluding that because schema-inconsistent information is "out of character" it is more memorable, while others find that inconsistent information is processed less fully and therefore forgotten more easily (Hastie, 1980). In contrast to the differential effects expected for schematics, aschematics will, predictably, perform at about chance levels for all types of policy statements.

Results

Categorization

The first question raised concerns the ability of sophisticates and unsophisticates to categorize correctly the policy statements as representative of the Repub-

lican or Democratic parties. Recall that the 81 subjects randomly assigned to the control group were asked to rate all 100 policies on a seven-point scale along the Republican-Democrat dimension.³ Eighty-eight of the 100 policy statements were judged by a majority of the control group as being characteristic of either the Republican or Democratic parties. These consensually defined policies are treated here as bona fide partisan policy statements. The 12 statements that did not meet this consensus criterion were eliminated from all further analyses.

First we analyzed the categorical judgments of the control group for evidence of a sophistication effect. Consensual responses—those matching the majority opinion—were coded as correct and assigned a score of 1, whereas incorrect and "not sure" answers were scored 0. For each subject the percentage of items correctly categorized was calculated and submitted to a one-way analysis of variance with level of sophistication as the independent factor. The results, presented in Table 2, show a strong schema effect ($F = 16.01, p < .001$), with scores increasing monotonically from a low of 54% for aschematics to 84% correct for schematics. Scheffe contrasts reliably differentiate each of the three levels of sophistication.

One alternative explanation for the differences in accurate categorization among the three groups is that partisan schematics, blessed with more verbal ability than aschematics, find the categorization of policy statements an easier task. The research design allowed us to examine this possibility using each subject's score on the vocabulary test. A median split on the vocabulary index produces six subject groups: high and low verbal ability crossed with the three schema groups. Table 3 reports the two-way ANOVA that includes both level of verbal ability and level of sophistication as factors. Although there is a main effect for verbal ability ($F = 6.5, p < .01$), the sophistica-

Table 2. Categorization of 88 Policy Items as Republican or Democrat by Schema Level for Control Group Subjects

Schema Level	Mean % Correct	Number of Cases
Aschematics	54.2	30
Middle	67.8	26
Schematics	83.8	25

$F = 16.01; p < .001$

Note: Scheffe contrasts are significant for each of the paired comparisons at the .02 level or better.

tion effect is maintained ($F = 11.1, p < .001$). Within each level of verbal ability, partisan sophisticates score higher than the middle group, who in turn do better than unsophisticates. While verbal ability enhances performance, schematics consistently outperform aschematics, regardless of their vocabulary scores.

Several other analyses were performed to check for the impact of other factors that might mitigate the strength of schema effects. First, performance was compared for Democratic versus Republican policy statements. Although accuracy of categorization was slightly higher for the Republican items (68.4% vs. 66.1%, $p < .05$), the same monotonic pattern of schema effects as described above holds: Schematics outperform subjects in the middle group, who in turn are more accurate than aschematics in categorizing the policy statements as characteristic of

the Republican or Democratic parties. Respondents were also classified according to their party identification, but no differences in categorization accuracy were found for Republicans, Independents, or Democrats. Similarly, whether an individual is a liberal, conservative, or moderate does not affect performance on this basic recognition task.

We now turn to test for schema and type-of-item context effects on the categorization of policies by the two experimental groups that evaluated the policy statements made by Congressman Williams along the Republican-Democrat dimension. Table 4 presents the percentage of consistent and inconsistent policies categorized correctly. Here again, level of sophistication exerts a significant impact on the categorization of both schema-consistent and schema-inconsistent information. As sophistica-

Table 3. Two-way ANOVA for Correct Categorization of 88 Policy Items as Republican or Democrat by Schema Level for Control Group Subjects

Source	Analysis of Variance		
	df	F	Significance
Schema	2	11.1	$p < .001$
Verbal ability	1	6.5	$p < .01$
Interaction	2	.77	n.s. ^a
Error	75		
Total	80		

^aNot significant.

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Table 4. Categorization of Consistent and Inconsistent Policy Statements as Republican or Democrat by Schema Level for Combined Experimental Groups

Schema Level	Mean % Correct		t-ratio	Significance	Number of Cases
	Consistent Items	Inconsistent Items			
Aschematics	43.4	37.6	2.88	$p < .005$	109
Middle	65.7	59.4	3.24	$p < .002$	110
Schematics	75.8	70.7	2.93	$p < .001$	118
Total	62.0	56.3	5.23	$p < .001$	
	$F = 62.9;$ $p < .001$	$F = 45.1;$ $p < .001$			

Note: All comparisons of group means for both consistent and inconsistent items differ at the .001 level or better.

tion increases from low to high, so does accuracy in categorization.

The results in Table 4 for experimental subjects provide strong support for the most basic hypothesis: Partisan sophistication facilitates the categorization of public policies. In the control group as well as in the Republican and Democratic experimental conditions, and for both consistent and inconsistent policy statements, the scores reliably order such that

sophisticates outperform the middle group, which in turn outperforms the aschematics. Note too, for future reference, telltale evidence of a bias in information processing, as our sample of subjects proves better able to categorize the schema-consistent than schema-inconsistent policy positions of the congressman ($t = 5.23, p < .001$).

The two-way ANOVAs reported in Table 5 show the effect of both the schema

Table 5. Two-way ANOVAs for Correct Categorization of Consistent and Inconsistent Policy Statements as Republican and Democrat by Schema Level and Verbal Ability for Combined Experimental Groups

Source	Analysis of Variance		
	df	F	Significance
<i>Consistent Items</i>			
Schema	2	54.81	$p < .001$
Verbal ability	1	11.28	$p < .001$
Interaction	2	.04	n.s. ^a
Error	331		
Total	336		
<i>Inconsistent Items</i>			
Schema	2	38.92	$p < .001$
Verbal ability	1	9.31	$p < .002$
Interaction	2	.59	n.s.
Error	331		
Total	336		

^aNot significant.

and verbal ability factors on the recognition of consistent and inconsistent policy items. For both kinds of policy statements, verbal ability produces a significant enhancing effect, but the impact of sophistication is greater than that of verbal ability. All in all, these results provide evidence that the pattern of schema effects routinely found in the social cognition literature for the categorization of non-political information holds true for the processing of political information as well.

The Recall of Policy Positions

The second and most important concern of this study focuses on the citizen's ability to recall accurately the policy positions voiced by a political leader. Keep in mind that in the final phase of the experiment subjects were presented with 40 policy statements, half of which were policy stands actually taken by Congressman Williams in the booklet and half of which were new policy statements. Of the 20 old policy statements, 10 were consistent and 10 inconsistent with the congressman's partisan label, just as 10 of the new policy statements were consistent and 10 inconsistent. Responses were coded as correct if the subject answered "Yes, Congressman Williams said that" to an old item which was actually presented in the booklet, or answered "No, he did not make that policy statement" to a new item. All correct answers received a score of 1, all incorrect and "don't know" answers a 0.

Here we are interested in two types of effects known to influence decision making: a sophistication effect and a type-of-item context effect. Based on the social cognition literature, we fully expect that, overall, schematics will be more accurate than aschematics in determining whether a policy statement was or was not actually made by the congressman. However, this advantage should only hold for

schema-consistent policies. Predictably, a well-developed schema will facilitate the recall of old policies that are consistent with their partisan schema, as well as help sophisticates reject—that is, say "no" to—new policies that are inconsistent with their schematic expectations. However—and this is the key point—the social cognition literature routinely finds that there are also serious liabilities to sophistication. To the extent that partisan sophisticates are like schematics in other domains, they will find it difficult to discriminate new from old consistent information, since schema-consistent policy statements are compatible with their expectations about what type of public policies Republicans and Democrats characteristically favor or oppose. Aschematics, on the other hand, lacking a well-developed partisan schema, should evidence neither facilitory effects nor strong biases, but poor performance throughout.

Table 6 reports the percentage of policy statements correctly recalled by each level of sophistication. Focusing first on the test for schema effects, we find differential rates of accurate recall by level of partisan sophistication: For all types of policy statements combined, schematics correctly recall significantly more policy statements overall than do aschematics. The schematic group averages 60.3% correct recall, the middle group 53.4%, followed by the aschematics, who do no better than chance (49.7% correct). A type-of-item effect on the recognition of policy information can be examined when the scores are broken down for each of the four distinct types of policies.

A schema effect is evident for three of the four types of policies. For the "old consistent," "new consistent" and "new inconsistent" policy statements, the levels of sophistication fall into the expected order: aschematics exhibit the poorest performance, the middle group performs better, and schematics demonstrate the most accurate recall. For the "old incon-

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Table 6. Mean Percentage of Correct Recall of All Types of Policy Statements by Schema Level

Schema Level	Policy Statement				Number of Cases
	Old Consistent ^a	Old Inconsistent	New Consistent	New Inconsistent	
Aschematics	57.2	50.1	43.3	48.0	167
Middle	62.4	50.3	45.5	55.5	171
Schematics	69.0	49.9	54.1	68.3	165
Total	62.9	50.1	47.6	57.2	503
	$F=9.2;$ $p < .001$	n.s. ^b	$F=5.09;$ $p < .01$	$F=16.6;$ $p < .001$	

^aFor Old Inconsistent items, group means differ at the .02 level or better.

^bNot significant.

sistent" items, none of the schema groups do better than chance—a consequence, we believe, of different factors. The inability of sophisticates to recall "old inconsistent" items is a liability of their partisan schema. Given the avowed partisanship of Congressman Williams, schematics tend to forget 50% of his inconsistent statements, while remembering 69% of his consistent policies. Conversely, the failure of aschematics to recognize "old inconsistent" information is in keeping with their general inability to use partisanship as an organizing principle for processing political information. The low schema group produces recognition scores at chance levels for all types of items. They apparently see the Republican and Democratic parties as Tweedledee and Tweedledum.

Considering just the "old" items in Table 6—those policies the congressman actually took a stand on in the campaign booklet—we again find evidence of a "consistency" bias. Whereas all subjects are likely to recognize correctly more consistent than inconsistent policies, this tendency is especially pronounced for schematics. A similar pattern of bias also emerges from the analysis of new information, in which political sophisticates err significantly less often than do

aschematics when processing inconsistent information about the policy stands of the congressman.

Facilitation effects of sophistication on political information processing are most clearly evident in the high rate of correct recall for "old consistent" and "new inconsistent" policies, while the liability of schematic processing shows up most strongly in the relatively low rates of recall for "old inconsistent" and "new consistent" policies.

Table 7 presents two-way ANOVAs for schema level and verbal ability for the percentage of old and new consistent and inconsistent policy statements recalled correctly. As is evident from these analyses, both schema level and verbal ability influence recall. For three of the four types of policies—"old consistent," "new consistent," and "new inconsistent"—verbal ability enhances recall, but once again the schema effect is maintained.

A summary measure of accurate recall can be computed by comparing the number of "hits" to "false alarms" in the recall of each type of policy. *Hits* refers to identifying correctly as "old," statements actually presented in the campaign booklet, whereas *false alarms* refer to responses of "Yes, he said that" to statements the congressman never made. False

Table 7. Two-way ANOVAs for Correct Recall of All Types of Policy Statements by Schema Level and Verbal Ability

Source	Analysis of Variance		
	df	F	Significance
<i>Old Consistent Items</i>			
Schema	2	6.27	$p < .002$
Verbal ability	1	11.50	$p < .001$
Interaction	2	.12	n.s. ^a
Error	497		
Total	502		
<i>Old Inconsistent Items</i>			
Schema	2	0.043	n.s.
Verbal ability	1	0.185	n.s.
Interaction	2	1.14	n.s.
Error	497		
Total	502		
<i>New Consistent Items</i>			
Schema	2	2.76	$p < .06$
Verbal ability	1	12.08	$p < .001$
Interaction	2	1.08	n.s.
Error	497		
Total	502		
<i>New Inconsistent Items</i>			
Schema	2	11.80	$p < .001$
Verbal ability	1	15.60	$p < .001$
Interaction	2	0.131	n.s.
Error	497		
Total	502		

^aNot significant.

alarm scores are obtained by subtracting the percentage of policy statements correctly recalled from 100. Then, by subtracting the false alarm score from the hit score, a measure of accuracy corrected for guessing is obtained. Scores near zero indicate relatively equal numbers of correct responses and errors—presumably a consequence of random responding—while positive scores reflect the ability to differentiate between information that was presented and information that was not. Table 8 tallies the accuracy scores by schema level, separately for consistent and inconsistent items.

The low schema group produces mean

scores indistinguishable from zero for both the consistent and inconsistent types of policies (.50 and -1.90), thereby confirming predictions of their chance performance, while the middle and high groups exhibit increasingly greater accuracy scores (7.90 and 5.50 for the middle group, and 23.10 and 18.23 for schematics), thus confirming the hypothesis that politically sophisticated individuals have a memory structure which facilitates the storage and retrieval of schema-relevant information.

A number of breakdowns of the recall scores were examined using both schema level and verbal ability as factors to com-

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**Table 8. Hits Minus False Alarms for the 40 Policy Statements
by Item Type and Schema Level**

Schema Level	Consistent	Inconsistent
Aschematics	0.50	-1.90
Middle	7.90	5.50
Schematics	23.10	18.20

pare the Republican versus Democratic conditions, one's ideological persuasion as a liberal, moderate, or conservative, and self-identification as a Republican, Democrat, or Independent. The pattern of results is consistent within each comparison: Schematics—whatever their party identification or ideological leaning—prove better able to recall the policy statements of a political leader more accurately than do aschematics, but, more interestingly, show clear evidence of consistency bias. Aschematics, on the other hand, whether Republicans or Democrats, liberals or conservatives, do poorly across the board.

Conclusion

Viewing the individual as an active information processor focuses attention on how citizens seek out, take in, organize, and act upon information given their prior knowledge structures. The pattern of results obtained in this study parallel those found in the cognitive literature for the processing of non-political information: Information processing is dynamic, at least for sophisticates, who are not passive collectors of information, but, rather, active "hypothesis testers" whose prior knowledge about political parties generates expectations that often-times help but at other times hinder their ability to deal with schema-relevant information.

In this experimental study, we find that partisan schematics outperform aschematics in the recognition and recall of

policy statements. That schematics can accurately categorize policy positions as Republican or Democratic, even when the statement is inconsistent with the spokesman's partisan label, attests to the strength of their sophistication. The discriminatory power of schematics contrasts sharply with that of aschematics, who, if they are willing to hazard a guess, are likely to assume the congressman's policy position probably is consistent with his partisan label. Similarly, Fiske, Kinder, and Larter (1983) found that novices—like our aschematics—processed political information in a very simplistic way, relying on stereotypes, whereas experts—akin to our schematics—were more likely to recognize schema-inconsistent information.

The more interesting substantive findings of this experiment relate schema level to the recall of policy statements. Here again, the pattern of the facilitation and liability effects of sophistication on political information processing confirms predictions derived from the cognitive literature. Aschematics conform to Converse's (1975) description of the poorly informed voter who confronts a new political message "with next to no past information at all, . . . it is just more political blather: in five minutes he will not remember that he heard such a statement, much less be able to reconstruct what was said" (p. 97). In this experiment the time between the presentation of the policy statements and their recall was 10 minutes, by which time the congressman's policy positions were indeed a blur, even

among those aschematics with higher levels of verbal ability.

Although partisan schematics prove to be rather effective processors of political information, they show clear evidence of stereotypy by remembering significantly more of the schematically consistent than inconsistent policy statements made by the congressman. Partisan schematics systematically distort the congressman's stance on the issues by imposing more schematic order on his policy positions than was actually present in the campaign message. This "restructuring" of memory, as distinct from veridical memory of the actual mix of consistent and inconsistent information in the campaign booklet, reflects a serious bias in the processing of political information. The "simple act" of labeling a congressman as a Republican or Democrat systematically affects what information about the candidate will be stored in memory and what information will later be available for informing one's evaluations.

Notes

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1. Relative to national averages our sample is overly representative of males (51%), whites (97%), the educated (67% above high school) and those with high income (70% reporting \$20,000 a year), but more fairly approximates Long Islanders.

2. Several questions determining the level of participation in conventional electoral activities were included in the questionnaire. However, because subjects in this sample, like the national population, do little beyond voting, measures of participation did not correlate highly with either interest or knowledge. Therefore, activity scores were not included in our sophistication index.

3. A list of the 100 policy statements used in this study with the percentage of "Republican," "Democrat," and "Don't Know" responses is available from the Laboratory for Behavioral Research, SUNY at Stony Brook, Stony Brook, NY 11794.

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SCHEMATIC ASSESSMENTS OF PRESIDENTIAL CANDIDATES

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This article applies theories of social cognition in an investigation of the dimensions of the assessments of candidates employed by voters in the United States. An empirical description of the public's cognitive representations of presidential candidates, derived from responses to open-ended questions in the American National Election Studies from 1952 to 1984, reveals that perceptions of candidates are generally focused on "personality" characteristics rather than on issue concerns or partisan group connections. Contrary to the implications of past research, higher education is found to be correlated with a greater likelihood of using personality categories rather than with making issue statements. While previous models have interpreted voting on the basis of candidate personality as indicative of superficial and idiosyncratic assessments, the data examined here indicate that they predominately reflect performance-relevant criteria such as competence, integrity, and reliability. In addition, both panel and aggregate time series data suggest that the categories that voters have used in the past influence how they will perceive future candidates, implying the application of schematic judgments. The reinterpretation presented here argues that these judgments reflect a rich cognitive representation of the candidates from which instrumental inferences are made.

Candidate evaluations are one of the most important but least understood facets of American voting behavior. In a classic article, Stokes (1966) argued that "personality" characteristics, rather than issues or parties, provide the best explanation for shifts in the vote from one presidential election to the next. Moreover, subsequent research has shown that candidates have been more salient to voters than

political parties since the late fifties (Kagay and Caldeira, 1975; Miller, Miller, and Schneider, 1980), and that since 1956 candidate affect has been more directly related to the vote than party attachment (Kelley and Mirer, 1974; Markus and Converse, 1979; Miller and Miller, 1976).

Yet despite the unquestioned importance of candidate evaluations, systematic theoretical and empirical research has

lagged far behind that devoted to parties and issues. This may in part reflect a predominate concern among social scientists for examining rational choice theories of candidate selection. Voting on the basis of personality characteristics is often viewed in the literature as "irrational" (cf. Converse, 1964; Page, 1978). The popular cynical view of candidates is that they are attractively packaged commodities devised by image makers who manipulate the public's perceptions by emphasizing traits with special appeal to the voting audience. Voters' judgments about alternative candidates are in this view based on superficial criteria such as the candidate's style or looks.

Prior models of voting behavior have generally treated candidates as idiosyncratic, short-term forces. The model presented in *The American Voter* (Campbell, Converse, Miller, and Stokes, 1960), for example, portrayed candidate assessments as dominated by the more enduring forces of parties and issues. The attitude consistency theory underlying this model suggested that party identification acts as a perceptual screen, and that the human need for consistency results in attitudes toward less central political objects, such as candidates, being brought into harmony with party identification. Alternatively, rational choice models have argued that citizens deal with the information available for judging candidates in a piecemeal fashion, calculating overall utilities that are then employed to select the candidate optimally satisfying their self-interest (Downs, 1957; Fiorina, 1981).

Doubts regarding the validity of these models of how individuals think about candidates have become increasingly frequent in recent years, however. With a decrease in the predictive power and stability of party identification, the model of *The American Voter* has been roundly criticized. In particular, many of the fundamental psychological underpinnings of the model have been questioned. The

assumption, for example, that people have a need for consistency has been challenged (Kiesler, Collins, and Miller, 1969; Lau and Sears, 1985). Similarly, the notion of a perceptual screen is now seen as a rather static and limited view of cognitive processes. The metaphor of a screen focuses our attention on the acquisition of information, while ignoring its storage and retrieval from memory. In addition, while partisanship has always been a potent determinant of evaluative direction, it has never predicted the content (other than party) of people's thoughts about the candidates. Although later research (Rabinowitz, 1978; Rusk and Weisberg, 1972; Weisberg and Rusk, 1970) showed that substantive issue dimensions could predict the relative affective ratings of political leaders, it nonetheless left unanswered the question of what determines our actual cognitions about candidates.

Rational choice models also fail to describe or predict the content of the cognitions that citizens have about candidates. Furthermore, such models have been criticized as requiring an overly complex mental procedure for representing how people process candidate information (Fiske, 1985; Herstein, 1981). In short, rational choice models offer only a partial—and debatable—depiction of how citizens judge candidates.

Recently, a very different approach to candidate assessments has begun to appear in the literature. This emerging theory holds that candidate evaluations are not necessarily superficial, irrational, or purely short-term. Voters may focus on the personal qualities of a candidate to gain important information about characteristics relevant to assessing how the individual will perform in office (Kinder and Abelson, 1981; Popkin, Gorman, Phillips, and Smith, 1976; Shabad and Anderson, 1979). A similar perspective rooted in social psychological theory contends that criteria used in judging candi-

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dates reflects relatively general and enduring tendencies (Conover, 1981; Kinder, Peters, Abelson, and Fiske, 1980).

This new approach is largely based on the premise that individuals organize their thoughts about other people into broad preexisting categories. These category "prototypes" are then used in making judgments when only limited factual information is available (Cantor and Mischel, 1979). Kinder et al. (1980), for example, explore the features that may define an ideal president, to determine if people use this prototype to evaluate presidential candidates. They find that people can choose attributes they believe would make for an ideal president, but that these prototypic conceptions are related only to ratings of the incumbent president. In contrast, Foti, Fraser, and Lord (1982) demonstrate that preexisting cognitions about political roles are related to impressions of political leaders. Yet they only demonstrate the use of category labels such as "effective political leader" on two national figures, one of whom was president at the time of their study.

These results seem to support the traditional interpretation of candidate assessments as short-term indicators of electoral change. If the presidential prototype is primarily a reflection of characteristics associated with the incumbent, then the attributes describing an ideal president must be subject to considerable variation over time. However, given the timing of the Kinder et al. study (spring 1979), it is reasonable to assume that the situational context may not have provided sufficient cues to evoke prototypical cognitions about any potential candidate other than Carter, who was then the incumbent. In short, the evidence testing the question of whether voters judge actual candidates against some prototype remains inconclusive.

It is our contention that a presidential prototype, or *schema*, as we shall label it, can and will be evoked during

the actual campaign period when people receive the appropriate stimuli to trigger these preexisting cognitions. We will attempt to test this hypothesis with open-ended survey responses evaluating presidential candidates that have been gathered since 1952 by the National Election Studies (NES) conducted by the University of Michigan's Center for Political Studies. Our analysis of people's responses to the open-ended candidate questions reveals a great deal of information about what the electorate values in political leaders, and more generally, how voters organize their cognitions about them. Before proceeding to the analysis of these data, however, we need to summarize the processes involved in schematic judgments and the hypotheses they suggest.

Theoretical Predictions of Presidential Criteria

Current theories of social cognition posit that people deal with the flood of information in their environment by employing cognitive shortcuts (Nisbett and Ross, 1980). The basic assumption is that humans are active information processors and cognitive misers (Taylor and Crocker, 1981). This theory contends that people categorize objects and simplify information as a result of general limits to human cognitive capacity. The complexity of the real world becomes simplified in our minds through a dynamic process that combines experience with real-world occurrences (and co-occurrences of objects and events) and the results of active mental processes, including the inferences we make to fill in missing information or go beyond the information directly available to us (Fiske and Taylor, 1984). These mental representations of the world, frequently called *schemas*, function to direct our attention and aid in the storage and retrieval of information in a manner that influences

both our memory of previous experience and the acquisition of new information (Fiske and Linville, 1980; Taylor and Crocker, 1981).

What this theory suggests for our purposes is that voters do not evaluate each political contender *de novo*, or simply with respect to readily apparent attributes, but rather in terms of their own embellished perceptions. As Lippman (1922) noted, "We do not so much see this man and that sunset; rather we notice the thing is man or sunset and then see chiefly what our mind is already full of on those subjects."

Person schemas are knowledge structures about people, and schemata about political candidates are organized cognitions about them in their political role. Candidate schemas thus reduce the complexity of our impressions by enabling us to categorize and label an individual politician according to certain abstract or representative features. These categories then serve as a set of cues from which we can draw further inferences about the candidate's future behavior (Schneider, Hastorf, and Ellsworth, 1979). For example, labeling Walter Mondale as a "big spender" may cue other unstated or inferred properties among conservatives, such as being a weak leader, being overly sympathetic toward those on welfare, and, of course, being a liberal.

This process of schematic thinking about candidates is similar to overt stereotyping, in which one assigns unobservable dispositional qualities (e.g., attitudes or intentions) to another. It is usually initiated by some social or physical feature of the perceived individual, a distinctive biographical fact or observable behavior pattern, or some commonly recognized label such as "Democrat" or "liberal." Once invoked, it acts to embellish what we know about the person and provides an implicit cognitive theory regarding what we expect of the person in the future (Cantor and Mischel,

1979). Schematic images are abstracted from prior experience. Political candidates, especially those running for the presidency, would therefore appear to be prime targets for schematic thinking. Besides all the daily information about the incumbent president, people generally have observed enough presidential candidates that it seems reasonable to hypothesize that they will employ categorical criteria when evaluating them.

Another hypothesis suggested by this theoretical framework is that a few broad criteria, rather than specific information, will be used to judge candidates. Furthermore, these categories should be similar over time, despite the uniqueness of each candidate. We would also predict, for reasons spelled out below, that comments of a personal nature, rather than issues or partisan ties, should predominate in these evaluations. Contrary to the common assumption by those who view personality as irrational, schema theories suggest that more politically informed voters will be the most likely to make comments about the candidates' inner dispositions and behaviors. These types of assessment reflect inferences that go beyond the available information, and cognitive theories predict that people with "richer" schemas will make considerably more inferences, based on their larger store of experience with the object. On the other hand, those with less well-formulated schemas can be expected to limit their inferences to observable features and to make fewer and less complex inferences. In sum, the frequency with which particular attributes will be used in appraising presidential candidates should reflect the availability of informational cues, the personal relevance of these cues, and one's knowledge about politics.

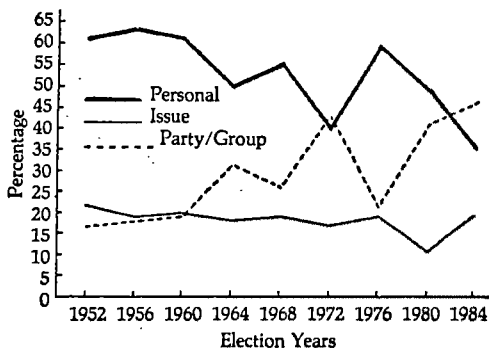
Party, Issues, or Personality?

To examine the general framework people use in organizing their thinking about

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politics, it is important to ascertain how thoughts are structured. Because open-ended questions allow respondents to establish their own frame of reference, they are particularly well suited for this purpose. By examining the distributions and interrelationships among thousands of open-ended comments about the candidates obtained in the 1952-1984 NES studies, we can gain substantial clues about the cognitive representations people have of political leaders. The frequency of mentions will denote the relative importance of particular characteristics for the public; the way these comments are organized into larger domains (as determined via factor analysis) will reveal the basic structures underlying the multitude of responses. Fortunately, the relevant open-ended questions about candidates have been asked in exactly the same way since 1952, thus eliminating any methodological concerns about changes in the wording of questions.

Figure 1. The Distribution of Personal, Issue, and Partisan Comments About Presidential Candidates, 1952-1984



Source: SRC/CPS National Election Studies.

Note: Data are the percentage of all comments in all three dimensions. Comments associating candidates with particular groups were combined with those mentioning party connections. As party differences were frequently discussed in terms of the groups they usually benefit, the two sets of comments are theoretically similar.

Schema theories offer a framework for interpreting these open-ended statements by suggesting the primary criteria for evaluating candidates and by illuminating the cognitive processes most likely involved. As gaining attention is the initial step in the categorization process, salient factors in the environment provide the primary stimulus by which schemas are invoked (Taylor and Fiske, 1978). Candidates should be very salient features of this environment, perhaps even more so than issues or parties, because they are less abstract; thus, it is easier to store and retrieve information about them from memory. In addition, media coverage of campaigns augments this focus by placing greater emphasis on the personal characteristics and background of the candidates (see Figure 1). Except for 1972 and MacKuen, 1979).

Previous research does in fact suggest that over half of the comments made about candidates are on the personal level (see Sears, 1969, for a summary).¹ Since 1964, however, the proportion of issue-relevant comments in response to candidate like/dislike questions has increased (see Figure 1). Except for 1972 and 1984, the use of "person-related" characteristics has dominated cognitions about the candidates. Sears (1969) refers to this emphasis on personal characteristics as "the personalization of politics." We concur, and would further argue that it reflects the result of *performance* or *behavior* inferences made about the inner dispositions of the candidates.

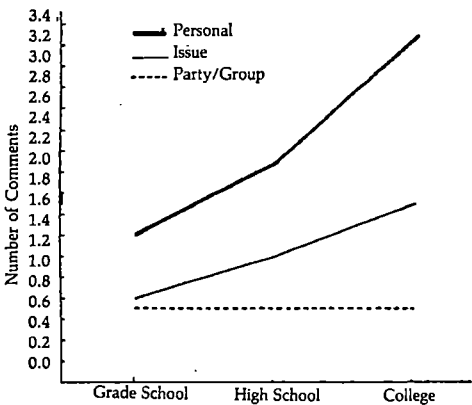
If, on the other hand, these comments are nonsubstantive and idiosyncratic, then we would expect a good deal of instability on the individual level—that is, that people would not offer these responses consistently from year to year. Such is not the case, however. Examining the two long-term NES panel studies, one obtains the following continuity correlations for the number of comments made by respondents in each domain:

	1956-1960	1972-1976
personal	.41	.31
party/group	.07	.18
issues	.17	.23

Despite the greater similarity in the correlations for the three different components in the 1972-1976 panel, it is nevertheless still clear that the frequency of comments about candidates in terms of personal qualities is more stable across elections than is the case with either party or issue comments.² This greater stability at least hints at the possibility that some more enduring or schematic thinking underlies personal assessments.

A more traditional explanation for the prevalence and stability of so-called personality comments stems from Converse's (1964) cognitive capacity model. Converse suggested that better-educated people are more likely to focus on policy concerns rather than on nonsubstantive topics such as the candidates' personal features. This prediction is based on the assumption that well-educated people seek out more political information and that their formal training enables them to absorb better the complexities of issue politics. Figure 2, however, presents data

Figure 2. Average Number of Personal, Issue, and Partisan Comments by Education



Source: SRC/CPS National Election Studies.
Note: The Figure entries are averages based on all nine presidential surveys for the period from 1952 to 1984. There was little variation in the year-to-year figures, thus justifying collapsing the years together. The yearly figures can be obtained from the authors upon request.

directly at odds with the cognitive capacity model. In every presidential election survey since 1952, better-educated

Table 1. Multivariate Analysis of the Use of Personal, Issue, and Partisan Comments in Candidate Evaluations

Predictors	Personal	Issue	Party/Group
Education	.18*	.08	.04
Media exposure	.11*	.08	.06
Political interest	.13*	.13*	.06
Strength of party identification	.04	.03	.14*
Articulateness	.28*	.23*	.21*
Multiple R	.50	.35	.29

Source: National Election Studies, University of Michigan Center for Political Studies.
Note: Table entries are standardized regression coefficients and multiple R. The dependent variable is the number of comments made in each of the three substantive areas, respectively. The figures presented are averages of the coefficients for the nine data sets from 1952 to 1984. The significance figures are also averaged across the years, and do not necessarily indicate the significance levels that would be obtained with a data set that combined all the years. A table with the coefficients for each individual year can be obtained from the authors.

*p < .01.

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respondents volunteered more personal comments about the candidates than did the less well educated, a finding that remains true even after controlling for articulateness.³ Nor does the difference in the number of issue comments made across education levels fit with the prediction of the cognitive capacity model. Once articulateness was controlled for, level of education was not significantly correlated with the number of issue comments a respondent made in any of the years examined.

An alternative explanation is suggested by theories of schematic information processing. People generally use situational information as a basis for making inferences about another person's dispositions (Schneider, Hastorf, and Ellsworth, 1980). This implies that voters who regularly follow politics will have more available information about the candidates and be more likely to make dispositional inferences concerning them. Indeed, the regressions displayed in Table 1 demonstrate that political interest and media usage are significantly related to the number of personal attribute comments offered about the candidates. Yet even after controlling for these other factors, education remains a strong predictor of statements regarding the candidate as a person. Comments associating the candidates with issues, on the other hand, are predominately related to interest in politics (see Table 1).

In general, the regressions in Table 1 support the hypothesis that when asked to evaluate the candidates, people emphasize personally relevant factors. What is salient to the perceiver can also be a property of the perceiver, or as Markus (1977) argues, individual cognitive schemas about the self can direct our attention to particular aspects of the environment. For example, people who think of themselves as interested in politics seek out more political information and tend to make more issue comments, regardless of their

educational background. Similarly, we find that strong partisans focus their comments on party connections, presumably because partisanship and the particular groups associated with each party are more personally meaningful to these individuals.

Surely the most important finding in Table 1 is the counter-intuitive notion that elections are more likely to be "personality contests" for college-educated voters. In order to develop a complete and meaningful interpretation of this important finding, it is clearly necessary to examine the content of the personal comments.

Cognitive Categories Underlying Personality Comments

Numerous statements have been made by survey respondents regarding the various candidates who have run for the presidency during the past quarter-century covered by the NES. The most crucial use of these data for our purposes is to determine if the comments cluster together into relatively few general categories, as schema theory would predict, and to ascertain from the content of the comments what judgmental criteria are employed. Previous researchers (Campbell, Gurin, and Miller, 1954; Page, 1978; Shabad and Andersen, 1979) have organized people's responses in a number of different fashions according to their own particular interests. The variation in the resulting categorization schemes suggests the need for a systematic, empirically based approach to these open-ended responses. Rather than imposing ad hoc typologies on people's comments, we have chosen to investigate empirically how the respondents themselves structure their responses.

The approach employed was to apply factor analysis to the set of comments made by the respondents.⁴ In each of the years examined, the factor analysis

showed a five-dimensional solution as the best fit to the data. Although the components are not perfectly distinct for every year, they are clearly separate—the average correlation among the factors was only .10. These results confirm the prediction from schema theory that people think about presidential candidates in terms of a limited number of broad categories rather than in terms of a multitude of discrete traits. While the categories are similar in some respects to those used by previous researchers, they have the advantage of being identified through an analysis of the data themselves rather than by preconceived notions of the investigator.

The first generic category or dimension people use involves the candidate's past political experience, ability as a statesman, comprehension of political issues, and intelligence; we call this *competence*. Another dimension, *integrity*, deals with notions of trustworthiness and includes remarks such as "honest," "sincere," and "just another politician," and references to corruption in government. A third dimension, *reliability*, is similar to integrity but contains important distinctions. Reliability refers to a candidate as dependable, strong, hardworking, decisive, aggressive, or the opposite of any of these. That the two dimensions are separate is most evident in the evaluations of Goldwater, who received the highest integrity rating of any candidate except Eisenhower and the lowest reliability rating of any candidate in the entire series. Reliability thus serves as a bridge between integrity and competence. Perhaps the best description of it would be trust in terms of capability rather than honesty.

A fourth factor, which we label *charisma*, encompasses a candidate's leadership, dignity, humbleness, patriotism, and ability to get along with, communicate with, and inspire people. Finally, a fifth major category can be seen as com-

prising the most purely personal comments. Included here are references to a number of observable features about the candidates' appearance (e.g., age, health, smile, speech pattern) or background factors (e.g., military experience, religion, wealth, previous occupation). We refer to this as the *personal* dimension, but it should not be confused with the label *personal attributes* that has traditionally been used to designate all of the comments encompassed by the five different dimensions. (Please see the Appendix for a complete list of which codes fell onto which factors.)

In summary, the factor analysis demonstrates that people do in fact think about candidates in terms of a limited set of broad categories. These categories appear to incorporate both observable descriptive characteristics and inferred dispositions that citizens group in a coherent and consistent fashion when judging candidates. That most of the specific comments are incorporated by the five dimensions indicates rather widespread consensual cognitions of political candidates. Likewise, the similarity in the structure of the comments revealed by the factor analyses for the various years suggests that people share common expectations across time about what presidential candidates should be like. In general, the data imply schematic rather than piecemeal processing of information about presidential candidates.

The Prevalence of Specific Characteristics Over Time

Further evidence relevant to the question of whether people use relatively stable judgmental categories to evaluate candidates comes from an examination of the frequency with which the various categories have been employed over time. Despite the uniqueness of each specific candidate, we can expect that the inherent nature of schematic structures will limit

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Table 2. The Relative Prevalence of Generic Categories of Candidate Characteristics, 1952–1984

Dimension	1952	1956	1960	1964	1968	1972	1976	1980	1984
Competence	36	41	46	39	42	42	39	44	39
Integrity	14	15	9	18	16	25	27	16	16
Reliability	5	5	8	24	19	22	13	16	17
Charisma	10	9	6	8	9	5	5	5	10
Personal	35	30	31	11	14	6	16	19	18

Source: National Election Studies, University of Michigan Center for Political Studies.

Note: Table entries are percentages of all comments from the five dimensions.

their accommodation to new information. The novelty of each candidate and the historical situation may influence the prevalence of different criteria, but continuity in the general dimensions used in judging presidential contenders across time would further confirm that people process information about candidates schematically.

Over the years, respondents have consistently made more comments reflecting the internal dispositions and expected behaviors represented by the dimensions of competence, integrity, and reliability (see Table 2). Remarks concerning candidate competence have consistently been the most prevalent, thereby implying that voters are quite sensitive to cues concerning the candidate's past or potential performance in office. Such was particularly the case in 1960, when many people mentioned Nixon's experience as vice president. The fact that Kennedy deliberately tried to counter Nixon's advantage on this criteria by emphasizing his own knowledge and expertise no doubt also increased the frequency of these responses. Similarly, both candidates in 1980 emphasized their previous experience—Carter as president and Reagan as governor of California. The survey comments reflect these emphases, as well as a widespread questioning of Carter's competence to solve governmental problems.

The greatest variation in the frequency of responses occurs on the dimensions of

charisma and personal characteristics. Presumably the frequency of these comments evidences greater variation across time because they reflect the uniqueness of particular candidates, rather than prototypical features. Eisenhower's military record, poor health, and advanced age in 1956, as well as Stevenson's divorce and Kennedy's religion were all vivid features that provoked comments in the early surveys. McGovern and Nixon, on the other hand, were not perceived as particularly unique in appearance or background, as evident from the relative lack of such remarks in 1972. Similarly, their lack of personal appeal can be seen by the equally low percentage of comments classified as charisma. In general, candidates running since 1964 have been less likely to evoke comments in terms of either charisma or personal characteristics.

The Continuity of Judgmental Criteria

The endurance of these prototypical judgmental criteria suggested by the aggregate figures is further supported by panel data. Those respondents who used the competence, integrity, or reliability dimensions for evaluating a candidate at one point in time were significantly more likely to employ the same dimension four years later, even though different candidates were now running for office (see

Table 3. Stability Across Time of Cognitive Dimensions Used in Evaluating Candidates, as Determined from Panel Data

Dimension	Percentage mentioning dimension in:			
	1960		1976	
	Who Used It in 1956	Who Did Not Use It in 1956	Who Used It in 1972	Who Did Not Use It in 1972
Competence	70	45	66	47
Integrity	49	23	59	38
Reliability	20	11	49	26
Charisma	38	20	21	16
Personal	52	41	35	33

Source: National Panel Studies, University of Michigan Center for Political Studies.

Table 3). For example, 70% of the 1956-60 panel respondents who mentioned competence in 1956 did so again in 1960, compared to only 45% for those who had not used competence in their evaluations in 1956. Similar differences were also generally found for the integrity and reliability categories in both the 1956-60 and 1972-76 panels.

The degree of continuity in the three main judgmental categories—competence, integrity, and reliability—when assessing candidates is significantly higher than might be expected given all the measurement problems involved in coding open-ended responses. This is not to imply, however, that the use of these criteria is unaffected by historical circumstances or the events of particular campaigns. On the contrary, the percentages for integrity and reliability in Table 3 reveal a general increase in these concerns during the 1970s, when citizens began to question whether politicians could and would do what they promised. Nevertheless, respondents who had employed one of the three major personal dimensions in the 1972 campaign were significantly more likely to use the very same criterion four years later when assessing two new candidates for the presidency. A much lower level of continuity was found for the dimensions of charisma and personal

characteristics (see Table 3). Such instability further demonstrates the sensitivity of these two criteria to the unique features of specific candidates.⁵

If, as suggested earlier, people schematically draw dispositional and behavioral inferences from the candidate's appearance, then a variety of observable features will apparently cue the same inferred performance-related criteria when judging candidates. In other words, a candidate's unique personal attributes or campaign style might call attention to some characteristic from which the voters would infer that he lacked competence, while leaving unchanged the overall basic structure employed in candidate appraisals.

Explaining Candidate Assessment Dimensions

The consistent predominance of comments referring to competence, integrity, and reliability offers a considerably different interpretation regarding the meaning and political relevance of personality responses than is standard in the literature. The finding that better-educated people are more likely to offer personal comments now seems more understandable. As can be seen from Table 4, the competence, integrity, and reliability dimensions are those most frequently

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Table 4. Average Number of Comments on Personal Attribute Dimensions, by Level of Education

Education	Competence	Integrity	Reliability	Charisma	Personal
Grade School	.58	.19	.19	.10	.37
High School	.76	.38	.29	.19	.47
College	1.15	.74	.66	.29	.48

Source: National Election Studies, University of Michigan Center for Political Studies.

Note: For the sake of parsimony the number of comments per dimension was averaged over the years 1952-1984 rather than presenting the figures for each year. Values for the individual years can be obtained by writing the authors.

mentioned by better-educated respondents. There is only a slight tendency for well-educated people to make more comments pertaining to the charisma and personal dimensions, and within education levels it is clear that these features are more salient to those voters with the least education. Therefore, a partial explanation for the decline noted earlier across time in comments referring to personal attributes may be the general rise in education that occurred during this period.

In sum, college-educated voters appear more likely to make inferences about the expected performance and internal dispositions of the candidates, whereas less-educated voters rely on more readily observable features. These observable features of the presidential contenders should cue inferences about the expected behavior of candidates according to the social cognition theory outlined above. Unique aspects of the candidates' looks, behavioral mannerisms, and background are often quite vivid and well known, thus providing relatively cost-free information used by many voters when evaluating candidates (Herstein, 1981). Therefore, we would expect that personal characteristics should carry more weight in schematic judgments made by the least-informed voters.

The evidence presented in Table 5 does, in fact, suggest that people—especially those with a low level of education—draw

inferences about the competence, integrity, reliability, and charisma of a candidate from the candidate's personal characteristics. For example, the average correlation of the personal characteristics dimension with the other four was larger ($r = .14$) than the average correlation among the four substantive dimensions ($r = .08$). Although the multiple correlations in Table 5 are quite low for the college educated they are significant, thereby indicating that personal features play some role in their judgments about a candidate's prospective performance. These personal cues are clearly more important for the less well educated, though, as predicted by the theory. The only exception to this pattern occurs in 1980, due primarily to the tendency of better-educated voters to connect Carter's religious beliefs with his honesty. When these comments are removed, the multiple correlations return to the pattern found for the other years. However, this does suggest that what people personally value may determine which inferences get connected with particular characteristics.

Beyond education, it is also important to investigate other factors that influence the relative use of the various prototypical candidate judgments. Previous research on schemas (Markus, 1977; Tesser, 1978) suggests that better-informed voters should have a greater store of previously gathered information upon which to base candidate inferences. By following the

Table 5. Multiple Correlation between Evaluations of Personal Candidate Characteristics and Competence, Integrity, Reliability, and Charisma, by Education

Education	1952	1956	1960	1964	1968	1972	1976	1980	1984
Grade School	.30	.25	.24	.19	.25	.26	.35	.19	.21
High School	.18	.14	.19	.13	.18	.15	.16	.18	.19
College	.09	.13	.08	.11	.12	.14	.11	.20	.12

Source: National Election Studies, University of Michigan Center for Political Studies.

media they should have a clearer perception of the political environment, thereby fostering richer inferences. Similarly, more politically interested citizens will have spent more time thinking about the candidates, and therefore be more likely to report central attitudes and themes rather than easily observable characteristics.

A test of these hypotheses was accomplished with a multivariate analysis predicting the emphasis given to each dimension of candidate appraisal. Included among the independent variables, besides education, were (1) campaign interest and strength of party identification, to measure personal involvement in politics; (2) media exposure, to indicate familiarity with campaign information; and (3) a

measure of articulateness, to guard against the possibility that the results simply reflect verbosity.

The results of the analysis demonstrate that interest in politics does significantly increase the likelihood of making performance-relevant inferences about the candidates—especially with regard to competence (see Table 6). However, political interest has no significant impact on references to charisma or to personal characteristics. It is also clear that information the media provides is relevant primarily to making competence inferences, and also focuses attention on the candidates' charisma—no doubt by informing voters of the candidates' previous experience and how they are performing in the campaign. Strength of party identifica-

Table 6. Multivariate Analysis Predicting the Origin of the Schematic Criteria People Use When Evaluating Candidates

Predictors	Competence	Integrity	Reliability	Charisma	Personal
Education	.12*	.14*	.17*	.09	.04
Political interest	.14*	.10*	.10*	.06	.08
Media exposure	.11*	.06	.05	.13*	.06
Strength of party identification	.03	.02	.04	.04	.03
Articulateness	.14*	.17*	.16*	.16*	.12*
Multiple R	.38	.33	.32	.29	.17

Source: National Election Studies, University of Michigan Center for Political Studies.

Note: Table entries are standardized regression coefficients and multiple R. The values are averages of the coefficients obtained for the nine presidential election surveys from 1952 to 1984, as are the significance figures. The individual figures for each study may be obtained by writing the authors. The dependent variable was the number of comments made in each of the five judgmental domains.

* $p < .01$.

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tion, on the other hand, apparently plays no role in triggering either personal or performance-related judgments.

Of particular interest in Table 6 is the fact that education remains significantly related to one's attention to competence, integrity, and reliability, even after controlling for the effects of political interest, media usage, and articulateness. Therefore, this relationship is not simply the result of college-educated people being more politically interested and attentive to the media. A plausible alternative explanation is that these particular criteria are more relevant to the personal values of college-educated citizens. Perhaps better-educated people place greater emphasis on these characteristics in their own lives or in the schemata they apply to themselves, which they in turn use to evaluate candidates. In addition, formal education acts to socialize people into cultural norms regarding how a president ought to act. If the expectation that political leaders ought to be competent, honest, and reliable is largely determined culturally, then the relationship with education is quite understandable. Given the somewhat weak multiple correlations of Table 6, however, it is apparent that further research is needed in order to reach a definitive answer for why citizens apply particular prototypic criteria in their cognitions of political leaders.

In sum, empirical analysis reveals substantial variation in the inferential richness of candidate schemas from one individual to the next. College-educated people, for example, place more emphasis on performance criteria, which they infer from a broader information store, while the less well educated base their inferences more directly on the observable attributes of the candidates. There is also variation in the relative prevalence of particular judgmental criteria across time, thus revealing how unique features of the candidates or different campaign events influence cognitions of presidential contend-

ers. Despite this variation, the general structure of the prototypic criteria remains in effect, guiding judgments about even relatively unknown persons who emerge as presidential candidates.

Extensions to Affective Evaluations and the Vote

As we have been reminded lately, politics—and human interactions more generally—usually have an emotional or affective undercurrent (Zajonc, 1980). The very act of assessing candidates implies the notion of preference or affect. Thus, a brief examination of how schematic judgments are associated with affective candidate evaluations and the vote is clearly in order. For the sake of brevity, we will consider only a limited set of expectations regarding the link between cognitive and affective candidate evaluations. For example, at a minimum, we would expect that criteria that are the most frequently used in candidate assessments should also weigh most heavily in overall affective evaluations (Tesser, 1978). If, as we argue, people evaluate candidates on the basis of inferences they draw from salient cues, then abstract inferences should be more strongly related to affective ratings and the vote than to the readily observable characteristics of the candidates. In addition, it follows that performance-relevant criteria should predict candidate affect better for college-educated respondents, whereas personal characteristics will be relatively more important for the less educated.

It is widely recognized that during the period from 1952 to 1980 presidential candidates in general became less personally popular with the public (cf. Miller and Wattenberg, 1981; Rosenstone, Behr, and Lazarus, 1984, p. 225). This rise in negative evaluations did not, however, occur equally across the five categories we have identified. A separate analysis reveals that for the population as a whole,

Table 7. Average Regression Coefficients Predicting Affective Candidate Ratings and the Presidential Vote from Schematic Judgments, by Education

Dimensions	Grade School	High School	College
<i>Predicting Democratic Candidate Feeling</i>			
<i>Thermometer Ratings from:</i>			
Competence	.29	.25	.26
Integrity	.17	.25	.27
Reliability	.10	.13	.15
Charisma	.07	.08	.07
Personal	.16	.13	.07
Multiple R	.45	.47	.47
<i>Predicting Republican Candidate Feeling</i>			
<i>Thermometer Ratings from:</i>			
Competence	.28	.28	.33
Integrity	.16	.21	.22
Reliability	.12	.15	.17
Charisma	.07	.08	.06
Personal	.13	.12	.05
Multiple R	.44	.47	.51
<i>Predicting Presidential Two-Party Vote from:</i>			
Competence	.31	.30	.36
Integrity	.17	.21	.23
Reliability	.11	.12	.15
Charisma	.08	.09	.07
Personal	.16	.12	.07
Multiple R	.52	.54	.55

Source: National Election Studies, University of Michigan Center for Political Studies.

Note: All coefficients are averages for all the years available. The feeling thermometers range from 1968 to 1984, whereas the vote is available for all years. The independent variables employed in this analysis were computed by using the comments in each domain to derive the net balance between positive and negative responses for each candidate.

the increase in negative assessments is most evident in the areas of competence and reliability. As hypothesized, the rise in negative comments varied in terms of content across education levels. Among the better educated, negative comments on competence and reliability increased markedly, whereas comments regarding integrity, charisma, and personal characteristics revealed little systematic rise in negative judgments. The less well educated exhibited a relatively smaller increase in the percentage assessing candidate competence and reliability negatively, with the key difference being that respondents with a grade school educa-

tion also became increasingly negative in their statements about the personal characteristics of the candidates (e.g., Reagan's age or Carter's southern roots in 1980).

These differences also appear to influence affect toward candidates and the vote choice in the manner suggested by Tesser (1978). As can be seen in Table 7, the regressions predicting candidate thermometer ratings and the vote reveal a substantially greater impact for the performance-relevant criteria than for the more superficial and apparent candidate characteristics. Moreover, competence, integrity, and reliability judgments gener-

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ally increase in importance as education rises, while the impact of personal characteristics declines. The major exception to this pattern occurs for the impact of competence on the ratings of Democratic candidates. The somewhat higher average coefficient for competence among those with a grade school education occurs because their ratings of Carter were strongly associated with their belief in 1980 that he had gained valuable experience during his term in office. For the remaining three election studies, the competence coefficients fit the predicted pattern.

In general, the personal attributes of candidates play a more central role in the political cognitions of less well-educated citizens. Yet, even among these respondents, inferences about whether a candidate will perform competently, reliably, and honestly far outweigh considerations of the candidate's appearance. While voters might base their inferences about the candidate's future behavior on observable characteristics, actions, and other readily accessible political cues, it is the performance or instrumental inferences, rather than the observable features, that become linked with the vote decision. The variation in the magnitude of the coefficients across the different dimensions and education groups also fits with Tesser's (1978) suggestion that evaluations vary depending upon individual differences in cognitive schema.

Conclusion

The evidence presented above provides consistent support for the theory that Americans evaluate presidential candidates on the basis of a limited set of general and enduring criteria. In other words, people have a preexisting knowledge structure, or schema, concerning what a president should be like, and judge real candidates according to how well they match the elements of these schemas.

While these criteria have remained quite stable over time, the relative importance of particular categories has varied with changes in historical circumstances. The primary dimension used by citizens throughout the 1952-1984 period has been competence, clearly a performance-related criteria. Integrity and reliability have become more prevalent in judgments of candidates since 1964. These broad expectations that citizens hold about presidential performance appear to reflect in part the actions of past presidents and in part the agenda as set by the media or by current candidates.

The cognitive process underlying the evaluation of candidates, which we have described above, is clearly a dynamic one involving an interaction between the individual and the political environment. Voters abstract from their experience of past presidents those features and behaviors they associate with political success, and then evaluate other candidates with respect to these same characteristics. During the campaign the candidates no doubt emphasize certain characteristics in ways that reflect on or cue judgments of their competence, integrity, and reliability, because they believe these are relevant to the conduct of the office. Voters in turn respond to these campaign messages not only because they are relevant to their schema for presidential candidates, but also because these are the terms in which the political dialogue is conducted. Particular events of the campaign and the unique strengths and weaknesses of the specific candidates, as well as cognitive and personal variation across individual voters, focus attention on certain of these criteria, giving more emphasis to some and less to others. Nevertheless, despite some variation in the specific content of political cognitions across time, the general structure of the abstracted inferences and judgments derived from the historical particulars remains fairly stable over the years.⁶

Unfortunately, the micro-level cognitive processes involved in evaluating candidates remain less clearly specified than the content and structure of candidate schemas. A number of alternative models describing the evaluation process could be examined. For example, voters might evaluate candidates by comparing each one individually with some abstract ideal or exemplary president. According to this model, a voter for Reagan would prefer him because he is as competent as their ideal president was or should be, independent of what they think about Mondale. An alternative model would have the voter explicitly comparing the two candidates with respect to the various prototypic categories (e.g., is Reagan more competent than Mondale?).

Differentiating between alternative cognitive process models at the individual level can be important to predicting overall candidate preferences. The open-ended questions from the National Election Studies are not, however, ideally designed for specifying these models. Further analysis with different forms of data in addition to the open-ended items is necessary before they can be fully tested.

Much work clearly remains to be done. Nevertheless, the relevance of the research presented here is that it offers a theoretical framework differing from previous approaches, and within which future work can be done. Earlier treatments of the candidate factor, especially the rational choice theories or those employed in *The American Voter*, de-emphasized the cognitions and processes discussed here by treating them as idiosyncratic, irrational, and piecemeal rather than systematic and instrumental.

The enduring quality of the judgmental criteria over time challenges the conception of candidates as short-term forces presented in *The American Voter*. While the affective response to particular candidates has shown considerable volatility over the years, recent evidence reveals

greater stability in the affective ratings of some political leaders than in feelings toward the Democrats or Republicans (see Wattenberg, 1984, ch. 2). Many politicians remain in office for so long and become salient enough to the general public to be considered long-term forces. Moreover, despite the fact that each candidate brings a unique set of personal characteristics to the public's attention, inferences based on these features do reflect more stable and enduring evaluative categories.

Evaluating candidates on the basis of personal qualities has for years been regarded as emotional, irrational, and lacking in political relevance. This interpretation arose in part because candidate evaluations were considered to be uninformed idiosyncratic responses based on superficial criteria. The evidence now suggests that a reinterpretation is clearly needed. Rather than represent a concern with appearance, candidate assessments actually concentrate on instrumental concerns about the manner in which a candidate would conduct governmental affairs. In general, they represent a schematic conception of the president as someone who can be relied upon to deal competently with the nation's problems in an honest and even-handed manner.

Appendix

The following is a list of the codes from the National Election Studies (NES) which form each of the five personal dimensions for our baseline year of 1972. In earlier years some of the codes on different dimensions were combined, necessitating the reading of the actual protocols for these cases to sort out the various codes and maintain consistency from year to year.

Integrity

313. A politician/political person; (too) much in politics; a good politician

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- 314. Not a politician; not in politics; above politics; a bad politician
- 401. Honest/sincere; keeps promises; man of integrity; means what he says; not tricky
- 402. Dishonest/insincere; breaks promises; no integrity; doesn't mean what he says; tricky
- 403. Man of high principles/ideals; high moral purpose; idealistic
- 404. Lacks principles/ideals; not idealistic
- 603. Honest government; not corrupt; no "mess in Washington"
- 604. Dishonest/corrupt government; "mess in Washington"

Reliability

- 213. Dependable/reliable; a man you can trust with the responsibilities of government ("trust" in the capability sense, rather than the honesty sense)
- 214. Undependable/unreliable; a man you can't trust with the responsibilities of government
- 319. (too) careful/cautious
- 320. (too) impulsive/careless
- 407. Public servant; man of duty; conscientious; hard-working; would be a full time President
- 408. Does not take public service seriously; lazy; would be a part-time President
- 431. Unsafe/unstable; dictatorial; craves power
- 432. Safe/stable
- 709. Good for country; has country's best interests at heart
- 710. Bad for country; doesn't have country's best interests at heart

Competence

- 201. General reference to him as "a good/bad man"; R has heard good/bad things about him; qualifications; general ability (low priority code)
- 211. Experienced
- 212. Inexperienced
- 217. His record in public service; how well he's performed in previous offices
- 218. Has government experience/political experience
- 219. Lacks government experience/political experience
- 220. A statesman; has diplomatic experience
- 221. Not a statesman; lacks diplomatic experience
- 315. Independent; no one runs him; his own boss
- 316. Not independent; run by others; not his own man/boss
- 413. Understands the nation's problems; well-informed
- 414. Doesn't understand the nation's problems; poorly informed
- 415. Idealistic/pragmatic/practical; down-to-earth; not too idealistic
- 416. Too idealistic
- 417. Uses common sense; makes a lot of sense

- 418. Not sensible; impractical
- 419. (too) well educated; scholarly
- 420. Poorly educated; unschooled
- 421. Intelligent/smart
- 422. Unintelligent/stupid/dumb
- 601. Good/efficient/businesslike administration
- 602. Bad/inefficient/unbusinesslike administration
- 609. General assessment of job he would do; he'd be a good/bad President, provide good/bad administration (low priority code)
- 707. Candidate as good protector; will know what to do
- 708. Candidate as bad protector; won't know what to do

Charisma

- 301. Dignified/has dignity
- 302. Undignified/lacks dignity
- 305. Inspiring; a man you can follow; "a leader"
- 306. Uninspiring; not a man you can follow; not a leader
- 307. People have confidence in him
- 308. People don't have confidence in him
- 309. Good at communicating with blacks, young people, other "problem" groups
- 310. Bad at communicating with blacks, young people, other "problem" groups
- 311. Knows how to handle people (at personal level)
- 312. Doesn't know how to handle people (at personal level)
- 317. Humble; knows his limitations; doesn't pretend to know all the answers
- 318. Not humble enough; too cocky/self-confident
- 411. Patriotic
- 412. Unpatriotic
- 433. Sense of humor; jokes a lot/(too much)
- 434. No sense of humor/humorless/(too) serious
- 435. Kind/warm/gentle
- 436. Cold/alooof
- 437. Likeable/gets along with people
- 438. Unlikeable/can't get along with people
- 439. Democratic (in nonpartisan sense)
- 440. Undemocratic (in nonpartisan sense)
- 441. High-fallutin/high-brow; talks in circles; can't talk to common man; can't communicate ideas well
- 442. Not high-fallutin/low-brow; talks straight; can talk to common man
- 703. Will save America; America needs him
- 704. Will ruin America; last thing America needs
- 705. Will unite Americans/bring people together
- 706. Will divide Americans/drive people apart

Personal

- 215. A military man; a good military/war record
- 216. Not a military man; bad military/war record; no military/war record
- 423. Religious; "moral" (in religious sense); God-fearing

- 424. Irreligious; "immoral" (in religious sense).
- 425. Self-made; not well off; started out as a poor boy
- 426. Wealthy; rich; born with silver spoon in his mouth
- 443. Well-known
- 444. Unknown/not well-known
- 445. Reference to his family
- 446. Reference to his wife
- 447. His speaking ability
- 448. His health
- 449. His appearance/looks/face/appearance on TV
- 450. His age
- 451. (too) old
- 452. (too) young
- 453. Mature
- 454. Immature

Notes

1. The actual open-ended questions from the NES were as follows: Is there anything in particular about [Democratic candidate] that might make you want to vote for him? Is there anything in particular about [Democratic candidate] that might make you want to vote against him? These questions were then repeated with regard to the Republican candidate. Thus, one possible reason for the prevalence of personal responses is that the items focus the respondent's attention on the candidate rather than on the issues or parties.

2. In a similar analysis, but using LISREL to correct for measurement error, Lau (1985) obtained across-time correlations of .80 for the personal comments, .32 for party, and .52 for issues in the 1972-1976 panel.

3. Articulatness was measured in terms of the number of comments made in response to another set of open-ended questions asking what people liked and disliked about the two parties.

4. The factor analysis proceeded by first forming a series of dummy variables for all the codes in the open-ended questions that deal with candidate attributes. The dummy variables were coded one if the respondent mentioned a particular candidate characteristic, and zero if no mention was made. A tetrachoric r correlation matrix was then computed for all the resulting dummy variables in the 1956, 1972, and 1976 surveys, and subjected to factor analysis using both orthogonal and oblique rotations. Selecting only those factors with eigenvalues greater than 1.0 produced a five-dimensional solution that accounted for 68%, 65%, and 71% of the total item variance in 1956, 1972, and 1976, respectively.

The final solutions for each of the factor analyses were then compared to determine their degree of similarity. Using the OSIRIS COMPARE program with 1972 as the target configuration, measures of fit

(normalized symmetric error ranging from .028 to .031) indicated that the three different solutions were virtually identical (Schonemann and Carroll, 1970). In addition, an examination of the correlations among the measures created to indicate each of the five dimensions suggested that the structure underlying the comments was similar in all three years. Given these comparisons, it was decided that further application of these techniques to the survey data for other years was not necessary. Please see the Appendix for information on the specific code categories incorporated into the measure for each of the five dimensions.

5. A multivariate analysis with panel data confirms the results of Table 3. Mention of a dimension in 1972 was used to predict the use of the same dimension in 1976 after controlling for education, strength of party identification, and media usage. The results demonstrate that the prior use of the competence, integrity, and reliability dimensions significantly predicted (better than .001 level) the subsequent use of these dimensions, whereas no significant impact across time was found for the charisma or personal dimensions.

6. Recent research has suggested that a new "compassion" or "empathy" dimension of candidate evaluations may be emerging since 1980 (Kinder, 1985). A closer examination of the open-ended NES responses relevant to this theme, however, reveals little support for this contention. The percentage of comments that referred to codes encompassing compassion (codes 435, 436, 807, 808, 831, and 832) was only 3.3% in 1984, compared to 3% in 1980. Our analysis shows that closed-ended items that initially may be thought of as compassion, such as "in touch with ordinary people" or "really cares about people like me," are largely incorporated by the group benefits dimension in the open-ended responses.

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THE PRESIDENT AND THE POLITICAL USE OF FORCE

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Throughout the post-World War II period the president has been called upon to make decisions concerning the use of force as a political instrument. The explanation that is offered is based upon a characterization of the president as a cybernetic human decision maker facing limitations. These limitations, in conjunction with the complexity of the environment, lead presidents to develop and use a relatively simple decision rule. The dependent variable, which is the probability of the use of force at any point in time, is explained in terms of enduring and essential concerns, which are operationalized as coming from the international, domestic, and personal environments. Data are taken from Blechman and Kaplan's Force Without War. On the basis of our estimation and evaluation, presidential decisions to use force are based on factors in all three arenas.

As the leader of one of the world's great powers, the president of the United States is charged with the responsibility of guiding and implementing policies to protect and advance U.S. interests abroad. This is an onerous responsibility; the fortunes of various presidents have risen or fallen on the basis of the American public's satisfaction with their performance in this capacity. However, this responsibility also represents an arena of decision making in which the president has a relatively free hand to operate according to his own dictates, and one in which he can command on his own a wide variety of instruments of policy in implementing his intentions.

One such option is obviously the use of the United States military. With extensive capabilities and global reach of the U.S.

armed forces, components of it can be directed by the president to undertake a broad spectrum of foreign policy activities, ranging from the transporting of military equipment and advisors, to "showing the flag" with minor maneuvers, to engaging in military combat with an opponent. Certainly the most momentous and costly act of foreign policy is the fighting of a full-scale war. Our interest, however, is focused on the use of the U.S. military by the president in circumstances short of involvement, or intended involvement, in extended military combat. In these instances, the armed forces may be said to have been engaged not for the achievement of a military objective per se, but for "political" purposes, and their actions said to constitute "political uses of the armed forces"—that is, overt policy acts directed by the U.S. president that

fall somewhere between acts of diplomacy and intentional uses of military power such as in Korea and Vietnam.

In a recent study, Blechman and Kaplan (1978) demonstrate convincingly that the use of force has proved, in the postwar era, to have been a frequently employed instrument of foreign policy. They show, for example, that between the years 1946 and 1976, the U.S. deployed military units abroad for political purposes 226 times. In light of this historical record, it is surprising to find that little has been written about these uses of force, as a set of events, or about the use of force as an instrument of U.S. policy, or about the decision-making process which precipitates these results.¹

The Political Use of Major Force

Blechman and Kaplan (1978, p. 12) define the political use of armed forces as

physical actions . . . taken by one or more components of the uniformed armed military services as part of a deliberate attempt by the national authorities to influence or be prepared to influence, specific behavior of individuals in another nation without engaging in a continuing contest of violence.

For these authors, decisions by the U.S. to use force in this manner are political in that actions are taken for nonmilitary objectives, and in that they take effect within the external environment of the U.S., involving the clash of the international political interests of many parties. Blechman and Kaplan go on, in their empirical work, to examine and classify the various types of international situations in which uses of force were undertaken, and to assess the effectiveness of different sequences and configurations of U.S. force deployments. Our interest in the political use of force by the U.S. differs from and extends the work of Blechman and Kaplan in three ways.

First, we wish to concentrate on the

subset of Blechman and Kaplan's data in which *major* or *nuclear-capable* levels of force were used.² These uses, hereafter referred to as *major uses of force*, are presented in the appendix. We maintain that by restricting our focus to these incidents we have isolated the most important and consequential of U.S. political uses of force in this period.

Second, we posit that these incidents can be specifically characterized and studied as a set of presidential foreign policy decisions. This assumption rests on the facts that the president bears legal responsibility for the use of force, must sanction such uses, and will be given credit or blame for the success or failure of these ventures. Since the president is ultimately responsible, we assume either that he was directly consulted and gave his consent before any major force deployment was made, or that a choice was made according to his established guidelines. We are aware that the degree of direct involvement in these incidents was variable. However, a search of post-war presidential biographies and memoirs indicates that, with very few exceptions, all the major uses of force listed in the appendix were recognized as prominent presidential decisions.

Third, having identified the president as the relevant decision maker regarding political uses of major force, we wish to construct a model of his decision-making process. The president will be viewed as operating within three capacities—as chief executive, commander in chief, and political leader. He operates over time with the goal of effectively managing, or simultaneously balancing, his interests in the international, domestic, and political arenas. The president is faced with the need to monitor these various fronts, with a good deal of uncertainty about the effects of chosen forceful actions. In seizing certain opportunities to use force but rejecting others, the president clearly operates in a “political” fashion. He

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assesses a range of actors, not only in the international context, but also in the American domestic context and in the context of his political leadership. The president will, for instance, consider his domestic political standing, his relations with Congress, the public's attention or inattention to foreign policy matters, the public's dissatisfaction with the progress of the economy, etc. Also, whether an election is forthcoming and whether the president has a strong resource base of popular support may well be important in presidential calculations about acting in the international arena. In short, the set of variables monitored by the president, in the context of a decision on the major use of force is likely to come from international, domestic, and political sources.

Thus, as part of a larger research effort, this study attempts several steps toward a better understanding of the political uses of force by the United States and of presidential foreign policy decision making. By constructing a model that takes into account the trifold context in which decisions to use force are made, positing the effects of foreign, domestic, and political contextual factors, we provide a more complete understanding than has been previously available from studies which examine a less complete and/or exclusively domestic or international set of factors (e.g., Blechman and Kaplan, 1978; Mahoney, 1976; Stoll, 1984). Furthermore, our model of presidential decision making assumes that presidents throughout the postwar period have behaved as boundedly rational, cybernetic decision makers. We build upon and extend the work of Simon (1959, 1969, 1979), Steinbruner (1974) and others who established this tradition in the foreign policy decision-making literature.

The second major goal of this study is the operationalization of the decision-making model and its application to the Blechman and Kaplan (1978) data set of major uses of force in order to see how

well the model, as specified, accounts for the occurrence and nonoccurrence of uses of force on a quarterly basis from 1948 to 1976—that is, the presidential terms of Truman through Ford.³ Upon presenting our results we go on to assess the overall success of the model, looking at the relative impact of international, domestic, and political factors, and to examine how well it captures the record of individual presidents, testing our assumption that a common cybernetic mode of decision making effectively accounts for their actions.

A Cybernetic Model of Presidential Decision Making

There is widespread agreement in both the crisis and non-crisis literature as to the three competing characterizations of decision making—rational actor, cybernetic, and cognitive process (e.g., Gallhofer and Saris, 1979; Maoz, 1981; Ostrom, 1978; Steinbruner, 1974). In this regard, we assume that the president behaves not as a rational decision maker, but in a fashion similar to that suggested by the cybernetic approach to decision making. Operating in a context that has been described as "structural uncertainty" (Steinbruner, 1974, p. 18), the president is not able to determine the state of the environment, locate available alternatives, or assess the consequences of those alternatives—in short, the raw materials of rational choice are absent. In place of the rational choice perspective, we will argue that the cybernetic perspective is more appropriate, because it provides an understanding of how human decision makers reach decisions in highly complex and volatile environments by formulating simple and manageable decision algorithms. The mechanics of choice are simple: the president monitors a limited set of essential or critical factors, and considers a restricted set of decision options. Choice is tied to

the essential variables by a relatively simple decision rule.

Underlying the cybernetic characterization of decision making is the following principle: "A man, viewed as a behaving system, is quite simple. The apparent complexity of his behavior over time is largely a reflection of the complexity of the environment in which he finds himself" (Simon, 1969, p. 25).⁴ To model the environmental connection in a cybernetic fashion, Simon (1959) argues that it is necessary to take into account (a) the cognitive structure of the president-as-a-decision-maker, (b) the formulation and content of his decision premises, and (c) the logic of the inference process (or the decision rule) followed to reach a decision. We turn now to the task of delineating each of these specifically for a model designed to explain the series of U.S. presidential decisions to use major levels of military force in foreign affairs in a political fashion.

Cognitive Structure

A process of simplification will lead the president to develop a stable means for dealing with the complexity of the environment. First, as a decision maker, he will focus on a small and relatively fixed number of environmental factors that will be regularly monitored. The fact that the president operates at the nexus of numerous actors who are "pushing and pulling" him means that he will constantly be reminded of various factors that need to be taken into consideration. Second, the president, however, will perceive these multiple inputs in terms of quite gross distinctions. He will not attempt to catalog exhaustively the actual state of the environment and each and every one of the options or decision choices open to him; instead, only very general aspects of the environment will be monitored. In short, we assume that the president will have a stable cognitive structure that conditions the decision to use force.

We further assume that all presidents in the postwar era have adopted, to a large extent, a similar role in office, and thus may be characterized by a common model of decision making. As Simon (1959, p. 274) points out, "A role is a social prescription of some, but not all, of the premises that enter into an individual's choices of behavior." In these terms, the role of president brings with it certain premises that will be shared by all occupants of the Oval Office. Insofar as the political use of force is concerned, a similar role is manifested by presidents not because they all share all of the same individual beliefs or possess similar personalities, but rather because they are assumed to share three basic overarching goals: a preference for action, anti-communism, and "containment" of the U.S.S.R. Kegley and Wittkopf (1982, p. 36), in summarizing the literature, argue that these three tenets have remained uppermost in the minds of U.S. foreign policy makers:

1. The United States must reject isolationism permanently and substitute for it an active responsibility for the direction of international affairs.
2. Communism comprises the principal danger in the world, and the United States must use its power to combat the spread of this menace.
3. Because the Soviet Union is the spearhead of the communist challenge, American foreign policy must be dedicated to the containment of Soviet expansionism and influence.

These beliefs have led U.S. presidents to identify most unrest and turmoil as the result of the international communist movement, and to focus on the U.S.S.R. as the primary challenger to U.S. international interests. Furthermore, these beliefs have created a presumption that forceful actions are a necessary component of the containment strategy. In short, political uses of the military repre-

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sent a very important component of U.S. foreign policy.

As a result, these shared assumptions will have led all presidents to react, evaluate, and behave in a similar fashion. The differences that one might ascribe to the different men that have occupied the Oval Office are, in part, a result of the differences in the contextual configurations (i.e., the environments) in which they were forced to operate. It shall be assumed that presidents have operated according to a common model of decision making by structuring and simplifying their need for information; by interpreting their role as entailing the monitoring and management of the domestic, international, and political environments; and by focusing generally upon the same essential concerns (termed *decision premises* below) in each of these environments.

In practice we will construct and apply a model to explain the use of force decisions throughout the 28-year span from 1949 to 1976. However, after assessing the capacity of this model in general terms, attention will focus on whether the decisions of some presidents are not accounted for as well as others—that is, is there an indication of idiosyncratic behavior or is there support for the validity of our assumption of commonality across individual office holders?

Decision Premises

The cybernetic decision maker structures his consideration of information and alternatives around a select number of *decision premises*, each premise specifying the "computational procedures for assessing the state of the environment and its implications for action" (Simon, 1959, p. 274). Following this logic, we maintain that the president selects or establishes his decision premises on the basis of his three major functional responsibilities: commander-in-chief, chief executive, and political leader. As commander of the U.S. military, the president is bound to

protect the interests of the United States and to maintain strength and credibility in the ability of the U.S. to realize and protect its interests. Critical indicators of his achievement in this capacity, not only for himself but also for the U.S. public and for our opponents and allies abroad, will be matters such as the state of relations with our superpower adversary and the status of the relative strategic balance. As chief executive, the president is expected to meet the expectations of the American people at large concerning peace, prosperity, domestic tranquility, and leadership. In order to do so, he must not only monitor and act to preserve and enhance his power and credibility in the national environment, but must also be alert to "transfer effects" of his actions in the external arena (and vice versa). From this perspective, the president will be sensitive to public attitudes towards international tensions and involvement, as well as to domestic considerations such as the state of the economy. Finally, as political leader of the government, the president will be concerned with maintaining and enhancing a "political resource base" in order to assure his political survival, freedom of action, and the electoral fortunes of his party. Doing so naturally focuses his attention on factors such as his current and relative popularity and the U.S. electoral calendar.

Thus, the president will be viewed as operating within a tripartite context. He will be seen to monitor salient dimensions in the domestic, international, and political arenas, and in his decision making will assess the relative importance of each of these dimensions before taking action. Based upon our earlier discussion and assumptions about the president as a cybernetic decision maker, we view the number of such dimensions as limited. To be specific, we posit that the decision premises of the president are structured according to the following list:

I. International environment

- A. Level of international tension
- B. Relative strategic balance
- C. Extent of U.S. involvement in ongoing war
- II. Domestic environment
 - A. Public attitude toward risks of international involvement—international tension
 - B. Public attitude toward risks of international involvement—strategic balance
 - C. Public aversion to war
 - D. Condition of the domestic economy
- III. Political environment
 - A. Level of public support
 - B. Overall political success
 - C. Position on the electoral calendar

We proceed to provide a brief rationale for each of these separate decision criteria. Although this list is admittedly abbreviated, our model advances previous considerations of presidential foreign policy behavior by (a) positing a tripartite distinction among his decision criteria and (b) explicitly taking into account the potential impact of domestic conditions and public attitudes in determining external actions taken by the president.

Decision Premises:

The International Environment

Any deployment of major force by the U.S. can be expected to have international ramifications—escalation, involvement of the U.S.S.R., further commitment of men and material. The president is seen as considering the following three international decision premises to determine the likely consequences of a use of force.

Level of international tension. The degree of tension in the international system is determined by the aggregation of the actions and statements of a wide variety of actors. However, while many nations

play a role, the U.S. and U.S.S.R. are the most active and direct contributors to the overall level of international tension. Furthermore, the basic goals of U.S. foreign policy have led presidents to view the U.S.S.R. as the primary adversary of the U.S. Indeed, if there are to be any ramifications emanating from a use of force, it is likely that they will involve the U.S.S.R. Hence, the president will be centrally cognizant of the level of tension existing between the two superpowers. Furthermore, we posit that, *ceteris paribus*, the higher the existing tensions between the superpowers, the more likely it will be for the president to consider acting in a forceful manner. Failure to act or backing away from confrontations means that the president runs the risk of diminished stature or resolve (see Wills, 1982, on Kennedy). A president will seek to maintain his international and domestic credibility by indicating his willingness to use force—particularly, perhaps, for “political” ends—so as to demonstrate simultaneously U.S. resolve to act tough and U.S. restraint at not engaging in direct conflict (Schell, 1976, pp. 366–67).

Relative strategic balance. In both symbolic and real terms, the U.S. is preoccupied with its relative strategic power (e.g., Prados, 1982). A key consideration is whether the perceived strategic nuclear balance has an impact on the exercise of force for political purposes. Despite the fact that the use of strategic capabilities may not, in any given situation, be directly or immediately contemplated, there will be a level of assurance and confidence provided the president by the perception that the U.S. is strategically dominant within the international environment as a whole. It is possible that an increased propensity to use force grows out of perceived strategic inferiority; the less secure the president is, the more likely he is to engage in bellicose actions to demonstrate to our opponents that the U.S. is not

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cowed. This suggests, in turn, that the greater the dominance, the less likely the president will feel the need to run either the risks that could arise from the escalation of local or regional violence, or the direct or indirect involvement of the U.S.S.R. We subscribe to this view and posit that the president has little to gain when the U.S. is strategically dominant. In such circumstances, if the use of force is not successful, the symbolic power attached to strategic dominance will be reduced. Since it seems that risks of use may, other things being equal, outweigh the gains, we argue that the greater the strategic dominance, the lower the propensity to use force.

Extent of U.S. involvement in war. During the post-World War II era, the U.S. has become involved in two wars: Korea and Vietnam. In each instance, the commitment in terms of lives and funds has been great, and involvement in war has brought the president under intense and close scrutiny (e.g., Mueller, 1973). The longer a conflict drags on, the more risky and costly it becomes for the president. For example, an enemy offensive may indicate the U.S. commitment is less than sufficient or poorly orchestrated; battle casualties and/or troop levels increase the importance of victory (Gelb and Betts, 1979). Citizen dissatisfaction with a war ultimately can undermine the president's overall ability to govern effectively (e.g., Lyndon Johnson in 1968). Thus, once involved in a major regional conflict, the president will concentrate most of his energies on the conduct of the war. Military manpower and reserves will be focused on the actual violence, and, as U.S. involvement in a fighting war intensifies, the president will be less likely to contemplate undertaking other actions that, even if noncombative to start, run the risk of engaging the military in further action, especially in other regional contexts.

Decision Premises: The Domestic Environment

There are significant ramifications in the domestic arena for the use of force abroad. If there is a general consensus on the part of the public that the risks associated with such actions are acceptable, the credibility and future effectiveness of the president may be enhanced. However, the converse will be true if the public does not feel that the risks are acceptable, or if the venture is unsuccessful (e.g., Jimmy Carter and the abortive hostage rescue mission, 1980). We assume that the following four decision premises represent the president's attention to the domestic environment in his consideration of the use of force.

Public attitude toward risk of international involvement—international tension. The president's stability and willingness to participate in international affairs will be affected not only by his assessment of international tension, but also by the public's perception of this tension level and the attendant risks of international involvement by the U.S. and its personnel. In fact, his estimate of this latter effect may be more important than his individual "objective" assessment. To take into account this "filtered" or interactive effect, the president will be alert to the issues that concern the public and the extent to which its feelings are optimistic or pessimistic regarding action in the international arena. Thus, there are times when the American public views foreign affairs with trepidation because of the perceived danger of major foreign involvement or an elevated possibility of nuclear war. When international tension is high and the general public expresses concern about international affairs, as was true throughout the 1945–62 Cold War era, the consequences, both foreign and domestic, of presidential failure will increase. Therefore, we posit that during

those periods when the American public views foreign affairs with trepidation, higher levels of international tension will cause the president to lower his willingness or propensity to use force in a political fashion in foreign affairs.

Public attitude toward the risk of international involvement—strategic balance. To the extent, however, that the public views the U.S. as maintaining strategic dominance over its major rivals, the public's focus on foreign affairs may be interpreted, unlike instances of tension, as a call to action. In such circumstances, the political use of force will be seen as a less risky undertaking, perhaps even as a legitimate expectation of the U.S. in a role as world peacemaker or policeman or defender of democracy. The president, cognizant of these attitudes, will reverse his propensity to use force (as far as strategic balance is concerned). We argue, therefore, that whenever the American public views foreign policy matters as most important, the previously noted reluctance to use force in the face of a favorable strategic balance will be offset. In short, the risks will be worth taking.

Public aversion to war. If the U.S. is involved in a military confrontation resulting in the loss of life of U.S. service personnel, the public's attitude toward the risk of foreign involvement is effected during and after the period of fighting. (See for example, Holsti and Rosenau's [1984] analysis of the effects of Vietnam.) Furthermore, the greater the involvement in a "shooting" war, the longer will be the time period following the war in which the public will resist any further or additional involvement that might lead to U.S. casualties. It would be unwise for the president to consider undertakings with the risk of additional casualties, because of the lingering resistance to foreign involvement that follows such outbreaks of violence. Therefore, we expect that in periods following U.S. involvement in a

shooting war, the president's propensity to use force will be markedly reduced.

Condition of the domestic economy. On the domestic front, the public's attention is often preoccupied with economic prosperity (i.e., general economic conditions). The state of the economy has a major impact on both popular support levels (e.g., Ostrom and Simon, 1985), and electoral outcomes (e.g., Tufte, 1978). It is an essential concern of the president, at times one which will preoccupy his attention at the possible expense of foreign relations. However, there may be an important indirect relationship between the use of force and the economy. The more prosperous the economy, the higher the president's prestige and anticipation of electoral successes. The absence of prosperity has the opposite effect, and, in times of economic misery, may lead the president to look for strategies to deflect attention from the lack of economic success and to bolster his sagging image. Publicized deployment of the U.S. military represents one possible lever at the president's disposal; another may be the engineering of visits with international leaders. Therefore, although it may be debatable, we will argue that as the state of the economy worsens, the overall propensity of the president to use force will increase.

Decision Premises: The Political Environment

Any major use of force draws great public attention to the president as an individual, and the anticipation of such visibility and attendant public scrutiny will have an impact on presidential decision making.

Level of public support. In making almost any decision, including a foreign policy decision, the president must consider his personal resource base. Given the critical importance of popular support as a

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resource, it can be expected that the president will manage it with care. There are two basic outcomes to a use of force. If the action fails or the U.S. gets drawn into a much bigger and/or costlier event, the president stands to see his credibility, his popularity, and the support for his party in Congress undermined. If the action is "successful" in the eyes of the public, not even necessarily in objective terms—e.g., Kennedy's involvement in the 1961 Bay of Pigs fiasco—the president's resource base stands to be enhanced. Hence, it can be expected that, all other things being equal, the president will act when he perceives he can afford to lose or when he possesses a "popularity buffer." We are led to argue that the higher a president's current approval rating, the greater will be his propensity to use force. Presidents with relatively low levels of popular support, and in turn congressional support, will tend to become immobilized.

Overall political success. When making a decision on the use of force the president will consider his long-term "track record" as well as his immediate domestic support base. Neustadt (1980) argues that the best indicator of track record in terms of success, power, and prestige is revealed in the comparison between the president's initial level of popular support and his current popularity. If, during the course of his tenure in office, the president has been successful, his popularity will have remained relatively constant (or have risen). If the president has been less successful, his popularity will have declined. The relative level is important because it represents an easily accessible summary measure of the degree to which the president's performance has matched his perceived promise. Large drops in approval during the course of a president's tenure in office are taken as reflecting fundamental and broad-based dissatisfaction with the president's stewardship. During times when the president is experiencing little

overall success, it will be natural for him to pursue actions that will deflect attention away from failure. A "successful," highly visible use of force may be seen as a needed tonic. Consequently, we assume that a decline in presidential success will promote the acceptance of risk and a greater propensity to use force in the international arena.⁵

Position on the electoral calendar. Finally, it may be that the electoral calendar has an impact on presidential propensity to use force. During an election campaign period the president may well try to look "presidential." The anticipated favorable attention following a political use of force may be an incentive for the president to initiate such actions to boost his and/or his party's electoral chances. Also, in such circumstances a president is likely to downplay the risks of such involvement. This argument suggests that during quarters encompassing midterm and presidential elections, there may be more "strategic" behavior on the part of the president. Therefore, the propensity to use force will be greater during electorally important periods.⁶

In summary of our discussion of the decision premises considered by the president, we posit that the propensity of post-World War II presidents to use military force for political purposes is effected in the following ways.

By international contextual factors:

1. High levels of international tension between the superpowers increase the propensity to use force.
2. However, the greater the strategic dominance of the U.S., the less the likelihood that force would be used.
3. The deeper the involvement of the U.S. in a shooting war, the lower the propensity of the president to exercise force elsewhere in the international arena.

By domestic contextual factors:

4. During those times when the U.S. public is concerned about the level of international tension, the propensity to use force will be reduced.
5. During those times when the U.S. public is aware of the relative strategic dominance of the U.S., there will be a greater propensity for the president to use force.
6. In the period following involvement in a war, the propensity to use force will be affected negatively.
7. As the state of the domestic economy worsens, there will be an increased propensity to use force.

By personal and political contextual factors:

8. The higher the president's current approval rating, the more his propensity to use force will be expected to decrease.
9. However, as the overall success rating of the president declines, this propensity will be increased.
10. During national electoral campaigns, the propensity to use force will increase.

As can be seen, the effects of these decision premises considered separately are not mutually reinforcing. In virtually all instances, the president will experience crosspressures and internal conflicts in deciding whether or not to employ force. We are particularly interested, for example, in the possibility of discovering any differential effects on presidential decision making of international tension and of the relative strategic balance between the superpowers, depending on how the U.S. public views these factors and transfers its concerns to the president. For example, with high levels of international superpower tension the president's propensity to use force may be enhanced, but if the public expresses concern over the dangers of international involvement, this may

well contribute to a lessening of the president's initial positive reaction, highlighting our general argument that foreign policy decisions can and will be affected by considerations other than international affairs.

A Presidential Decision Rule

Having discussed the president's cognitive structure and the substance of his decision premises, it is necessary to turn our attention to the form and structure of his use of the major force decision rule. Simon (1979, p. 3) observes that

human powers are very modest when compared with the complexities of the environments in which human beings live. If computational powers were unlimited, a person would choose the course of action that would yield maximum utility under the given circumstances. . . . But real human beings . . . cannot follow this procedure. Faced with complexity and uncertainty, lacking the wits to optimize, they must be content to satisfice—to find "good enough" solutions to their problems and "good enough" courses of action.

The alternatives or possible responses have to be sought out and evaluated in order to make a decision. Search, furthermore, "takes place in a space that is essentially infinite" (Simon, 1979, p. 3). This, coupled with limited computational powers, compels human beings to limit their search by evaluating a small number of options and choosing the first one that is "good enough."

Thus, the first task in setting up a decision rule is to identify the choice set or set of alternatives that the decision maker seeks to evaluate. Following the logic of the cybernetic perspective, and thereby seeking to ease the burdens of calculation, we assume the president's choice set to be limited to two alternatives: (a) do not use major force and (b) use major force. Let Y_t represent the president's choice set, with $Y_t = 0$ for (a) and $Y_t = 1$ for (b).

In the current context we are evaluating the degree to which the president monitors, evaluates, and responds to "essen-

tial" aspects of the international, domestic, and political environments when deciding each quarter upon the political use of force. To accomplish these tasks the president simplifies his evaluation by developing a composite evaluation of the environment and determining the values of the composite index that are conducive insofar as the use of force is concerned.

In order to resolve the inherent conflicts among the 10 decision premises, we assume that the president, at each point in time, develops a composite index in which the evaluation of the decision premises is simplified in such a way that any set of environmental factors can be placed on a single scale of propensity to use force, Y_t^* .⁷ The larger the value on the composite scale, the more conducive the environment is to major uses of force. To make such a placement, and thereby integrate the evaluation of the 10 premises, the president must complete two tasks. First, the salience of each of the decision premises must be determined so that each is weighted in a manner consistent with its importance to the president. Second, once weighted, the premises must be combined to yield a single position on the composite scale. One very plausible method is the accumulation or addition of the individual weighted components. This may be formally represented by

$$Y_t^* = \sum_i a_i I_{it} + \sum_j b_j D_{jt} + \sum_k c_k P_{kt} + e_t, \quad (1)$$

where I_{it} , D_{jt} , and P_{kt} are the essential variables identified by the international, domestic, and personal decision premises, respectively; a_i , b_j , and c_k are the weights for the international, domestic, and personal variables, respectively; and e_t recognizes the fact that other factors will impinge on the decision in an unsystematic fashion.

To identify environmental states that

are conducive to and thereby complete the decision making process, it is our contention that there exists a threshold, h , which, from the standpoint of the president, represents the point at which the environmental evaluation moves from being unfavorable to favorable and vice versa. In other words, it is the *critical value on the propensity to use force scale*. A major use of force will be chosen ($Y_t = 1$) whenever

$$Y_t^* > h, \quad (2)$$

where h is some point on the composite environmental evaluation. Therefore, the threshold identifies the range of Y_t^* for which the environmental context is conducive to major uses of force. Since any value of Y_t^* greater than h leads to the same decision—that is, to use force—decision making is greatly simplified.

It should be clear that the results of the composite environmental evaluation are not observable. When this is coupled with the fact that the evaluation is affected by random shocks (represented by e_t), it is clear that we must model the presidential decision rule in a probabilistic fashion. Taken together, equations (1) and (2) suggest the probability that the president will decide to use major levels of force during quarter t can be characterized in the following manner:

$$\begin{aligned} Pr(Y_t = 1) &= Pr(Y_t^* > h) \\ &= Pr[(\sum_i a_i I_{it} + \sum_j b_j D_{jt} + \sum_k c_k P_{kt} + e_t) > h]. \end{aligned} \quad (3)$$

Several implications of this characterization of the cybernetic decision rule are noteworthy. First, the number of decision options each quarter is reduced to two alternatives. Second, presidents simplify their environmental evaluations by developing a single, composite index. Third, presidents use a threshold to determine whether major force is to be used; the environment is either conducive to a

major use of force or it is not. Fourth, the presidential decision rule can best be represented as a step function. A change in the value of Y^* , does not always lead to the same degree of change in Y_t . Only those changes that pass the threshold will lead to a change in behavior. All in all, equation (3) presents a probabilistic model of the president as a cybernetic decision maker.

An Operational Model of Presidential Decision Making

The Dependent Variable

The dependent variable, Y_t , is operationalized utilizing the data on the political uses of force presented by Blechman and Kaplan (1978). Note once more that we restrict our attention only to those occasions in which major force components or nuclear-capable U.S. forces were deployed (see Appendix).⁸ These major political uses of force are aggregated on a quarterly basis over the time period 1949–1976. Y_t is given a value of one if one or more political uses of major force occurs in a given quarter; it takes on the value of zero otherwise.

It is important to understand that our's is a *time series* model in which we seek to predict, for each quarterly period, the probability of a major use of force given the particular configuration of environmental factors at that time. Thus, we are not trying to model decisions to use major force in specific instances as responses to "opportunities" and/or in specific regions. This is an important and interesting question that to be answered requires quite different model constructions and data that are not currently available.

Independent Variables

The next task in developing the model is to operationalize the essential concerns in the president's use of force decision calculus. A single variable is defined for

each of the decision premises described earlier.

International Variables. The first international factor operationalized in the model is international tension, I_{1t} . This variable has been devised, utilizing the COPDAB monthly event data base, to reflect an overall level of tension in the bipolar international environment (Azar, 1982). The international tension measure is an index representing the difference between the total directed conflict and cooperation between the U.S. and the U.S.S.R. for each quarter divided by the total directed behavior of either a conflictual or a cooperative nature.⁹ Consequently, I_{1t} ranges from -1 (pure cooperation) to $+1$ (pure tension). The greater the level of international tension, the more likely the president is to engage in a political use of force.

The second international factor is strategic balance, I_{2t} , a variable designed to represent the relative strategic power of the U.S. with reference to its major opponent in the international system, the U.S.S.R. I_{2t} is an index ranging from -1 (total U.S.S.R. dominance) to $+1$ (total U.S. dominance), calculated by taking the difference between U.S. and U.S.S.R. strategic capabilities and dividing by the total capabilities of both actors. Data on strategic forces is taken from Squires (1982).¹⁰ It is posited that presidents are less likely to take risks when they have a strategic advantage—i.e., that the strategic balance (I_{2t}) and the propensity to use force will covary negatively.

The effects of the magnitude of any current U.S. involvement in major conflict are represented by the third measure, I_{3t} . The effects of war are viewed as cumulative, and thus the marginal impact of each additional death decreases as the number of deaths accumulates. For the years when the U.S. was participating in the Korean and Vietnam wars, the war variable, I_{3t} , takes on the value of the logarithm of the sum of battle deaths (Mueller, 1973). It is

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assumed that war (I_{3t}) will play an inhibitive role upon Y_t^* during the relevant time periods, because the president will be increasingly unwilling to risk further loss of life and/or material in other foreign policy actions.

Domestic Variables. The first and second domestic concerns operationalized in the model, D_{1t} and D_{2t} , are variables designed to reflect the degree to which the public is likely to sanction uses of force during those periods when it indicates attentiveness and concern over foreign policy issues facing the country. The overriding fact is that the American public has not frequently indicated in opinion surveys that it was concerned about high-risk foreign policy issues such as threat of war or nuclear war, relations with the Soviet Union, and resistance to communist expansion. However, when the public does identify high-risk foreign policy issues as most important, then the existing level of tension and the relative strategic balance are likely to be viewed as critical to the people and, in turn, to the president and his propensity to employ force. In operational terms, D_{1t} is equal to I_{1t} , and D_{2t} is equal to I_{2t} in those quarters in which high-risk foreign policy issues are identified by the public as the most salient; otherwise they are equal to zero.¹¹ Recall that we argued that during times in which high-risk foreign policy issues are dominant, international tensions will decrease the propensity to use force, but existing strategic balance will increase the propensity to use force.

The third domestic variable, aversion to war, D_{3t} , is designed to represent the lingering impact of war on the attitude of the public toward subsequent international involvement. It is our view that the lingering impact of war is a mirror image of the seriousness and longevity of the previous war. Therefore, we have operationalized the aversion to war as a mirror image of the war measure (i.e., $I_{3t-\Delta t} =$

$D_{3t+\Delta t}$), to represent the aversion to risk on the part of the public with a presumed negative impact on the president's propensity to use force.

The measure of economic well-being, the misery index, D_{4t} , is operationalized as the sum of the unemployment and inflation rates multiplied by the percentage of the U.S. public identifying the economy as the most important problem (Ostrom and Simon, 1985, pp. 342-43). This combined measure reflects the overall level of "misery" imposed on the public by the state of the economy weighted by the salience of the economy to the public. The weighted misery index posits that the impact of economic performance is dependent jointly upon the seriousness of economic problems and the number of people paying attention to the problems. In discussing the connection between the economy and the use of force, presidential attempts at deflection of attention from domestic economic conditions was stressed. Thus, it is hypothesized that the higher the misery index, the higher will be the propensity to use force.

Political Variables. A quarterly presidential approval variable, P_{1t} , has been determined by applying a set of coding rules to the record of answers to the question asked of the public in Gallup surveys: "Do you approve or disapprove of the job (name of president) is doing as president?" The coding rules were as follows:

1. If only one opinion poll was conducted during a given quarter, P_{1t} is equal to the approval percentage for that quarter.
2. If the question was not asked during a given quarter, P_{1t} is interpolated by averaging the approval percentage on both sides of the gap.
3. If the question was asked more than once during a quarter, P_{1t} is the average of all approval percentages during that quarter.

4. When the question was asked over two successive quarters, that approval percentage is used for both quarters.

It argued that the president will be more likely to use force when his level of approval is high.

The president's current "power situation" (e.g., Barber, 1977) is assumed to play a prominent role in the determination of whether to use force. Defined as the difference between his initial level of public approval upon taking office and his current level of approval, the relative power variable (P_{2t}) is designed to measure in relative terms the "success" of the president during his tenure in office in satisfying the expectations of the public. It is hypothesized that P_{2t} is positively related to the propensity to use force, keeping in mind that positive values of P_{2t} are indicative of drops in public approval. Thus, the argument is that the president will move to counter these declines with an increased likelihood of foreign action.

The electoral variable, P_{3t} , designates those quarters that are electorally prominent. It takes on the value of one during the third quarter of even-numbered years—the periods in which the midterm and general election campaigns are winding to a close. In our view, the propensity to use force will increase in these periods, as presidents seek to enhance their credibility as action-oriented leaders.

Model Evaluation

The Estimation Procedure

To evaluate empirically the cybernetic model depicted in equation (3), it is necessary to obtain estimates of a_i , b_j , c_k , and h . This is accomplished using an ordered probit model (Aldrich and Nelson, 1984; McKelvey and Zavoina, 1975). In the dichotomous case (i.e., two-element choice set), the threshold (h) is set to zero and the parameters are estimated using maximum likelihood. Consequently, the

estimated model predicts the president will use a major level of force during quarter t if Y_t^* is greater than zero; that is, when the environmental evaluation is "good enough." He will not use a major level of force during quarter t if Y_t^* is less than or equal to zero; that is, when the environmental evaluation is not "good enough." The probability [$Pr(Y_t=1)$] resulting from the model is an estimate of the conditional probability that a president will use force given the status of the international, national, and political contextual factors.¹²

The Estimated Cybernetic Model

Table 1 presents a number of summary measures of the overall performance of our cybernetic model of presidential decision making. As can be seen, the model performs very well. All of the estimated coefficients for the essential variables have the hypothesized sign, and 8 of the 10 coefficients are significant, in terms of a one-tailed test, at the .05 level. On average, our model's use of force predictions are correct for three of every four quarters during the 1948–1976 period. This is noteworthy when one considers that there is a 45–55 split between use and non-use. As such, the model represents a substantial increase in predictive success over the naive alternative model that predicts the most frequent category continually. Note that the overall measure of fit (i.e., $-2 \times LLR$) is statistically significant at the .001 level. There is substantial empirical support, therefore, for our general assertion that a model of decision making based upon contextual factors can account for U.S. political uses of major force aggregated on a quarterly basis.

Closer examination of the model reveals interesting results regarding the differential impact of international, domestic, and political factors. To assess the relative impact of the three sets of variables, an instrumental variable was

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Table 1. Parameter Estimates

Variable	Descriptor	Maximum Likelihood Estimate (MLE)	Standard Error (SE)	MLE/SE	Change in Decrease ^b	$Pr(Y_i=1)^a$ Increase ^c
I_{1t}	International tension	1.288	.590	2.184 ^d	-.203	.223
I_{2t}	Strategic balance	-1.301	.940	-1.384	-.185	.203
I_{3t}	Cumulative war dead	-.198	.110	-1.797 ^d	-.152	.159
D_{1t}	International tensions when foreign policy is primary public concern	-2.120	.863	-2.454 ^d	-.260	.293
D_{2t}	Strategic balance when foreign policy is primary public concern	1.374	.769	1.788 ^d	-.218	.241
D_{3t}	Aversion to war	-.202	.104	-1.940 ^d	-.145	.151
D_{4t}	Weighted economic misery index	.180	.093	1.925 ^d	-.206	.227
P_{1t}	Presidential approval	.087	.032	2.683 ^d	-.352	.417
P_{2t}	Overall presidential success	.058	.029	2.000 ^d	-.260	.290
P_{3t}	National elections	.491	.401	1.222	-.061	.064
Constant		-5.315	2.358	-2.254 ^d		

$N=112$; 75% correct; 55% correct—null; -2×33.28 , $p < .05$.

^aChange in $Pr(Y_i=1)$ given a one standard deviation change (both probability enhancing and probability decreasing) while all variables take on their mean value. When all variables are held at their mean, $Pr(Y_i=1) = .44$.

^bDecrease refers to change in probability brought on by a one standard deviation change in the variable in the direction that decreases the probability of using force.

^cIncrease refers to change in probability brought on by a one standard deviation change in the variable in the direction that increases the probability of using force.

^dSignificant at the .05 level.

created for each of the three types of variables, and the probability of use was predicted using the three instrumental variables.¹³ The resulting beta weights of .605, .671, and .856 for international, domestic, and political instruments respectively, provide a rough indication of the relative importance of the three sets of environmental factors. Even though the use of force decision is grounded in the state of the international environment, the impact of the international variables is far from dominating in the model. In fact, $Pr(Y_i=1)$ is also quite responsive to

changes in the domestic and political environments, with the political variables exerting the largest combined impact. This finding provides a foundation for greatly expanding the implications of the adjective *political* in the political use of force.

These general conclusions can be supplemented by a consideration of each of the individual coefficients. It is difficult to assess the impact of probit coefficients, since the estimation technique is non-linear. To provide one means of assessment we offer an impact range. Table 1

presents the maximum likelihood coefficients (MLE) along with the impact that a decrease or increase of one standard deviation in each variable has on the $Pr(Y_t=1)$, assuming that all other variables are currently at their mean.¹⁴ As such, the impact range provides an indication of the range of the effect of the variable in probability terms (i.e., how much change in $Pr(Y_t=1)$ results from changes in the environmental variables). For example, the impact range of P_{1t} is from $-.352$ to $+.417$, indicating that when all other variables are held at their mean, a one standard deviation drop in approval (13 approval points) from its mean (55) causes a drop in the predicted probability of major use from .44 to .09, while a one standard deviation rise in approval causes an increase in the predicted probability of major use from .44 to .85.

The index of U.S.-U.S.S.R. conflict and cooperation, I_{1t} , has a persistent impact on the predicted probability of the political use of force. Evaluating the coefficient in terms of a decrease or increase of one standard deviation, changes in I_{1t} can lead to a change of $\pm .20$ in the probability of using force (the probability of use would be .24 or .64). Based on the estimated model, higher levels of I_{1t} in the international system increase the probability of the use of force.

However, the second international variable, I_{2t} , has a statistically insignificant impact on the predicted probability of the political use of force. Therefore, contrary to our initial argument, there is no indication that in periods in which the U.S. has been dominant in terms of strategic forces, the president is more likely to use force.

The predicted probability of the use of major force for political purposes is affected in a negative fashion by the extent of current U.S. involvement in war. The third international variable, war casualties, has a statistically significant impact. Thus, in our model, as U.S.

battle deaths increase—that is, accumulate over the course of the hostility—the president is less and less likely to use the U.S. military elsewhere, even for so-called “political” purposes.

Our results contrast with Blechman and Kaplan (1978, p. 27), who suggest that international factors are most important in their analysis, and that current or recent involvement in war has a positive impact on the use of force. However, these authors were analyzing data including both minor and major uses of force—the former constituting about 150 incidents, many of them merely naval “sail bys.” It is very plausible that while the U.S. is involved in extended conflict, it will be willing to engage in such relatively “costless” and risk-free political uses of force, but at the same time be increasingly reluctant to deploy major force components. Also, note should be made of the difference in operationalization. Blechman and Kaplan, as well as Stoll (1984), employ a dichotomous dummy variable to indicate U.S. war activity on an annual basis. Our measure is sensitive to the duration and the increased cost and commitment of U.S. combat action as the war progresses.

The effects of the domestic contextual factors are very prominent in our results. The first domestic variable, D_{1t} , reflects, on a quarterly basis, whether the public is concerned over the level of tension between the superpowers. Based upon the estimated coefficient of -2.12 , during those quarters when the public is concerned about high-risk issues in foreign affairs, the probability of the political use of force is substantially dampened. Since the coefficient of D_{1t} represents the change in the coefficient of international tension, the impact of I_{1t} during times when the focus is on high-risk foreign policy issues is $-.83$ (i.e., $1.288 - 2.120$). Thus, in those quarters in which high-risk foreign issues are dominant, the positive impact of international tension is offset by

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public concern with war; in fact, during such periods the net effect is that tension is negatively related to the probability of a political use of force.

The second domestic variable, D_{2t} , relates the public's concern over foreign affairs to the level of the U.S.-U.S.S.R. strategic balance. Based upon an estimated coefficient of +1.374 for D_{1t} , I_{2t} has no impact ($-1.301 + 1.374$) during quarters when foreign policy issues represent an important American public concern. The relationship between strategic balance and the use of force disappears—that is, major political uses of force are not related to the strategic balance.

It is interesting to note the significance of the change in effects of the variables between I_{2t} and I_{1t} when the two variables are modified to reflect public attention and concern, yielding the variables D_{1t} and D_{2t} . The interactive effect of public concern completely changes the nature of the relationships found at the "international" level with regard to tension and the strategic balance. This provides one more piece of evidence to underscore the contention that the political use of force is a decision undertaken with reference to domestic as well as international contextual factors.

The third domestic variable, D_{3t} , indicates that immediate postwar periods are marked by substantially reduced propensities to use force. The similarity in the magnitude of the coefficients for I_{3t} and D_{3t} suggests that the termination of a war does not have an abrupt impact on the political use of force; instead, a significant inhibiting effect of the war experience lingers on for a considerable period of time. It also suggests that the inhibiting effects of past wars decline with the passage of time.

The final domestic variable, D_{4t} , economic misery, also proves important. The estimated coefficient of .18, which is statistically significant, indicates that the cumulative normal function increases by

one standard deviation for every five-point increase in the weighted misery index. When evaluated in terms of the impact of a standard deviation change, it is clear that a one standard deviation change (3.23 on the weighted misery index) leads to substantial changes ($\pm .20$) in the probability of using force. The magnitude and importance of this coefficient imply that the president is more prone to use force in times of economic stress.

Finally, turning to the effects of political leadership factors upon presidential decisions, the coefficient for P_{1t} suggests that the higher the level of presidential approval, the more likely the president will be concerned with the possibility that the use of force, if unsuccessful, could reduce his personal resources. The value of .09 indicates that the cumulative normal function will increase nine-tenths of a standard deviation for every 10 rating points in the polls. Note that a president's approval rating is the most important variable in the model from a statistical point of view.¹⁵ Holding all else equal, a one standard deviation change in approval (13 rating points) can lower or raise the probability of the use of force by .35 and .41 respectively. Since changes of 13 points or more in approval do occur frequently, a good deal of the impetus for the large swings in the propensity to use force can be traced to the presidential approval rating.

The estimated coefficient for overall presidential success, P_{2t} , indicates that each 10-point decline during a president's tenure in office increases the cumulative normal function by six-tenths of a standard deviation. It demonstrates, however, that the probability of use does not decline in the face of falling approval as rapidly as the coefficient of P_{1t} suggests. That is, the more negative the president's overall record in office (as represented by declining popular support), the more likely he is to act in the absence of a popular-

ity buffer. For example, taking the constant term and coefficients of approval and overall success from Table 1, we find that, *ceteris paribus*, $Pr(Y_t = 1) < .50$ when $P_{1t} < 43$, assuming that 70 is the high point of P_{1t} for the term and all other variables equal zero. If we take approval and overall success together while holding all other variables at zero, three "zones" relating to use are suggested: (1) when $P_{1t} > 58$, the probability of the use of force is greater than .67—hence the president has a buffer of popular support that will enable him to act; (2) when $P_{1t} < 43$, the probability of the use of force is less than .50, indicating that in the absence of other factors the president is unlikely to use force at all; and (3) when $58 > P_{1t} > 43$, the president may use force in anticipation of being able subsequently to regain some of his lost approval. All in all, the president's absolute and relative level of approval has a startling, very interesting, and significant impact on probability that he will use force in a political fashion.

The final variable in the model, P_{3t} , was designed to detect an electoral rhythm in the political use of force. Although the coefficient for this variable fails to achieve statistical significance, it is of substantive interest. According to the estimated version of our model, the cumulative normal score increases by .49 during each quarter prior to a national election, indicating a positive electoral cycle impact upon decisions involving the political use of major force.

As indicated earlier, it is difficult to compare our enterprise with Stoll's (1984) study. He focuses solely upon presidential reelection campaigns, finding some support for his hypothesis that during those reelection campaign periods occurring while the U.S. is also fighting a major war abroad there would be an increase in the certain uses of force for political purposes. Our model, on the other hand, is broader in scope, not only in its inclusion of a wider range of international and

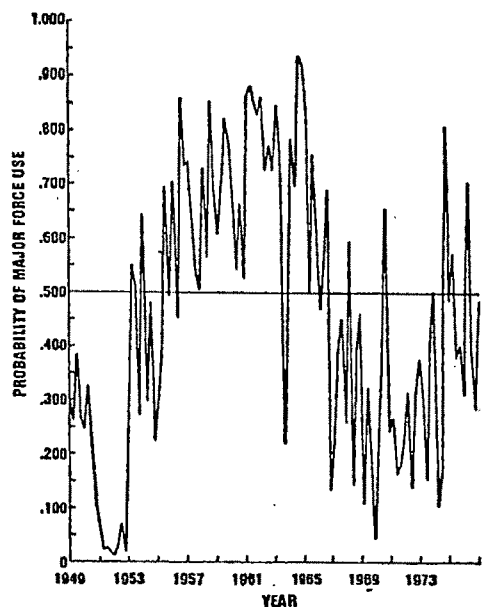
domestic factors, but also in its inclusion of all congressional election periods, and has quite different operationalizations for many key variables. On the whole, however, we would tend to suggest that the impact of electoral concerns would be quite subsidiary to the president's other, more influential, pressures and considerations.

Overall Explanation

Recall that the specification of the president's decision rule was based on locating environmental configurations conducive to the use of force. By implication, we expect a cybernetic decision maker to reach the same decision whenever the composite environmental evaluation remains below (or above) the threshold. This, in turn, leads us to expect extended periods, or eras, in which similar decisions persist.

Figure 1 presents the predicted values of

Figure 1. Propensity to Use Major Force



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$Pr(Y_t = 1)$ over the 1949-1976 period. Turning to a consideration of the ebb and flow of presidential decisions to use force, as reflected in the changing probabilities, we find there are apparently three distinct eras in U.S. decisions to use major force for political purposes. The first period runs from 1949 through mid-1953, and is marked by values of $Pr(Y_t = 1)$ far below .50. The dominance of the decision not to use force is due to low values of presidential approval, the Korean War, and the fact that the American public identified high-risk foreign policy issues as the most important problem facing the country during these years. The second period, which runs from mid-1953 through 1965, is dominated by values of $Pr(Y_t = 1)$ well above .50. Our explanation for the relative frequency with which force was predicted and used during this period is based on high presidential approval ratings and the absence of war, creating an environment in which the U.S. president could operate in a relatively unrestricted fashion in the foreign policy arena. The third period runs from 1966 to 1976 and is marked by very low values of $Pr(Y_t = 1)$. These predictions result from low values of approval, reduction in international tension, and the ongoing and residual effects of the Vietnam War. Thus, the cybernetic model provides a clear explanation for the extended periods over which presidential decisions with respect to the major use of force did not change.

The rather wide fluctuations in the predicted probability of using force in each quarter during the 1949-1976 period provide ample indication of the tremendous impact of the international, domestic, and political arenas on foreign policy decision making. In spite of the wide fluctuations, the observed behavior is predicted to remain quite stable. These results, when interpreted from a cybernetic perspective, provide support for the proposition that the president is predisposed to use force or not use force based upon the current state

of the environment. While the present analysis sheds no light on decision making in specific instances, it does provide a clear indication of the importance of contextual environmental factors on the likelihood that a president will use force each quarter.

Conclusions

The model presented in this paper produces a general, comprehensive explanation in which the political use of force by the U.S. is viewed as a presidential decision determined by a limited number of international, domestic, and political/personal factors. The cybernetic decision-making perspective and the model developed on this basis have credibility and deserve further consideration. Certainly, there is support for the proposition that the use of force is a presidential decision that resides in a decidedly political context. First, the decision is influenced by all three of the environments within which the president is assumed to operate. Second, the international variables are not the single most important contextual determinant of decisions on the use of force. Third, the effect of the international variables, when modified to account for domestic public perceptions, was dramatically altered. Fourth, political leadership factors appear to play a very prominent role in establishing the propensity to use force. In fact, the absolute and relative levels of popular support turn out to be the most important influence on the political use of major force.

Therefore, our cybernetic model of presidential decision making provides a substantively-based explanation for decisions to use force during the entire 28-year period spanning six presidents. If the time series displayed in Figure 1 is divided according to presidential terms, one can calculate the mean predicted probability value for the quarterly political use of force by each president. Doing so yields

Table 2. Analysis of Model Predictions According to Presidential Term

Presidential Term	Predict=1 Actual=0	Predict=0 Actual=0	Predict=1 Actual=1	Predict=0 Actual=1	% Correct
Truman	0	14	0	2	88
Eisenhower	6	6	17	3	72
Kennedy	3	1	7	1	67
Johnson	3	7	8	2	75
Nixon	0	15	2	5	77
Ford	1	5	2	2	70
Total	13	48	36	15	75

Note: Percentage of nonuses predicted correctly = 79%; percentage of uses predicted correctly = 71%.

quarterly predicted propensities of .15 for Truman, .58 for Eisenhower, .73 for Kennedy, .54 for Johnson, .26 for Nixon, and .48 for Ford. There are thus substantial variations among presidents. Kennedy, for instance, was seen to be likely to use major force for political purposes in three of four quarters; that is, Kennedy was five times as likely to make such decisions as Truman.

Table 2 provides information on the number of correct and incorrect predictions on a term-by-term basis for the presidents. The percentage predicted correctly across all presidencies is remarkably stable: about 75%. Two patterns in these predictions are worth noting. On one hand, the model appears to err by predicting a use of force when there was none more often in those quarters prior to 1964 than in those after 1964. On the other hand, the error of predicting no use of force when there actually was one occurs most frequently for those quarters after 1964. It should be emphasized again, however, that our model assumes common decision-making behavior for all presidents.

There are a number of features of the prediction errors made by our cybernetic model that merit special attention. Table 3 divides these incorrect predictions into two groups based upon the type of prediction error made by the model. The left-hand column lists those quarters in which

a use of force was predicted when there was no such use; this type of error occurred more frequently prior to 1964 and most often during the Eisenhower presidency. An in-depth analysis of presidential decision making during these quarters would provide some insight into the genesis of the decision against the use of force. There are at least two possible explanations for these mistakes. First, on the surface, it would appear that although tensions ran high and the U.S. maintained a superior strategic position throughout the Cold War of the 1950's, presidents were somewhat adverse to risk, and were not always willing to capitalize on these circumstances by using the military for political purposes. Second, it could be the case that there were no opportunities to use force in a political fashion.

The right hand column, in Table 3, lists the actual uses of force not predicted by the model. The events represented by these errors share three obvious characteristics. First, a majority were initiated by other international actors: The problems surrounding the assassination of Diem, the 1967 Arab-Israeli War, the Czechoslovakian invasion, the EC-121 being shot down, the Mayaguez incident, and the North Korean attack in 1976 were all unanticipated by the U.S. Second, a number of the missed predictions involve events surrounding the Korean and Vietnam wars. While in subsequent analyses

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Table 3. Listing of the Model's Incorrect Predictions, 1949-1976

Use of force predicted when none occurred			Actual use of force not predicted		
Quarter	Year	President	Quarter	Year	Event Number
			3	1950	30
			1	1951	31
1	1953	Eisenhower	2	1954	41
1	1956	Eisenhower	3	1954	42
4	1958	Eisenhower	2	1956	51
			4	1963	133
1	1960	Eisenhower			
2	1960	Eisenhower			
3	1960	Eisenhower	2	1967	174
4	1961	Kennedy			
1	1962	Kennedy	3	1968	179
			2	1969	182
3	1962	Kennedy	1	1971	192
4	1965	Johnson	2	1972	198
2	1966	Johnson	4	1972	199
3	1966	Johnson	1	1973	200, 201
1	1976	Ford	2	1975	218
			2	1976	225

most of these uses may be eliminated as components of the larger U.S. South Asian war, for this initial venture we performed a straightforward secondary analysis of the Blechman and Kaplan data set of nuclear-capable or major-level force incidents. Third, several "missed" predictions were events that became extremely important and very visible because of wide publicity and public attention. Consequently, whether or not the president "wished" to use force, based on his assessment of contextual factors as set out in the model, immediate short-term pressures

may have exerted overriding influences toward the use of force.

This raises two final points that must await examination in further research. One omission from the present analysis is a consideration of the "opportunity to use force." We chose to focus instead on the general context in which use of force decisions were made. However, having shown that the context is an important determinant of presidential decision making, in subsequent research we turn to the development of the concept of *opportunity* and an operational procedure for

the identification of such incidents. In conceptual terms, an *opportunity* is a set of circumstances in which the president considered using force of some kind to achieve U.S. interests. While we cannot know actual presidential considerations, we can assemble a set of events about which we can reasonably assume that the use of force was a considered option. Previous research suggests that factors such as a threat to U.S. security, endangerment of U.S. personnel and civilians abroad, a threat to a state with U.S. security commitments, a threat to a state in which the U.S. has interests, a threat to a state that receives U.S. security and/or economic assistance, a threat to U.S. economic interests, a threat posed by "communist" elements, or any one of a set of moves by a major opponent could be representative features of the class of opportunities to use force. In this regard, it will also be important to focus on the region in which the opportunity takes place. Construction and collection of an opportunity set will necessitate extensive research in presidential writings, govern-

ment documents, and in data archives. However, such efforts are necessary, because a decision-making model cannot be said to be applied satisfactorily if its only referents or applications are those incidents in which a positive decision was made to employ force. The research reported on in this paper provides a necessary first step towards this larger goal.

Second, the Blechman and Kaplan data set ends in 1976. Arguably the 1977-1984 period—which saw, among other incidents, the hostage rescue attempt in Iran, the Marines in Lebanon, and the invasion of Grenada—is a very interesting one. It is imperative that we obtain the data necessary to identify all of the major or nuclear-capable uses of force for this period. Once this data is available, it will be possible to test the predictive capacities of our model, as well as to determine whether the recent upsurge in uses of force in the Reagan administration is due to the peculiar nature of the president, or whether this propensity to use force is consistent with the ebb and flow of the post-World War II era.

Appendix: Nuclear-Capable or Major Uses of Force, 1949-1976

Event Number ^a	Force Level ^b	Quarter ^c	Year ^c	Description ^d
29	1	3	1950	Korean War: Security of Europe
30	3	3	1950	Political developments in Lebanon
31	3	1	1951	Security of Yugoslavia
38	2	3	1953	End of war in Korea
39	1	3	1953	Security of Japan/Korea
40	3	1	1954	France-Viet Minh War: Dienbienphu
41	1	2	1954	Guatemala accepts U.S.S.R. aid
42	3	3	1954	France-Viet Minh War: Dienbienphu
43	3	3	1954	British airliner shot down by China
44	1	3	1954	China-Taiwan: Tachen Islands
46	3	4	1954	Accord on Trieste
48	3	3	1955	Austrian State Treaty
51	3	2	1956	British General Glubb ousted in Jordan
52	3	3	1956	Egypt nationalizes Suez Canal
53	1	4	1956	Suez crisis
56	3	1	1957	Political-Military crisis: Indonesia
57	2	1	1957	Political-Military crisis: Jordan

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Appendix (continued)

Event Number ^a	Force Level ^b	Quarter ^c	Year ^c	Description ^d
61	3	3	1957	China-Taiwan conflict
62	2	3	1957	Political developments: Syria
63	3	4	1957	Indonesia-Netherlands crisis
65	3	1	1958	Political-Military crisis: Indonesia
66	2	2	1958	Political crisis: Lebanon
69	1	3	1958	Political crisis: Lebanon
70	1	3	1958	Political crisis: Jordan
71	1	3	1958	China-Taiwan: Quemoy-Matsu
75	2	1	1959	Security of Berlin
78	1	2	1959	Security of Berlin
79	3	3	1959	China-Taiwan conflict
80	3	3	1959	Civil war: Laos
82	3	4	1959	Political developments: Cuba
91	3	4	1960	Cuba supports insurgents: Guatemala and Nicaragua
96	2	1	1961	Civil war: Laos
99	2	2	1961	Trujillo assassinated
102	1	2	1961	Security of Berlin
103	3	3	1961	Security of Kuwait
110	3	2	1962	Civil war: Laos
114	1	3	1962	Cuban Missile Crisis
119	3	2	1963	Civil war: Yemen
121	3	2	1963	Withdraw missiles: Turkey
122	3	2	1963	Political crisis: Jordan
123	3	2	1963	Civil war: Laos
125	3	2	1963	Buddhist crisis in South Vietnam
133	3	4	1963	Assassination of Diem
136	3	1	1964	Security of Panama Canal
138	2	1	1964	Cyprus-Greece-Turkey crisis
139	3	1	1964	Coup in South Vietnam
143	2	2	1964	Civil war: Laos
149	3	3	1964	Cyprus-Greece-Turkey crisis
151	2	3	1964	North Vietnam fires on U.S. ship in Tonkin Bay
155	3	4	1964	Viet Cong attack Bien Hoa barracks
157	3	1	1965	Viet Cong attack Pleiku
158	3	1	1965	Viet Cong attack Qui Nhon
159	2	2	1965	Civil war: Dominican Republic
163	3	3	1965	War in Vietnam: Withdraw troops from Europe
164	3	3	1965	Political developments: Cyprus
166	3	3	1967	Civil war: Dominican Republic
174	3	2	1967	Arab-Israeli War
178	1	1	1968	Pueblo seized by North Korea
179	2	3	1968	Invasion of Czechoslovakia
182	2	2	1969	EC-121 shot down by North Korea
189	2	2	1970	Civil war: Jordan
192	3	1	1971	Withdraw troops from South Korea
198	2	2	1972	North Vietnamese offensive in South Vietnam
199	3	4	1972	Break down of peace talks: North Vietnam
200	3	1	1973	Civil war: Laos
201	3	1	1973	Civil war: Cambodia
205	1	4	1973	Arab-Israeli War
210	3	3	1974	Cyprus-Greece-Turkey crisis
216	2	3	1975	Collapse of regime in South Vietnam

Appendix (continued)

Event Number ^a	Force Level ^b	Quarter ^c	Year ^c	Description ^d
218 ^e	2	2	1975	Cambodia seizes Mayaguez
225 ^e	1	2	1976	North Koreans attack demilitarized zone

^aTaken from Blechman and Kaplan (1978).

^bTaken from Blechman and Kaplan (1978).

^cRefers to the quarter and year in which the U.S. became involved.

^dTaken from Blechman and Kaplan (1978).

^eThese numbers are taken from the ICPSR code book and are different from those reported in Blechman and Kaplan (1978).

Notes

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1. The already noted Blechman and Kaplan (1978) study is the first to catalog, categorize and study U.S. uses of force in a systematic fashion. Several authors, while employing the phrase "use of force" and constructing theoretical explanations that could apply more broadly, have focused their empirical efforts exclusively upon U.S. postwar direct military interventions (e.g., Pearson, 1974; Tillema, 1973; Tillema and van Wingen, 1982; Weede, 1978). George, in his work on "coercive diplomacy" with Hall and Simons (1971), in his work on deterrence with Smoke (1974), and in his own extensive work on presidential decision making (1980a and 1980b), has illuminated various questions and cases of relevance to this subject. Much has been written on crises (e.g., Holsti, 1972; Lewis, 1981; Snyder and Diesing, 1977, and on "noncrisis" decision making (e.g., Allison, 1971; Halperin, 1974; Steinbruner, 1974, but decisions to use force "fall between the cracks" of these studies. Krasner (1978) briefly considers the use of force by the U.S. government to advance commercial interests, but finds no empirical examples.

2. Blechman and Kaplan (1978, pp. 49-50) identify five levels of force. The three highest entail the use of a strategic nuclear unit and/or a major force component. A *major force component* consists of (a)

two or more aircraft carrier task groups, (b) more than one ground battalion, or (c) one or more combat wings.

3. We have chosen to restrict our attention to post-1948 uses of force (a) to focus on complete presidential terms, and (b) to avoid any contamination caused by immediate post-World War II foreign policy activities. In spite of the fact that many interesting events have taken place since 1976, our reliance on the Blechman and Kaplan data limits our analysis to pre-1977 uses of force.

4. Most certainly, cognitive processes and individual personality will have an impact on presidential decision making. Rather than assert that these factors are unimportant, we simply have chosen to look to the environmental connection first.

5. Insofar as the absolute level of support is concerned, the higher the president's popularity, the more likely it is he will have a popularity buffer that might increase his propensity to accept the risk of failure. With respect to overall success, it is our contention that declining *relative* success may lead the president to take some type of military action. Consequently, the negative impact of declining political success.

6. Note should be made of a recent article by Stoll (1984, p. 234) hypothesizing that if the U.S. is involved in or "close to" a war at the time of a reelection campaign, there will be an increased likelihood of "visible uses of military force." Our argument, therefore, runs counter to Stoll's.

7. We assume that this scale is continuous and ranges from $-\infty$ to $+\infty$.

8. We realize that questions can be raised about the exclusion of "nonmajor" uses of force. For example, the use of the Sixth Fleet in Cyprus in July 1974 is included, whereas the August 1967 display has been omitted. Whereas the former was coded as a "three," the latter was coded as a "four." Rather than raise questions about the restrictions, we have opted for a straightforward secondary analysis using the original Blechman and Kaplan codes.

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9. The measure is constructed as follows. We sum U.S.→U.S.S.R. and U.S.S.R.→U.S. directed conflict. From that we subtract U.S.→U.S.S.R. and U.S.S.R.→U.S. directed cooperation. The difference is divided by the sum of directed conflict and cooperation, creating an index which ranges from -1 to +1.

10. For the years prior to 1955, it is assumed that the U.S.S.R., with its limited airforce capabilities, posed no direct strategic threat to the U.S. (e.g., Kilmarx, 1962; Lee, 1961).

11. In statistical terms, the coefficients for D_{1t} and D_{2t} represent the change of the impact of tension and balance when the public is concerned about high-risk foreign policy issues. By including these interactive variables, the relationship between international tension and strategic balance on one hand and the probability of using force on the other is subject to change. The impetus for the change is public opinion.

12. There is a theoretical and methodological issue that deserves special attention before we proceed: the possibility of a simultaneous relationship between the $Pr(Y_t=1)$ and P_{1t} . There are three reasons that lead us to reject this possibility and hence to estimate equation (3) separately, without reference to an approval equation. First, it is our contention that the relationship between $Pr(Y_t=1)$ and P_{1t} is recursive; that is,

$$P_{1t} \rightarrow Pr(Y_t=1) \rightarrow P_{1t+1}.$$

Second, the impact of the relationship $Pr(Y_t=1) \rightarrow P_{1t+1}$ is mediated by other considerations. Previous research (Ostrom and Simon, 1985) suggests that the impact of dramatic international events is proportional to the extent of the media coverage given to the event. Thus, on an a priori basis, it seems clear that there will be no direct relationship between $Pr(Y_t=1)$ and P_{1t+1} . Finally, the time gradient is too coarse to allow us to discern a simultaneous relationship. It is not possible to tell empirically whether force leads approval or vice versa. At this juncture it seems safe to proceed with the estimation of the single equation.

13. To create the instruments required the following computation:

$$\begin{aligned} I_{iv} &= 1.228 \cdot I_{1t} - 1.301 \cdot I_{2t} - .198 \cdot I_{3t} \\ D_{iv} &= -2.120 \cdot D_{1t} + 1.374 \cdot D_{2t} - .202 \cdot D_{3t} \\ &\quad + .180 \cdot D_{4t} \\ P_{iv} &= .087 \cdot P_{1t} + .058 \cdot P_{2t} + .491 \cdot P_{3t}. \end{aligned}$$

When the $Pr(Y_t=1)$ is estimated using the three instruments, the maximum likelihood estimate (MLE) for each instrument is equal to 1.000. The standard deviation of the underlying scale (Y_t^*) and of each instrument were used to compute the estimated beta weights (McKelvey and Zavoina, 1975, p. 115).

14. Using the estimated coefficients and the mean values for each of the independent variables, we determined that the probability of using major force is .44. While holding all variables at their mean value, we calculated the change in the probability of use that results from each variable evidencing both a plus and minus one standard deviation change from its mean. The two columns in Table 1 represent the probability enhancement and decrement brought on by these changes.

15. This finding is confirmed by Blechman and Kaplan's (1978, p. 27) result that, in bivariate terms, approval was most highly correlated with the use of force.

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CANDIDATES AND POLICY IN UNITED STATES SENATE ELECTIONS

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This analysis demonstrates that policy issues play an important role in the selection of members of Congress. We differ with the conclusion of much of the existing research on congressional elections, which indicates that policy considerations are of minor importance. We have conducted an analysis of the 1982 U.S. Senate elections, drawing on data from the CBS News/New York Times 1982 congressional poll and from 23 statewide exit polls. We demonstrate that (1) candidates behave as though they believe issues are important to voters; (2) candidates' policy positions systematically influence voters' decisions; and (3) candidates' issue positions and voters' evaluations of the president and the economy interact to provide clear patterns of policy effects on Senate election outcomes. Policy effects are substantial and systematic in Senate elections, and cannot be omitted if we are to appreciate the importance of congressional elections in the national policy-making process.

Research on congressional elections has grown substantially, although unevenly, in the last decade. The several strands of research on congressional elections do not currently add up to a full picture of the interactions among candidates, voters, and national issues in House and Senate contests. We have learned a great deal about incumbency and the impact of economics in congressional elections, especially House elections, but little about the impact of policy generally, and even less about Senate elections in particular.

We approach legislative elections with the assumption that their major function is the selection of alternative sets of policy makers. This perspective implies the need to consider and explain the issue stances of candidates as well as voters' choices, and to consider more broadly the role of policy in congressional elections. Previous work that takes this view suggests policy plays a part in congressional elec-

tions. We know, for example, that opposing candidates for House and Senate seats usually represent substantial policy alternatives (Backstrom, 1977; Erikson and Wright, 1985; Sullivan and O'Connor, 1972), and that who wins makes a difference in the types of policies enacted (Brady and Lynn, 1973; Brady and Sinclair, 1984; Wright, 1986). We also know from aggregate-level cross-sectional studies that there is considerable correspondence between the general liberalism or conservatism of House incumbent issue positions and various measures of district ideology (Erikson, 1971; Erikson and Wright, 1980, 1985; Schwarz and Fenmore, 1977). In aggregate time-series studies, researchers find that economic conditions and evaluations of the president explain changes in the two-party House and Senate vote from one election to the next (Hibbing and Alford, 1982; Kramer, 1971; Tufte, 1978). The evidence for policy congruence between represen-

tatives and constituents, and the pattern of voters "punishing" the presidential party for poor economic or presidential performance argue that elections do provide a policy linkage between voters and Congress.

In spite of these studies, the predominant message in the recent burst of congressional elections research is that policy factors are not important to voters. One reason for this conclusion is a matter of emphasis. Most of the recent attention to candidates has been on their service, contacting, and advertising activities rather than on issues (Abramowitz, 1980; Hinckley, 1980a; Mann and Wolfinger, 1980; Parker, 1980; and many others). This is consistent with the research agenda set by the mystery of the "vanishing marginals" and the apparent increased advantages of incumbency in House contests (Mayhew, 1974; cf. Jacobson, 1985). However, this collective concern with incumbency has been a major factor shaping the common wisdom that policy considerations are rather unimportant in congressional elections.

This conclusion is reinforced by analyses of the information voters have about candidates. Issues and ideology do not loom large in the electorate's images of Senate and House candidates. Most of what voters have to say about candidates concerns service, integrity, experience, and other nonpolicy attributes (Abramowitz, 1980; Hinckley, 1980a; Mann, 1978; Mann and Wolfinger, 1980). Policy thus plays a distinctly secondary role in explaining individual candidate choice. In competing against other aspects of candidate image or even party identification, policy issues are not high on the list of predictors of individual votes. But does this mean that policy plays an insignificant role in the overall electoral process? We argue it does not. The purpose of this paper is to develop and test a model of the interactions of candidate issue strategies and voter decision making. We assess the

model at the level of candidates and at the level of voters. We then show how candidate strategies combine with economic and presidential evaluations to shape the pattern of election outcomes.

We look, in this paper, at Senate elections. Abramowitz (1980) and Hinckley (1980b) both argue that voters know more about Senate than House candidates. Since the challenger is better known, more experienced, and national issues are more important in Senate than in House elections, senatorial contests are a logical first place to look for the policy linkages between candidate issue strategies and presidential performance and voter decisions. We discuss later how House elections might compare to the findings reported here.

Theoretical Perspective: The Strategic Problems of Candidates and Voters

To consider the linkage between members' behavior and the process by which they are selected, we develop an approach that focuses on the policy strategies of candidates and voters. We begin by thinking about the goals and constraints working on candidates and on voters in senatorial elections. Our approach is narrower than others, in that we purposely ignore some important factors: we are not concerned here with candidate personalities, with voter psychology, with service to the state or lack thereof, with media usage and coverage, or with campaign spending. Our approach is broader, however, because we try to see policy as part of the larger electoral process—encompassing the perspectives of candidates and voters—and to see how these combine to shape the outcomes of U.S. Senate elections.

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Candidate Strategic Problems

In the context of the general election, we assume that the overriding short-term goal of all senatorial candidates is to get elected (or reelected). Here we do not need to assume that reelection is a candidate's only goal, only that it be seen clearly as a necessary condition to accomplishing other policy or power goals (Dodd, 1977; Wittman, 1983). Convergence toward the median voter appears to be the rational vote-maximizing issue strategy under a broad array of conditions (Downs, 1957; Hinich and Ordeshook, 1970; Hinich, Ledyard, and Ordeshook, 1973). Certainly this is the case for one-dimensional issue spaces, and the recent congresses do appear to be described adequately by a single underlying liberal-conservative dimension (Poole, 1981; Poole and Daniels, 1985; Schneider, 1979; Shaffer, 1980). While convergence is a rational strategy for the general election, candidates do not in fact converge, but usually offer distinct choices to the electorate (Erikson and Wright, 1985; Poole and Rosenthal, 1984; Sullivan and O'Connor, 1972; Wright, 1978a).

One reason for the "failure" of the Downsian model is that the expectation of convergence ignores the impact of primary elections on candidate issue strategies (Wright, 1978b). The first hurdle for any candidate is to win the primary, and it is the winners of the primaries who face the larger citizenry in the general election. If voters behave sincerely rather than strategically, in the sense of voting for their most preferred candidate in the primaries, then the two parties are frequently going to offer candidates that diverge, not converge, on the ideological spectrum. The reason for divergence is the clear ideological distinctiveness of the activists and primary voters in the two parties, and perhaps the candidates' own policy preferences (Wittman, 1983). The likely winner of most

Democratic primaries is going to be more liberal than is optimal for the general election, and the likely winner of most Republican primaries will be more conservative than is optimal for victory in the general election.

Aranson and Ordeshook (1972) and Coleman (1972) arrive at this conclusion on the effects of primaries from a formal perspective. On the empirical side, we have used both national surveys and state-level exit poll data to compare the ideological profiles of primary and general election voters (Wright, 1978b; Wright and Berkman, 1985). The results are as one would expect: voters in Democratic primaries are more liberal and voters in Republican primaries are much more conservative than are general election voters. We can add to this primary election factor the polarizing effects of party activists and financial supporters. Few activists or "amateurs" are moderate. Those who are well-informed and active in politics tend to have coherent belief systems (Converse, 1964), and thus cluster at the liberal and conservative poles of the ideological continuum. This is important for candidates initiating a campaign, who must begin by contending with sets of potential contributors and supporters who have little taste for the lackluster politics of moderation. Candidates thus face strong forces for ideological divergence at the same time that they would like (if only for purposes of getting votes) to appear moderate to the general electorate. The significance of the primary election and core ideological supporters is that candidates cannot easily shift positions toward the median state voter in the short-run without risking both alienation of those core supporters and charges from the opposition of being two-faced and inconsistent.

For incumbents the available evidence indicates that the adjustment in issue stances occurs two or three years before the election (Elling, 1982; Fenno, 1982);

Hibbing, 1984; Thomas, 1985). The general pattern is one of moderation in roll call voting as the general election approaches. The incumbent has an interesting strategic problem; too much movement to the middle early on could invite stronger challenges in the primary, whereas unmoderated ideological purity would offer a more tempting target for potential general election opponents. And the incumbent has to commit to an overall issue stance—by a record of roll call if nothing else—before knowing much about the actual level and intensity of the primary or general election competition that he or she will face.

The situation of the challenger is somewhat different. He or she is likely to be fresh from a contested primary and not to have the visibility and the resources for raising money that come with incumbency. While challengers would, we expect, profit from policy moderation, they may be less able to wean themselves, ideologically and financially, from their primary electorates.

Voter Strategic Problems

Elections are often seen as a means to "send them a message." The voter has the problem of how to send a precise message with just a simple X on a piece of paper or just the pull of a lever in the voting booth. In the end, the vote simply sends one candidate or the other to an office. However, there are several things voters can reasonably wish to accomplish with their ballots, and dealing with these sometimes conflicting goals gives rise to the nature of the voter's strategic problem.

We assume that most of what voters want to accomplish in senatorial voting can be summarized by three objectives. First, voters would like to elect the "best" representative, in the sense of one who is an effective policy maker, who shows the capacity for leadership, accomplishes things for the state, and generally has the personal characteristics and qualifications

the voter associates with being a good senator. Second, citizens are assumed to want policy representation, in the sense of having a senator who votes and works on issues in ways that are generally consistent with the voter's own preferences on the issues of the day. Third, congressional elections provide the opportunity to register sentiment about the president, his performance in office, and his handling of the economy.

Voters may frequently find that they are unable to satisfy all these goals with the same vote. A conservative who is unhappy with Reagan's economic policy may find herself confronted with a liberal incumbent with good committee assignments who is an effective advocate for her state's interests. How to vote? Obviously putting primary weight on the first and third goals would push toward a Democratic ballot, while attention to the second goal pushes her in the direction of a GOP ballot. Voters may often face such problems in deciding what "message" to send.

Our analysis of senatorial voting takes advantage of the voters' plight to explore the way in which they respond to candidates' strategic behavior. The logic we impute to voters is straightforward: they should use the information that is relevant and easily available to achieve the three goals. For example, if nothing is known about the leadership abilities of the candidates, this goal gets less weight; if it is known that a candidate shares one's issue positions and the other candidate generally embraces the flip side on the issues, one votes for the favored candidate; and, if the candidates are seen to disagree on the president's program and his performance, then this race may be an appropriate forum for a referendum vote.

The approach here focuses on the interaction of candidates and voters. Because of the differences in the distribution of issue preferences between primary and general election voters, candidates face one set of strategic problems, while voters

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must decide how to weight various factors that might be relevant to their decisions in meeting competing goals. We use this interaction to explore the role of policy in Senate elections.

Data Sources and Measures

Our approach has data requirements that differ from most studies of voting in congressional elections. We need objective measures of candidate issue stances in Senate contests, as well as statewide samples of voters in these races. Both sources of data used here are from the CBS News/*New York Times* (NYT) data collections from the 1982 U.S. Senate elections. Candidate issue information comes from their 1982 congressional poll. In that data set, all candidates and members of Congress were sent a questionnaire tapping a variety of issue- and campaign-related items. Here we deal only with the Senate data, and in particular with scale scores derived from responses to 10 issues. The 10 items scale nicely into a single liberal-conservative dimension. Responses are summed to form a measure of policy conservatism running from -5 to $+5$.¹ Thus, we have for all 1982 U.S. Senators and candidates an overall measure of policy conservatism. Since responses were made during the campaign and to a major news organization, we take these responses to represent the candidates' and senators' public stands on the issues at the time of the election.

For the voters' side of the analysis, we draw on the CBS/NYT statewide election day exit polls. These data are different from the familiar National Election Studies (NES) surveys. Whereas the NES are personal interviews, getting in-depth information about a smaller number of voters sometime after the election, the CBS/NYT exit polls obtain much less information, but immediately after the election and from many more voters.

Each type of data set is appropriate for different sets of questions. For our purposes, it is crucial to have large numbers of cases that are representative of the electorates of individual states. Since our theory of voter responsiveness focuses on candidate strategy as much as on voter characteristics, we require less information on voter backgrounds, perceptions, and attitudes. We trade off in-depth analysis of individual voters for the breadth afforded by the statewide CBS/NYT exit polls. This offers a better opportunity to assess the interactions of candidates and their electorates.

The full data set includes statewide samples for 23 of the 33 Senate races in 1982. The overall sample of these respondents is 34,516, with state samples ranging from 652 in West Virginia to over 3,000 in California. Table 1 lists all the states with 1982 Senate elections. It shows the candidates running, the *Ns* for the states for which we have poll data, and the percentage of the two-party vote received by the Republican candidate.

Mark Westlye (1983) has argued that the NES-based samples of voters in Senate contests are biased toward overrepresentation of "hard fought" elections that occur in the larger states. The argument that the larger states have more competitive Senate contests is born out by Hibbing and Brandes (1983). As a quick check against unwittingly reporting similar biases, we see that the mean winners' vote for the 23 states in our sample is 57.4%, while that for the full 33 states with Senate elections in 1982 was 59.2%. The omitted states are somewhat less competitive than those in the CBS/NYT data collection, but this is not so severe as to distort the findings offered here.

Candidate Issue Strategy

We have argued that candidates are torn between their primary and general election constituencies, but would like to

**Table 1. 1982 Senate Elections and the CBS News-
New York Times Exit Poll Surveys**

State	Democratic Candidate	Republican Candidate	Number of Respondents	Republican Share of Two-Party Vote (%)
Arizona	DeConcini	Dunn	No Poll	41
California	Brown	Wilson	3,057	53
Connecticut	Moffett	Weicker	2,038	52
Delaware	Levinson	Roth	No Poll	56
Florida	Chiles	Poole	No Poll	38
Hawaii	Matsunaga	Brown	No Poll	18
Indiana	Fithian	Lugar	No Poll	54
Maine	Mitchell	Emery	1,532	39
Maryland	Sarbanes	Hogan	No Poll	37
Massachusetts	Kennedy	Shamie	2,938	39
Michigan	Riegle	Ruppe	864	42
Minnesota	Dayton	Durneburger	1,201	53
Mississippi	Stennis	Barbour	975	36
Missouri	Woods	Danforth	2,063	51
Montana	Melcher	Williams	1,391	43
Nebraska	Zorinsky	Keck	1,194	30
Nevada	Cannon	Hecht	1,158	51
New Jersey	Lautenberger	Fenwick	2,073	48
New Mexico	Bingaman	Schmitt	1,161	46
New York	Moynihan	Sullivan	2,411	35
North Dakota	Burdick	Knorr	No Poll	35
Ohio	Metzenbaum	Pfeifer	985	42
Pennsylvania	Wecht	Heinz	No Poll	60
Rhode Island	Michaelson	Chafee	1,286	51
Tennessee	Sasser	Beard	850	38
Texas	Bentsen	Collins	2,044	41
Utah	Wilson	Hatch	1,547	59
Vermont	Guest	Stafford	1,092	52
Virginia	Davis	Trible	954	51
Washington	Jackson	Jewett	No Poll	26
West Virginia	Byrd	Benedict	652	31
Wisconsin	Proxmire	McCullum	No Poll	35
Wyoming	McDaniel	Wallop	893	57

appear moderate in the general election. Let us now examine the policy choices offered by the 1982 senatorial candidates. Table 2 shows the mean policy conservatism scores for six groups of senators and candidates. Mean scores are shown by party for incumbents not running, for incumbents running in 1982, and for open-seat candidates and challengers. Comparisons either between parties or among types of candidates within a party indicate that incumbents running are

more moderate than both their counterparts who are not under electoral pressure and their nonincumbent partisan colleagues. This difference is apparent for candidates in both parties, but more the case for Republicans.

Evidently, only incumbents seem to be playing the strategy of moderation. Challengers and open-seat contestants run much more as good ideological representatives of their parties than as seekers of middle-of-the-road votes. For example,

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Table 2. Mean Policy Conservatism for Senatorial Incumbents and Candidates in 1982

Party	Incumbents Not Running	Incumbents Running	Challengers and Open-Seat Candidates
Republicans	3.5 (26)	1.0 (19)	3.6 (14)
Democrats	-1.4 (41)	-0.4 (11)	-2.4 (22)
Difference	4.9	1.4	6.0

Source: Computed from the 1982 CBS News/*New York Times* Congressional Poll.

Note: Scale runs from -5 to +5, with higher scores indicating policy conservatism. The number of candidates or senators in each category is shown in parentheses.

the ideological difference between the GOP and Democratic incumbents running for reelection is just 1.4, compared with the ideological distance between nonelection senators of 4.9, or the even greater distance between nonincumbents of 6.0.

These data suggest elections with relatively extreme challengers contesting more moderate incumbents. However, no consideration has been given to the ideologies of the districts, so any inferences remain tenuous. Candidates are not running from random states, but from states with distinct political cultures and traditions. We need to control for the effects of state preferences before concluding that senators moderate their issue stances in response to electoral pressures. That is, Table 2 shows that election year incumbents are more moderate than nonelection year incumbents, but are they more moderate relative to their districts?

To check this, we estimate a more complete model of senator/candidate issue positions. We assume here the workings of party activists and the primary system, so there is an overall party effect. While allowing for an overall party effect, we also acknowledge heterogeneity within the parties, and at least some of this is likely to reflect state voter preferences. Thus, within parties, there should be some relationship between senator/candidate ideology and state ideology and, perhaps, partisanship. Here we use

the measures of state ideology and partisanship developed from aggregating at the state level 53 national polls taken from 1974 through 1982 (Wright, Erikson, and McIver, 1985).

If there is strategic moderation by senators, their expressed election year issue positions ought to be more moderate than those of nonelection year incumbents of the same party once state political preferences are controlled. To summarize our model of candidate issue strategy: We posit a party effect (Republicans are more conservative than Democrats), a state ideology effect (more liberal states should run more liberal candidates within both parties), and then we look for the election year pressure effects (incumbents and perhaps challengers of both parties should be more moderate than the senators of their respective parties who are not up for reelection). We also include a region effect for southern Democrats.

To assess this set of expectations we estimate the following model:

$$\begin{aligned}
 C = & a + b_1(1-P)YI + b_2(1-P)(1-I) \\
 & + b_3P + b_4PYI + b_5P(1-I) \\
 & + b_6(1-P)S_{11} + b_7S_i + b_8S_p \\
 & + e,
 \end{aligned} \tag{1}$$

where

C = senator/candidate issue conservatism (-5 to +5);

Table 3. Regression of Senator or Candidate Conservatism on State Partisanship and Ideology, by Election Year and Incumbency

Variable	Coefficient	t-ratio
Democratic incumbent, not running (a)	-5.16	-9.5
Democratic incumbent, running vs. Democratic incumbent, not running (b_1)	1.45	2.4
Democratic challenger vs. Democratic incumbent, not running (b_2)	-.25	-.4
Republican incumbent, not running vs. Democratic incumbent, not running (b_3)	4.89	8.3
Republican incumbent, running vs. Republican incumbent, not running (b_4)	-1.78	-2.9
Republican challenger vs. Republican incumbent, not running (b_5)	.94	1.8
Southern democratic challenger vs. non-southern Democratic incumbent, not running (b_6)	1.77	2.4
State ideology (b_7)	14.31	5.8
State partisanship (b_8)	-1.85	-1.1
$R^2 = .66 \quad \bar{R}^2 = .64 \quad N = 128^a$		

Source: For senator or candidate policy conservatism, see the CBS News/*New York Times* 1982 Congressional Poll. For state ideology and partisanship, see Wright, Erikson, and McIver (1985).

Note: The first six coefficients represent differences in policy conservatism among types of candidates. Read the value of b_1 , for example, as Democratic incumbents running for reelection were 1.45 points more conservative than Democratic incumbents not running, with the effects of state ideology and partisanship controlled.

^aThe N here is five less than in Table 2, because we do not have ideology and partisanship measures for Alaska and Hawaii.

P = party: 1 for Republican, 0 for Democrat;
 Y = election year: 1 for seat up in 1982, 0 otherwise;
 I = incumbency: 1 for incumbent, 0 for nonincumbent;
 S_{11} = region: 1 for the 11 states of the Confederacy, 0 otherwise;
 S_i = mean state ideology;
 S_p = mean state partisanship; and
 e = random error term.

The equation is set up this way to highlight the relationships of primary interest: strategic moderation and party differences. Specifically, if there is electoral pressure toward moderation in the general election, we expect Democrats running for reelection to be more moderate (i.e., conservative) than nonelection year Democratic incumbents (b_1 will be positive), while incumbent Republicans running will be more moderate (i.e., liberal) than nonelection year Republican senators (b_4 will be negative). Similarly, if challengers can moderate, b_2 will be posi-

tive (Democrats) and b_5 will be negative (Republicans). However, Table 2 suggests that nonincumbents are not moderate, so these coefficients could be about zero, or even have signs opposite those suggested. The overall effects of the party system, including primaries and ideologically distinct partisan supporters, can be seen in the magnitude of the party variable (b_3 should be positive). The results are shown in Table 3.

The general message from Table 3 is roughly the same as that from Table 2, where state partisanship and ideology are not controlled, but with some interesting additional information provided. Both Democratic and Republican incumbents running for reelection express more moderate issue positions than their counterparts not under election pressure. This is just a tendency, a shift toward the middle, not a case of leopards changing their spots. Interestingly, their challengers show no moderation relative to their districts at all. The signs for Democratic and Republican challengers indicate that, if

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anything, nonincumbents are more ideologically polarized than either set of incumbents. This, of course, is consistent with the simple mean scores in Table 2.

In addition, we see an interesting combination of party and district influences on candidate policy conservatism. The effects of party membership are positive and large. Controlling for the other factors, Republican senators who are not running are 4.9 points more conservative than their Democratic counterparts. Our model adds district ideology here, and it shows a substantial influence on candidate ideology: liberals of both parties tend to come from more liberal states. While state ideology is important, its effects here are less than the overall effects of candidate party affiliation: the standardized regression coefficient for state ideology is .34, while that for candidate party affiliation is .59. (Standardized coefficients for interaction terms do not have their usual straightforward interpretation, and hence are not presented.)

The dummy variable for southern Democratic candidates and incumbents is significant; they are more conservative than predicted on the basis of state partisanship and ideology. When this variable is omitted, the coefficient for state partisanship is significant and negative, which makes no sense. Thus, there is a regional effect not completely captured by our measures of state ideology and partisanship. The southern states produce by far the most conservative candidates (Wright, 1986).

The major message from our analysis of candidate policy strategies can now be summarized. First, there is generally a policy choice between candidates for the Senate, although this is muted by candidates' apparent efforts to moderate their issue positions for the campaign. The tendency toward moderation relative to the district holds only for incumbents; their challengers are purer ideologues. The reason is likely to be that challengers are

generally fresh from primary victories, and hence still tightly tied to their primary electorates and core ideological supporters. Incumbents, in contrast, have had six years to position themselves. One interpretation is that senators use the first years of their terms to build credit with their attentive party activists and core ideological supporters, and then they moderate later in their terms to increase their general election appeal. The resources that come with incumbency seem to enable some moderation by senators while they hold off significant primary challenges. This should put them at an advantage to the extent that general election voters rely on general policy liberalism-conservatism in their decision.

Voter Responsiveness to Candidates

Do individual candidates and the policy directions they represent matter to voters, or is policy information about candidates so irrelevant, ambiguous, or even nonexistent in senatorial contests that what candidates stand for does not count in voting? Do candidate stances matter more under some circumstances than others? The CBS/NYT data allow us to examine some, but certainly not all, of the relationships of interest under the broad topic of policy voting. In particular, the large *N* of these polls and the questions asked permit us to assess how the policy positions of candidates influence voters. Our interest here is not in the relative effects of variables on voting (e.g., party versus issues); it is in the ways candidates influence levels of policy voting. While we believe it is reasonable, our definition of policy voting here is constrained by the items available in the CBS/NYT exit polls. It is the propensity of voters to use general policy liberalism-conservatism and evaluations of the president and his handling of the economy in their senatorial voting decisions.

Voter Ideology

The role of ideology—that is, liberalism or conservatism—is said to have increased over time in presidential elections, and in several pseudo-elections issues have been found to vary in importance depending on the candidates voters are asked to choose between (Nie, Verba, and Petrocik, 1976, ch. 17). In House elections voters' issue liberalism was more closely related to the vote as the ideological distance between House members increased (Wright, 1978b), and the same is true for the Senate (Abramowitz, 1981). Based on these studies, and on the general knowledge gleaned from recent research on Senate contests, we expect that voters do have enough information about candidates to employ decision-making aids like partisan and ideological identifications selectively.

The basic idea here is simple, but the consequences are important. The idea is that voters will use their ideological identifications more when there is a clear ideological choice between candidates, and not use it when the candidates do not offer such a choice. This idea implies that the electorate has the ability to make reasonably fine distinctions. Whereas issues are said not to count to a significant degree in many subpresidential election studies, this hypothesis indicates that not only do they count, but that the degree to which they count is a function of the political system; here they matter to the extent that candidates take clear ideological positions. The logic here is similar to that of Hibbing and Alford (1981), who found economic conditions to have clearer effects in House elections in which an incumbent of the president's party was running. However, our formulation requires voters to respond to candidate issue positions rather than just to the party of the incumbent.

The model to test this proposition is estimated using simple product terms

between the respondents' ideological identification and the candidates' policy conservatism, while including the obvious variables of party identification and incumbency to avoid major misspecification:

$$V = a + b_1P + b_2R_I + b_3D_I + b_4C_R + b_5C_D + b_6I + b_7IC_R + b_8IC_D + e \quad (2)$$

where

- V = senatorial vote: 1 if for a Republican, 0 if for a Democrat;
- P = party identification: -1 for Democrats, 0 for independents, 1 for Republicans;
- R_I = Republican incumbency: 1 for voters in elections with Republican incumbents, 0 otherwise;
- D_I = Democratic incumbency: 1 for Democratic incumbent elections, 0 otherwise;
- C_R = Republican candidate policy conservatism;
- C_D = Democratic candidate policy conservatism;
- I = ideology: -1 for liberals, 0 for moderates, 1 for conservatives; and
- e = random error term.

Our hypothesis is that the slope for voter ideology increases as the candidates polarize—that is, the Democrat gets more liberal and the Republican gets more conservative. Our attention, therefore, focuses on the last three terms of the equation. The multiplicative variables are made interpretable by rearranging terms to determine the slope for ideology (I) (Friedrich, 1982). That is,

$$b_6I + b_7IC_R + b_8IC_D = (b_6 + b_7C_R + b_8C_D)I. \quad (3)$$

The effect of voter ideology varies with the issue positions of candidates. For example, if both candidates are moderate,

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Table 4. Probit Estimates of the Senatorial Vote on Selected Variables and the Interaction of Respondents' and Candidates' Policy Conservatism

Senate Vote with:	Probit Estimates	
	Maximum Likelihood Estimate (MLE)	MLE/Standard error
Intercept (a)	0.064	2.77
Party identification (b_1)	0.805	80.33
Incumbency: Republican (b_2)	0.007	0.30
Incumbency: Democratic (b_3)	-0.387	-16.10
Republican candidate ideology (b_4)	-0.005	-1.29
Democratic candidate ideology (b_5)	-0.003	-0.71
Respondent ideology (b_6)	0.162	7.16
Respondent's ideology \times Republican candidate's ideology (b_7)	0.054	11.38
Respondent's ideology \times Democratic candidate's ideology (b_8)	-0.038	-6.94
$N = 33,200$		Estimated $R^2 = .394$

C_D and C_R would be scored zero, the last two terms would drop out, and the slope for voter ideology would equal b_6 . Now let the Democrat stay put and the Republican run as an extreme conservative. C_R would now have a value of +5, so the slope for voter ideology would equal $b_6 + b_7(5)$.

We expect the impact of ideology to increase with the conservatism of the Republican candidate ($b_7 > 0$) and with the liberalism of the Democratic candidate ($b_8 < 0$). Table 4 shows the model estimated using probit analysis. The dichotomous dependent variable makes significance tests from OLS regression invalid (Aldrich and Nelson, 1984; Hanushek and Jackson, 1977, ch. 7). Probit is clearly more appropriate, but the probit estimates are less straightforward in their interpretation; they are the expected change in the cumulative normal probability distribution that results from unit changes in the independent variables. The last column of Table 4 shows the probit maximum likelihood estimates (MLEs) divided by their standard errors. These may be interpreted as the t -test in regression.

Our hypothesis is supported nicely. The signs of the "main effects" for ideology, as well as the interactions of voter ideology with candidate ideology, are in the correct direction and clearly significant. As the candidates become more polarized, the effects of voter ideology increase. This can be seen more clearly if the probit estimates are translated into probabilities. The effects of an independent variable in probit analysis changes with the values of the other independent variables. Thus, we show the effects of candidate polarization and voter ideology for each of the categories of our other independent variables, party identification and incumbency. Interestingly, incumbency did not help Republicans in 1982 (see Table 4), so no separate estimates are shown for those races.

Table 5 shows two sets of probabilities. Those on the left are estimated effects of voter ideology when both incumbents are centrists. Those on the right are our estimates for races with completely polarized candidates—a liberal Democrat versus a conservative Republican. Comparing the two columns of numbers shows the effect of candidate polarization on voters' use of

Table 5. Probabilities of a Republican Senatorial Vote, by Candidate Polarization

Incumbent's Party ^a	Voters' Party Identification	<i>Effect of Voter Ideology in Races with:</i>	
		Moderate Democrat vs. Moderate Republican	Liberal Democrat vs. Conservative Republican
		Difference Between Conservatives and Liberals ^b	Difference Between Conservatives and Liberals ^b
Democratic	Democratic	.068	.264
Open	Democratic	.098	.364
Democratic	Independent	.122	.444
Open	Independent	.128	.465
Democratic	Republican	.114	.423
Open	Republican	.089	.337

^aRepublican incumbents had no net advantage in the 1982 Senate elections; estimated probabilities by party identification are virtually the same as those for open-seat contests.

^bThe effect of voter ideology here is the difference between self-identified liberals and conservatives in their estimated probabilities of voting Republican. In the first row, for example, in a contest with a Democratic incumbent running for reelection where both candidates are moderates, the probabilities of a GOP vote for conservative and liberal Democrats were .167 and .099, respectively. The difference, .068, is the estimated effect of ideology.

ideology in Senate elections. The impact is marked: in the most competitive situation of independent voters casting ballots in open seat races, there is only a 13% difference between liberals and conservatives in support for the Republican. In fully polarized contests this difference increases to over 46%. Candidate polarization in effect would more than triple the weight voters give to ideological cues. The effect is less in other types of races, but in each case voter ideology is much more important when the candidates differ on the issues.

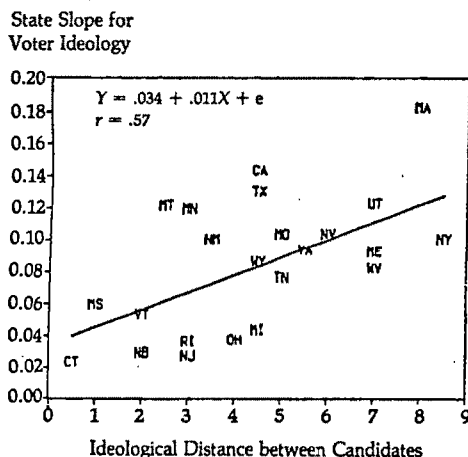
The CBS/NYT data permit us to look at candidate polarization and ideological voting at the state as well as the individual level. We begin with 23 separate state regressions of candidate choice on voter ideology. In these we control for party identification, for whether anyone in the respondent's household had been unemployed in the last year, and for the respondent's assessment of his or her finances compared with the last year. Figure 1 shows the relationship between the ideology slopes (which are monotonically related to the corresponding probit

estimates) and candidate polarization. The pattern of state outcomes is unmistakable. As candidates differ on the issues, voters give greater weight to ideological cues in voting. This also demonstrates that the individual results shown presented above are not due to any artifactual hocus-pocus involved with our interpretation of the interaction terms. Rather, ideological choice produces ideological voters and ideological outcomes.

Before accepting the causal linkage between candidate strategy and voting, we should consider an alternative explanation. If some states have political cultures more disposed to ideological politics than others, then we might observe a correlation between candidate polarization and ideological voting even if voters were not responding to specific candidates. From this perspective, candidate polarization measures an aspect of political culture rather than the specific stimuli affecting voters. This implies that states with larger candidate distances would respond more ideologically to any reasonable political stimulus, because an ideological culture is causing both can-

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Figure 1. The Importance of Voter Ideology in the 1982 U.S. Senate Elections, by Candidate Polarization



didate positioning and voting decisions. To assess this we substitute approval of Reagan for Senate vote in the equations above. Approval of Reagan is an ideologically relevant stimulus, and we find that it is strongly related to voter ideology. However, this relationship between approval of the president and ideology does not vary systematically with senatorial candidate polarization. In several tests, the weight given to voter ideology in assessments of Reagan does not vary with issue differences between Senate candidates. This gives us considerable confidence in the causal linkage between candidate strategy and voter calculus.

Evaluations of the Economy and the President

In the literature on congressional elections there is a sharp difference between findings obtained from analysts looking at aggregate time-series data and those looking at cross-sectional survey data (Kramer, 1983). The former consistently find large effects for the economy and presidential popularity. From the

aggregate level, midterm elections appear policy-oriented with a distinctly national and presidential cast to the policies that are relevant (Hibbing and Alford, 1982; Kramer, 1971; Tufte, 1978). However, in individual-level studies voters seem to give little weight to their personal economic circumstances, but they do vote according to how they feel the presidential administration is doing with the economy overall (Kiewiet, 1981; Kinder and Kiewiet, 1979; Kuklinski and West, 1981; Weatherford, 1983).

A question that remains largely unaddressed concerns the ways voters decide whether to “punish” candidates for an unfavorable economy or unpopular president. Most of the national level time-series studies seem to imply that candidates of the president’s party are stand-ins for the administration. They get blamed—or, less often, rewarded—based on sharing party affiliation with the president, regardless of their behavior. There is a nice responsible-parties-doctrine feel to this perspective, but it does not fit well with our images of congressmen that are increasingly independent of party and presidential administrations.

A dyadic perspective of policy representation, which sees senators serving as delegates representing state rather than party interests, would argue that candidates act—and should be judged by voters—independently of the administration. If voters see their senators as *their* senators, rather than primarily as members of a party that supports or opposes the president, then citizen attitudes toward the economy and the president may not have much influence in senatorial elections. The ability of voters to differentiate among candidates' characteristics in weighting decision factors is suggested by interoffice differences in economic effects. National economic issues seem to be less important as we move toward more localized elections, where incumbency and contact effects are well known. The econ-

omy seems to have a larger impact on presidential than on congressional elections (Kiewiet, 1981), and within congressional elections economic effects are larger for incumbents of the president's party (Hibbing and Alford, 1981) and in senatorial races (Hibbing and Alford, 1982).

From these considerations, we hypothesize that the referendum model of midterm elections applies to the extent that the candidates running disagree on the president's macroeconomic policy and general performance in office. If voters are even modestly sophisticated, they are more likely to use their evaluations of the president in senatorial decisions when they have a liberal Democrat criticizing a president like Reagan and a conservative Republican telling them to "stay the course." The president is less relevant to the senatorial voting decision if the candidates take similar positions on the major issues and vis-a-vis the presidential program.

To assess the differential impact of the economy and presidential evaluations across senatorial elections, we examine three measures included in the CBS/NYT 1982 exit polls. Respondents were asked (1) about the effects Reaganomics had on their state, (2) whether they thought Reagan's program would eventually help or hurt the economy, and (3) about their approval of Reagan's handling of his job as president.² Each of these questions is explicit in mentioning the president. The first is retrospective and uses the voter's state as a focus. Evaluations of Reagan on this question were the lowest of the three measures: over 60% of those answering thought the president's economic program had hurt their state. The second item is prospective and pertains to the national economy. It was clearly not a simple projection from past behavior; over 55% thought that the president's economic program would eventually help the economy. Voters were evenly split on the third

item, their approval or disapproval of the job Reagan was doing as president.

Reagan's clear conservative stance during his first two years as president means that conservative candidates are most likely to be identified with him and his program, and moderates and liberals less so. We expect, therefore, that the weight voters place on presidential evaluations in their Senate voting will vary directly with the differences between candidates.

Here the methodology is the same as that used in the previous section, but now we anticipate presidential evaluation effects to be a function of candidate ideological differences. Below are the equations we estimate. The variables and coefficients for b_1 through b_6 are the same as in the ideology analysis. We add the measures of attitude toward Reagan and his handling of the economy and we use these in the cross-product terms with candidate ideology:

$$V = a + b_1P + b_2R_i + b_3D_i + b_4C_R + b_5C_D + b_6I + b_7X_i + b_8X_iC_R + b_9X_iC_D + e, \quad (4)$$

where V and the first six right-hand side variables and their coefficients are the same as in the previous equation; and X_i equals one of three presidential/economic evaluation items discussed above. Each is scored -1 for an unfavorable evaluation of the president, 0 for a neutral or "don't know" evaluation, and 1 for a favorable evaluation.

In analyzing these economic/presidential evaluation items, we largely sidestep the self-interest versus sociotropic and the retrospective versus prospective voting debates (Fiorina, 1981; Kinder and Kiewiet, 1979; Kramer, 1983; Kuklinski and West, 1981; Sears, Lau, Tyler, and Allen, 1980). These controversies are not directly relevant to our purposes.³ We are interested in the selectivity with which voters reward and punish senatorial candidates for presidential management of

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Table 6. Probit Estimates of Senate Vote on Selected Variables with Economic and Presidential Evaluations, by Candidate Ideology

Senate Vote with:	Reagan Effect on State	Reagan Program Help or Hurt	Job Approval
Intercept (a)	0.102	-0.041 ^a	0.010 ^a
Party identification (b_1)	0.631	0.584	0.560
Incumbency: Republican (b_2)	0.033 ^a	-0.008 ^a	-0.007 ^a
Incumbency: Democrat (b_3)	-0.371	-0.381	-0.382
Republican candidate ideology (b_4)	-0.020	-0.017	-0.013
Democratic candidate ideology (b_5)	-0.022	-0.008	-0.011
Respondent ideology (b_6)	0.230	0.224	0.204
Economic and presidential evaluations (b_7)	0.406	0.434	0.410
Evaluations \times Republican candidate's ideology (b_8)	0.033	0.032	0.037
Evaluations \times Democratic candidate's ideology (b_9)	-0.039	-0.031	-0.036
$N^b =$	27,354	33,200	33,200
Estimated R^2	.480	.493	.488

^aCoefficient is less than twice its standard error.

^bRespondents in Massachusetts, Texas, and Vermont were not asked whether the Reagan economic program had helped or hurt their state.

the economy and the country. Thus, from the equation above, we anticipate that evaluations of Reagan will count for more as the Republican candidate becomes more conservative and as the Democratic candidate becomes more liberal. Support for this is found if $b_8 > 0$ and $b_9 < 0$.

The results for the three Reagan and Reaganomics items are shown in Table 6. The effects of evaluations of the president and his handling of the economy are substantial. Each of the Reaganomics items has a probit coefficient (b_7) of between .40 and .43, where both candidates are moderates. The signs are in the right direction for the effects of both Republican and Democratic candidate ideologies; and the interaction terms' coefficients are in the right direction and significant. The responsiveness of voters in using Reaganomics can be seen if we translate the predicted z-scores into probabilities. Let us consider how attitudes toward Reagan would influence an otherwise undecided voter—which, under probit, is where all variables will have their

maximum impact. For such a voter, in an election with two moderates the difference between Reagan supporters and detractors varies between 31 and 34 percentage points in voting for the Republican candidate, depending on which item is used. And this effect increases with candidate polarization. The effects of the presidential and economic performance items are estimated to increase to between 55% and 56% for the undecided voters in elections in which a completely liberal Democrat would face an equally conservative Republican. Clearly, in the 1982 elections reactions to Reagan and the economy were important, and this importance increased among voters confronting a larger policy choice between candidates.

In this section we have found convincing evidence that voters are responsive to the issue strategies candidates pursue. As the candidates offer a clearer ideological choice, voters put more weight on their own ideological identifications and on their evaluations of the economy and presidential performance. Next we look at

how these dynamics combine at the aggregate level to influence the outcomes of Senate elections.

Candidate and Economic Effects on the Outcome

The political significance of elections, for candidates, is not in whether voters base their choices on ideology and the president, but in how the candidate's own behavior affects the election outcome. We thus shift our focus and ask about the influence of candidate issue strategy on Senate election outcomes. In the process we see a significant difference in the importance of variables at the individual versus the aggregate level.

We begin by referring to Table 6 and the coefficients for the issue liberalism-conservatism of candidates. Evaluated at an expected probability of .5 (and assuming neutral attitudes toward Reagan), the largest probit coefficient is $-.022$. Translated into probabilities, this means a one-unit change in candidate liberalism-conservatism would change the probability of voting for the Republican by just .008—less than a 1% effect. Values of this magnitude would appear to yield the effects of candidate ideology as trivial. Indeed, were it not for the unusually large sample provided by the CBS/NYT exit polls, this effect would probably be lost. It would certainly not be statistically significant with the typical sample size of 1,500 or so. Such a conclusion of insignificance, however, would be a serious mistake.

To see this, let us compare voters in two hypothetical elections. We will hold all variables constant except candidate ideology. The first election has a liberal Democrat running against a moderate Republican with policy conservatism scores of -5 and 0 , respectively. We can compare this to an election where a moderate Democrat opposes a conservative Republican, with policy conservatism

scores of 0 and 5 , respectively. Using the probit estimates in the first column of Table 6, we find a voter that otherwise had a .5 probability of voting Republican would have this rise to .544 in the contest with a liberal Democrat, and drop to .460 in the contest with a conservative Republican.⁴ Thus, we get a difference due to candidate strategy of .084 (i.e., $.544 - .460$) between the two elections. Using the coefficients from the second and third columns of figures in Table 6 yields smaller, but still important, estimates of .050 and .048. Aggregated, this means the candidate strategy differences in our two elections would produce a shift of between 5% and 8% in the vote among those most undecided, who, of course, are the crucial voters in a close election.

Given the general competitiveness of Senate contexts, this is not a small effect. We can compare this to the effects of a familiar variable such as party identification to appreciate more fully the electoral impact of candidate issue positions. At the individual level, there is no doubt that party is a more important determinant of the vote. But at the state election level, this is less obvious. To see this, we need only consider how many voters' partisan identifications we would need to change in our example to achieve a 5% shift in the vote. A 5% shift in the vote at 50% is represented by a probit change of .125. How much partisanship change is needed to bring this about? The answer is the change in the partisanship mean multiplied by the probit MLE coefficient for party identification. We use the coefficient for party identification from Table 4, (.805).⁵ We would need a change in mean partisanship of .155 ($.125 = .805 \times .155$) to achieve an expected increase or decrease of 5% of the vote. Given the way party identification is scored, (-1 , 0 , $+1$), this means that over 15% of the electorate would have to change their partisanship in a uniform direction, say from Democrat to Independent and from

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Independent to Republican. The entire literature on the decline of party identification that has occurred over the last 30 years rests on changes that are no larger than this 15%. We can see that the electoral significance of candidate ideology is much larger than is implied by its small individual-level coefficients.

The reason for this is an important distinction for electoral analysts. Some variables, such as party identification, are good at explaining individual voter behavior, but do not change much over time, and are not responsive to individual candidates. Thus, while they can predict individual behavior, they do not help in explaining variation from one election to the next in the same constituency. Aspects of the campaign, like candidate issue positions, may have only slight effects on most voters, and because of this can easily be overlooked altogether with our traditional design and research objectives of accounting for individual voting decisions. However, a small effect, working in the same direction across a state or district, can sum to a substantial collective weight. Our point is that candidate issue positions may be much more significant when the votes are counted than when we are trying to predict individual decisions.

Having made the point that candidate strategy can have significant effects on election outcomes, let us now assess how Reaganomics played out in the 1982 elections. In doing so we see how the strategies of candidates and voters combine with the effects of macroeconomic policy to establish a clear pattern of outcomes.

There are two aspects involved in determining the effects of presidential and economic evaluations on senatorial voting. The first is the level of mean evaluation among any group of voters, here state electorates. The second is the weight given to that evaluation in voting. These can be combined to obtain an overall estimate of the net effects on voting of any set of appropriately scored attitudinal ele-

ments (Achen, 1982; Stokes, 1966). We measure the net Reaganomics effects on a state's vote for Senator by summing the effects for the three Reagan variables considered above.⁶

The attitude and its weight are likely in this instance to be influenced by different factors. The weight is, we have argued, a function of candidate strategy. The more the candidates differ the greater should be the weight given to evaluations of Reagan and his handling of the economy. Attitudes toward Reagan, however, are likely to be a function of the voter's experience. Across states the clearest factor that would lead to different evaluations was the differential impact of the Reagan economic program. The major national issue in the 1982 elections was the economy, particularly unemployment (Clymer, 1982), and the costs of the Reagan recession were not borne equally among the states. Parts of Reagan's 1980 Sunbelt coalitions suffered less than the industrial central and northeast states of the Rustbelt. Thus, we expect the state unemployment rate to influence attitudes toward Reagan (it does but we do not show the data here), and candidate polarization to affect how much voters rely on these evaluations in casting ballots for senator.

To simplify the presentation we divide the candidate polarization variable in half (above or below a candidate issue difference of 4.5) and present the data for these "high" and "low" polarized races. The data are shown in Figure 2. The relationship between state unemployment and the impact of Reaganomics is clear in the polarized races (Figure 2-A). Attitudes toward the president had an effect when the candidates differed more on the issues, and the direction of this effect varied with state unemployment. Republican candidates lost votes where the employment rate was higher, and gained votes from evaluations of Reagan and the economy where unemployment was lower. But

Figure 2. Net Effects of Attitudes Toward Reagan and the Economy on the Senatorial Vote, by Candidate Polarization

Figure 2-B. Contests with Low Candidate Polarization

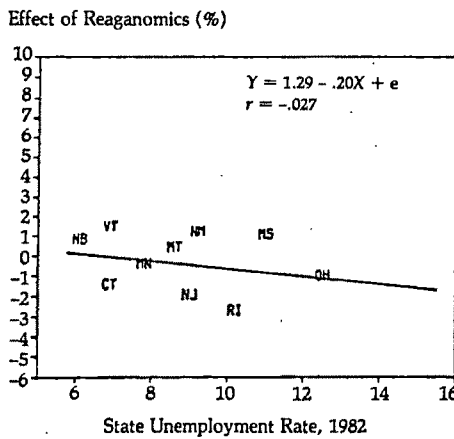
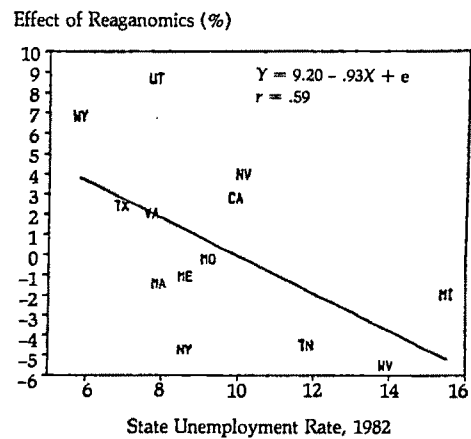


Figure 2-A. Contests with High Candidate Polarization



where the candidates were similar in their policy stances, attitudes toward the president had little effect on the election outcome (Figure 2-B). This can be seen in the nature of the scatterplots and in the steepness of the fitted regression lines: there is clearly a stronger relationship between Reagan's effect on the vote and unemployment in the polarized states.

There is an interesting lesson in these results. The effect of major national issues, such as the president's handling of the economy, may contribute to the outcome of a Senate election, but this is not just an across-the-board national judgment. For evaluations of the president to help the Democrat, the candidate needed higher unemployment and a Republican opponent who was clearly to the right. Republicans profiting from the Reagan phenomena needed lower rates of unemployment and Democrats considerate enough to stand out as a liberal alternative. The results here should caution us in generalizing about Senate elections as national referenda on the one hand or

personality run-offs on the other; they can be both or a combination, and which depends to a great degree on the policy choice candidates present to their electorates.

Discussion and Conclusion

The voters in Senate elections are sensitive to the choices they are presented. Our findings suggest a calculating and somewhat sophisticated electorate that differs from the portrait presented in numerous studies of House voters. Concerning policy, it may be that there are tremendously large office effects relevant in Senate but not in House contests. However, it may also be that we have simply missed the effects of policy voting in House elections, because its predictive power is a good deal less than that of incumbency, candidate visibility, advertising, and the like. We believe this is the case (Erikson and Wright, 1980, 1985). However, direct comparisons seem to indicate that because candidates are more

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visible in Senate elections, policy plays a larger role there than in the House (Abramowitz, 1980; Kuklinski and West, 1981; Weatherford, 1983). Unfortunately, most of the Senate-House comparisons use NES data, which in recent years have samples that undermine such analyses (Eubank, 1985; Jacobson, 1980; Westlye, 1983).

For some time, scholars have recognized the need to go beyond generalizations about the "voter in presidential elections" or the "voter in congressional elections." Through the National Election Studies, major strides have been made in seeing and accounting for the importance of incumbency in House contests. It is probably not an overstatement to say that there cannot be any serious analysis of House elections without consideration of incumbency, any more than studies of individual voting can omit party identification. These are among the basic elements needed to describe the electoral process.

Our hope is that this paper will enhance congressional studies in a similar way by reintroducing the importance of policy. The availability of the CBS/NYT congressional poll and state voter exit polls has permitted us to explore a different set of questions than is the norm in congressional elections studies. In developing these, our point of departure is that although Congress is foremost a policy-making institution, most of our research has little to say about any policy linkage between elections and Congress. Our goal in this paper has been to identify and explore the policy dimension in Senate contests. Having access to the congressional poll data allows us to consider systematically candidates' issue positions. Analysis of these data demonstrated something careful congressional observers frequently report: candidates believe and act as though their issue stances matter to voters (Dodd, 1986; Fenno, 1982; Kingdon, 1981).

Accounting for candidates' issue stances, and hence the nature of the choices elections provide, requires appreciation of the crucial role of the primary system and activist supporters. Together these help to produce choices rather than echoes in congressional elections. We also find, contrary to the prevailing view, that voters act as though they are policy oriented. We see this at the individual level in the differences in the salience of ideology and presidential evaluations according to the nature of the electoral choices offered, and we find it at the aggregate level, where candidates' issue positions influence the outcomes of Senate races. We find a pattern of reward and punishment of candidates of the president's party according to how the state fared economically, but this pattern emerges only when there are clear differences between the candidates' stands on issues. There is, then, a strong connection between senators as policy makers and the electoral process by which they attain and keep that office. Policy is important for candidates, for voters, and for Senate election outcomes. It should also become important for those of us who study them.

Notes

An earlier version of this paper was presented at the 1985 annual meeting of the Western Political Science Association, Las Vegas, NV. This research was supported in part by a grant from the National Science Foundation, grant SES 83-10443. The candidate and exit poll data used in this analysis were collected by CBS News and *The New York Times*. The exit poll data were obtained from the Inter-University Consortium for Political and Social Research. We are grateful to Kathleen Frankovic and Warren Mitofsky for making the 1982 congressional poll data available, and to D. Christine Barbour for her assistance. We appreciate also the comments and suggestions offered by David Brady, Vincent Buck, Richard Fenno, Robert Huckfeldt, Richard Stoll, Rick Wilson, and Leroy Rieselbach. The authors are solely responsible for the interpretations and analysis.

1. Candidates and incumbents were asked if they favor or oppose constitutional amendments to (1)

allow individual states to prohibit abortions, (2) permit organized prayer in the public schools, and (3) require a balanced budget. In addition, they were asked about positions on (4) the Equal Rights Amendment, (5) a mutual nuclear freeze with the Soviets, (6) domestic content legislation for foreign cars sold in the U.S., (7) cancelling the July 1983 tax cut, (8) cutting back increases in military spending, (9) additional reductions in domestic social programs, and (10) regulation of air pollution. Using regular factor analysis with varimax rotation we see two factors emerge, although the second is very weak, with an eigenvalue of just 1.008. Basing the factor analysis on the preferable tetrachoric correlations yields a single factor with loadings ranging from .6 to .95. Items included in the CBS/NYT congressional poll that do not scale on the liberalism-conservatism dimension are positions on solving the social security funding problem and on whether arms sales ought to be used to pressure Israel to negotiate with the Palestinians.

2. The wording of the questions was (1) "Has the Reagan economic program: helped [state name], hurt [state name] or had no effect?"; (2) "Will Reagan's economic program eventually: help the economy, or, hurt the economy?"; (3) "Do you approve or disapprove of the way Ronald Reagan is handling his job as President?"

3. It strikes us that no one but a microeconomist would believe that citizens act in politics primarily in terms of narrowly defined economic self-interest. In any case, the self-interest/sociotropic distinction breaks down at the group level. What is a person called who is doing well personally, thinks Reagan's program will eventually help the nation, but votes against the Republican candidate because Reagan's program has unfairly hurt the voter's state?

4. The effect of candidate ideology in the first election is simply $-.022(-5) + .020(0) = .11$. From a table with the values for the standard normal distribution we find the probability of .0438, which is added to the initial .5 for .544. Where the Democrat is moderate and the Republican conservative we get $-.022(0) - .020(5) = -.1$, and subtract the area under the normal curve at that value (.0398) from .5 to get our probability of .460.

5. The equations that produce the coefficients in Table 6 control for the presidential evaluation items, and these could not reasonably be expected to remain constant if party identification changed. Using the larger coefficient lowers our estimate of the amount of change needed in partisanship to obtain any given shift in the vote.

6. These are calculated from individual state regressions of the vote on party ideology, unemployment, personal finances, and the three Reaganomics variables. The net effect of Reaganomics in each election is then the sum of the products of the individual state slopes and the state means of these variables.

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Edward G. Carmines and James A. Stimson. "On the Structure and Sequence of Issue Evolution"

James Cassing, Timothy J. McKeown, and Jack Ochs. "The Political Economy of the Tariff Cycle"

Henry W. Chappell, Jr. and William R. Keech. "Political Motivation and Party Differences: A Dynamic Spatial Model of Party Competition"

Mary Dietz. "Trapping the Prince: Machiavelli and the Politics of Deception"

Bernard Grofman. "The Possibility of Faithfully Representative Committees"

Rhoda E. Howard and Jack Donnelly.

"Human Dignity, Human Rights, and Political Regimes"

Peter McDonough, Samuel H. Barnes, and Antonio López Pina. "The Growth of Democratic Legitimacy in Spain"

Anthony Mughan. "Toward a Political Explanation of Government Vote Losses in Midterm By-Elections"

Charles W. Ostrom and Robin F. Marra. "United States Defense Spending and the Soviet Estimate"

Karen Rasler. "War, Accommodation, and the Process of Violence"

Harold W. Stanley, William T. Bianco, and Richard Niemi. "Partisanship and Group Support over Time: A Multivariate Analysis"

Ulmer, S. Sidney. "Are Social Background Models Time-Bound?"

CONTROVERSY

INTERPRETING THE 1974 CONGRESSIONAL ELECTION

What factors best account for the 1974 congressional election results? Were the Democratic party gains in House seats due mainly to the behavior of "strategic politicians" who sought or accepted candidacies and raised effective campaign war chests? Or were the Democratic gains due more to the impact of Watergate and the economy on voters' choices? In this Controversy, Gary C. Jacobson and Samuel Kernell defend the "strategic politicians" thesis, while Eric Uslaner and Margaret Conway stand by their more conventional accounting of the 1974 election outcome.

In their report on "The Responsible Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974," Uslaner and Conway (1985) contend that the conventional interpretation of the 1974 election—that voters treated it as a referendum on Watergate and the economy—is adequate after all. Their case rests on two lines of argument. One is that vote switches between 1972 and 1974 were associated in the expected direction with voters' reactions to Watergate and the economy. The evidence, taken from probit analyses of data from the 1972-74 panels of the National Election Study (NES), is consistent with this argument, although the effects are by no means overwhelming. Apparently they had to construct a single variable from two distinct questions—about approval or disapproval of Ford's pardon of Nixon and changes in personal finances—in order to produce a statistically significant connection between the vote and reactions to Watergate or the economy (p. 793). Contrary to their hypothesis, changes in ratings of Nixon on the "feeling thermometer" were not significantly related to either votes or vote shifts. Still, in contrast to most pre-

vious survey studies of 1974, they show that some voters apparently did react directly to national conditions in the expected manner.

Uslaner and Conway's second line of argument is an attack on our contention that the strategic decisions of candidates and contributors, guided by expectations about how Watergate and the economy would affect electoral odds, had an important effect on 1974 election results quite independent of voters' direct reactions to national issues (Jacobson and Kernell, 1983). Their claim here, however, rests on a fundamental misunderstanding of how campaign spending affects congressional elections.

The heart of their analysis is a discussion of spending ratios between types of candidates (p. 791). But ratios are meaningful comparative measures of financial strength only if the marginal effects of campaign spending are identical for incumbents, challengers, and candidates for open seats. The evidence is overwhelming that this is not the case; specifically, challengers gain far more from a given amount of campaign spending than do incumbents (Jacobson, 1985a). The Uslaner-Conway approach

would, for example, equate a situation in which the average challenger and incumbent both spent \$1,000 with one in which both spent \$300,000. On the evidence of the last seven House elections, in the former case no challenger would ever win, while in the latter more than 20% would defeat the incumbent (Jacobson, 1985b).

The crucial figures for 1974 are not the ratios of spending by incumbent Democrats and Republicans to spending by their respective challengers, but the extraordinarily high average of \$59,331 spent by Democratic challengers and the extraordinarily low average of \$20,744 spent by Republican challengers. Using the expenditure coefficients estimated for our book (p. 46), which Uslaner and Conway offer as a justification for their approach (p. 790), we can project that, *with incumbent-challenger spending ratios unchanged*, had Democratic challengers spent as little as Republican challengers in 1974, their average share of the vote would have been 5.4 percentage points lower. Similarly, had Republican challengers spent as much as Democratic challengers, their average share of the vote would have been 3.4 percentage points higher. And finally, had Republicans and Democrats both matched the combined averages for both parties in 1974 (\$40,157 for challengers, \$63,993 for incumbents), the Democratic vote would have been 3.2 percentage points lower. This final figure is a reasonable estimate of the aggregate impact of campaign spending differences between Democratic and Republican challengers and incumbents in 1974 races for seats held by incumbents. It represents half of the 6.4 percentage point increase in the Democratic share of the two-party House vote between 1972 and 1974 in contests for these seats.

Uslaner and Conway's other test of the Jacobson-Kernell thesis produced mixed results. They find that Republican challengers' spending had a negative effect on

the probability of voting for a Democratic incumbent, which is consistent with the thesis, but they find no effect for spending by Democratic challengers. They argue that this undermines the thesis, claiming, in addition—and inaccurately—that spending should have had greater effects for Democratic than for Republican challengers (p. 795). Nothing in our argument implies such asymmetry.

However, later on (pp. 799–800) they point out that the panel sample underrepresents districts with Republican incumbents and, to an even greater extent, districts with Republican incumbents who had close contests: “just four people interviewed who switched votes from 1972 to 1974 . . . resided in districts that tossed out Republican incumbents!” (p. 800). They conclude that “the bias in the sample undoubtedly attenuates the coefficients for Watergate and economic grievances” (p. 800). If so, estimates of the effects of spending by Democratic challengers in these contests will also be “attenuated.” Uslaner and Conway cannot have it both ways, faulting the Jacobson-Kernell thesis when it does not fit the data, and faulting the same data when they do not support their argument.

There is, of course, an easy way to test whether Democratic challengers' expenditures affected vote shifts in Republican-held districts in 1974. By regressing the district-level vote shift on spending by the Democratic challenger and Republican incumbent, we find that Democratic challengers enjoyed an increase of about one percentage point over their party's 1972 vote share for every \$11,900 they spent ($t=4.79$). The proportion of votes shifting from Republican to Democratic candidates in these districts is strongly and positively related to how much the Democratic challenger spent on the campaign.

Potential campaign contributors clearly had very different expectations about the prospects of Democratic and Republican challengers in 1974, and they distributed

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their money accordingly. Republican challengers that year were the most poorly financed group of challengers in any election since accurate campaign finance data have become available. Democratic challengers were, in real terms, better funded than any group of challengers prior to the 1980 Republican cohort. Recognizing the effects of campaign spending in House elections, it follows that their strategic choices contributed substantially to overall Democratic vote gains. Uslaner and Conway present evidence that elite expectations were grounded in reality: some voters evidently did cast votes based on Watergate and the economy. This is an important point, for strategic behavior not rooted in reality would be irrational and unstable. But their approach leads them to underestimate the extent to which elite strategies magnified the effects of Watergate and the economy in 1974, and so to overstate the case for the conventional interpretation.

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Why did the Democrats gain 49 House seats (and 4 Senate seats) in 1974? Most academic accounts failed to find support for the "conventional wisdom" explanation that voters were motivated by Watergate and high inflation. Jacobson and Kernell (1983) suggest that because national political trends appeared to favor the Democrats, they were able to attract well-qualified and well-funded challengers to Republican incumbents. We have challenged this thesis and argued that there was retrospective voting in 1974, particularly among those voters who switched party preference from 1972 to 1974 (Uslaner and Conway, 1985).

Jacobson and Kernell criticize our formulation on several grounds. We shall address their specific complaints first, before moving on to the larger issue that separates us. They object to our construction of a variable combining the effects of Watergate and the economy, without specifically challenging our theoretical basis for doing so (cf. Brody, 1977). Moreover, they note that changes in people's thermometer ratings of Nixon failed to achieve statistical significance, although they did not point out that this variable had a marginal impact on the switchers' vote choice of more than 20% (Uslaner and Conway, 1985, p. 797). We claimed that the underrepresentation of districts with vulnerable Republican incumbents probably led to attenuated coefficients for our four retrospective voting predictors. Jacobson and Kernell correctly argue that this sampling problem also would affect Democratic challenger spending. Yet there is no reason to believe that the effects of spending will be more attenuated than those of the behavioral variables, and in the equations estimated, Democratic challenger expenditures always failed to reach significance.

Jacobson and Kernell also argue that our reconstruction of Table 4.1 in their book (1983) is based upon "a fundamental misunderstanding of how campaign spending affects congressional elections." We reanalyzed their data to see if there was support for their thesis that 1974 was marked by greater financial gains for Democratic challengers than for incumbents. In making these comparisons, we assumed that both incumbent and challenger spending matter in congressional elections. According to Jacobson and Kernell (1983), it is challenger spending that is most important. However, research by Ragsdale and Cook (1984) and even Jacobson (1985a) himself indicates that incumbent spending does matter, especially when sitting members are strongly challenged. If both incumbents'

and challengers' expenditures matter, then the appropriate question is not whether we can make comparisons, but how we do so. Jacobson and Kernell (1983) compare spending levels between Democratic and Republican challengers and incumbents, respectively. But Democratic incumbents do not run against Democratic challengers; they run against Republican challengers. Comparisons between candidates in the same party assume there is a fixed pie of contributions for each party. Data from campaign expenditures, including those reported by Jacobson and Kernell (1983), belie this conclusion. What made 1980 such an interesting election is how much money went to Republican challengers rather than to Democratic incumbents, so it certainly seems that this is the appropriate comparison. In our Table 1 (p. 791) we analyzed these expenditures by means of ratios. We did not assert that these ratios constituted a measure of the marginal effects of campaign spending. When we performed probit analyses of the effects of spending on voter choices, the actual levels of expenditures by Republican and Democratic candidates in 1974 were used.

The final issue raised by our critics points to the heart of our disagreement, as reflected in the regression they report on the effects of Democratic challenger expenditures in 1974. We are not at all surprised that an aggregate-level analysis shows a significant *t*-ratio for such expenditures. Rather than dismissing the "strategic politicians" thesis, we argued that the effects of the quality of candidates and of expenditures are decidedly secondary to national macropolitical forces (see esp. Uslaner and Conway, 1985, pp. 800-801). Without any controls for behavioral variables, it is only commonsensical that Democratic challenger expenditures would be strongly related to the aggregate party vote in a year such as 1974, when the party was able to attract the best and the brightest among potential

candidates, and when the level of funding for Democratic candidates compared to that of their Republican opponents rose dramatically across the board (including, of course, for challengers) from two years earlier. But does this imply that the "strategic politicians" thesis tells us all we need to know about the 1974 congressional elections? The aggregate analysis that provides the best support for our argument is that of Jacobson and Kernell (1983), which indicates that the partisan status of the challenger has a higher *t*-ratio than challenger expenditures for 1974, and that the lowest *t*-ratio is obtained for the quality of the challenger.

We shall never be able to resolve statistically the major outstanding issue of which came first, macropolitics or strategic politicians, because the two theses are complementary. We have no doubt that 1974 was marked by relatively well-funded and high-quality Democratic challengers, although we seemingly differ with Jacobson and Kernell as to whether Democratic incumbents also were so advantaged in that contest. But this is precisely the problem for the strategic politicians thesis: Democrats were blessed with a bumper crop of strong challengers precisely when they didn't need them. The major blessing a Democratic challenger had in that year was the simple fact of his or her party. In contrast, the Democrats needed a group of well-qualified candidates in 1980. With national trends running against them, they could not convince strong candidates, who would have the best chance of raising lots of money, to seek election to the Congress, while Republicans were bursting at the seams with such challengers. In more politically neutral years, such as 1976 or 1978, the impact of strong challengers might have been greater for either party. Yet, precisely because there were no macropolitical factors that would lead such candidates to see real chances for breakthroughs, neither party could attract the

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best set of challengers potentially available, thus limiting the gains either party could make. The strategic politicians thesis thus appears almost perfectly colinear with the macropolitics argument.

The strategic politicians thesis was offered by Jacobson and Kernell to account for what was perceived to be a failed conventional wisdom. Now that we have shown that the politicians and journalists did not get the story of 1974 wrong after all, there is no need to resort to a subsidiary, albeit complementary, explanation of what happened in that election. Second, the Jacobson-Kernell thesis is based upon an indirect model of political accountability, whereas ours is based upon a more direct linkage. Third, the macropolitical argument would make sense even if the strategic politicians thesis did not (or was not supported empirically). On the other hand, the strategic politicians thesis, taken by itself—that is, divorced from any macropolitical forces—makes no sense whatsoever. Finally, the macropolitics thesis explains both the breadth and depth of the Democratic sweep in 1974. The strategic politicians argument offers us no help whatsoever in understanding why so many Democrats with no previous electoral experience—and often with little or no funding—

ousted Republican incumbents in state and local contests. Even though the two theses are complementary, it is clear, to us at least, which came first—and therefore which is the more convincing.

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RESEARCH
NOTES

STRATEGIC POLITICIANS AND UNRESPONSIVE VOTERS

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To resolve the paradoxical finding that economic conditions and presidential popularity powerfully relate to quadrennial movement in the House vote, but explain little individual voting variation, Jacobson and Kernell have advanced their "strategic politicians" model of midterm elections. Purportedly, the early-year political environment helps determine how many of a party's strong potential contenders risk challenging incumbents; voters in turn respond to the quality of candidates before them in November, and thus indirectly reward the party favored by this early-year environment. Despite its current prominence, however, the theory does not stand up to empirical investigation. A time-series equation of midterm outcomes regressed on early-year national conditions does not fare particularly well when contrasted with comparable equations assuming direct-effects voting. Furthermore, challenger quality has only a weak influence on individual voters—subordinate, in fact, to the effects of economic attitudes and presidential evaluations.

Jacobson and Kernell's (1981, pp. 60-71) model of House midterm electoral behavior has come to enjoy widespread acceptance among political scientists. Adjectives such as "persuasive" (Bond, Covington, and Fleisher, 1985, p. 511), "compelling" (Brace, 1985, p. 107), and "ingenious" (Abramowitz, 1984, p. 710) are now virtually commonplace in references to it. Favorable notice has extended beyond the boundaries of academic political science as well; for example, in five 1982-83 articles, *National Journal* correspondents Richard Cohen (1982a, p. 60; 1982b, p. 1519; 1983a, p. 712; 1983b, p. 2018) and Dom Bonafede (1982, p. 1833) incorporate tenets of the model into their own electoral analysis.

The springboard for Jacobson and

Kernell's work is the oft-noted finding that national issues and presidential approval ratings relate powerfully to midterm outcomes in aggregate, time-series analyses, while explaining little individual voting variation in cross-sectional, survey-based studies. To resolve this paradox, they argue that issue and presidential evaluations largely register on voting decisions indirectly, mediated through "self-reinforcing" strategic choices of political elites who believe—regardless of the reality—in substantial November voting based on national concerns. Hence, fewer able candidates of the administration party and more of the out-party risk opposing House incumbents in years with unfavorable economic tidings and an unpopular president, and financial contributions from other elites are

similarly affected. The net result, then, is reinforcement of the more modest direct impact that national concerns alone have on voting.

Extensive supporting data on challenger quality and campaign expenditures are presented, but the former (coded according to whether he or she ever held elected office) are utilized more, in that reliable spending figures do not exist for the first seven post-World War II midterms considered. The novel contribution of Jacobson and Kernell here, as well as the key evidence exhibited to validate their model, is an effort to demonstrate that a party's numerical advantage in experienced challengers is not merely a response to a promising political outlook, but a major cause of favorable election results themselves. In a chapter of their book entitled "Midterm Congressional Elections Revisited: A Test of the Theory," they show that a 1946-78 equation regressing the administration party midterm vote on presidential popularity and annual change in real per capita income has a greater R^2 when the independent variables are measured early in midterm years rather than later, as in their modification of the equation previously developed by Tufte (1978, pp. 106-15), substituting annual third quarter-to-third quarter income change for his original midyear-to-midyear measure. That such early-year data from the time when serious potential contenders are said to be debating whether to run should offer a better explanation than data more contemporaneous with the voting act itself seems to suggest the obvious: direct election-day effects of national concerns are less important than prior strategic choices of politicians.

As subsidiary substantiation, less systematic but more straightforward evidence that candidate quality affects midterm results has been presented in a related paper by Jacobson (1981, p. 12). Brief mention is made that in 1946, 1954,

1958, and 1974, quality challengers brought about an average party vote shift about 2% better than that in districts where the party lacked such challengers.

The underlying theory of the "strategic politicians" model appears so plausible and the accompanying evidence—while limited in the sense just described—so consistent that it is not difficult to understand its current appeal. Undoubtedly, however, these same virtues have discouraged some needed reexamination. Such reevaluation, we shall argue, makes the authors' case far less compelling.

The "Strategic Politicians" Model Revisited

While the explanatory edge of early-year variables in Jacobson and Kernell's analysis is noteworthy, what is unconvincing is the conclusion that this advantage consequently vindicates their model. The assumption that the first part of an election year is the crucial period for making candidacy decisions is itself questionable. No systematic evidence is supplied. Although few studies bear on this matter, the existing information suggests a different decision time for large numbers of those who do end up running. Huckshorn and Spencer (1971, pp. 57-59) report that of all losing nonincumbents in November 1962, almost 30% publicly declared for office by the end of 1961. Given the obvious lag between personally committing oneself to the race and the announcement itself, an even greater share of candidacies actually must have been decided before January. More recently, all 1978 nonincumbent primary contenders surveyed by Maisel (1982, p. 20) claimed to have resolved to run no later than the preceding fall. Fowler (1976, p. 150), on the other hand, finds that owing to the lateness of the 1974 New York primary (September 10), only 18.6% of general election nonincumbents made commitments as early as 1973. Yet even

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here, relatively few decisions occurred early in the election year—48.8% were after May.

A more fundamental question concerns the presumption that a successful candidate-centered model can be built upon data covering all midterms since 1946. Most analysts regard the 1960s—especially the middle part of the decade—as the watershed wherein individual candidate and campaign characteristics came to play a major role in voting decisions. Nelson (1978–1979, pp. 666–74), for example, documents a partisan defection surge at this time brought on by incumbents' increased attractiveness to opposition partisans. Concurrent growth in the heterogeneity of swings within districts likewise has been read as a sign of more candidate-directed voting (Mann, 1977, pp. 83–87). More speculatively, Abramson, Aldrich, and Rohde (1983, pp. 200–201) have argued that campaign spending took on added significance in the mid-1960s when electioneering became more dependent on costly new technology. Thus, for about half of the postwar period considered, it is unclear why early strategic decisions of prospective candidates should strongly affect midterm outcomes.

We shall further demonstrate, however, that even at the level of explanatory power alone, it is difficult to make a strong circumstantial case for the model. Elaborate alternative models that assume direct-effects voting are not required to attain superior performance in many respects. All our analyses below will be extended through 1982.

Examination will first center simply on Tufte's (1978) original, formative model,¹ which regresses the administration party's standardized vote (its percentage of the national two-party House vote minus its average over the past 16 years) on percentage change in real disposable personal income per capita between the midpoints of the pre-election and election years, and on the presidential approval rating in the

election-year Gallup Poll administered closest to September 1.² As mentioned above, Jacobson and Kernell choose to examine their own revised version of this model, substituting third quarter-based annual income change. Even though we agree with their rationale—that July–September conditions, being nearer to election day, should more strongly relate to electoral choices if such choices indeed directly hinge on economic evaluations—it still seems unwise to exclude Tufte's original model altogether, owing to uncertainty in the literature over the time lag necessary before concrete economic events register on assessments of government (Norpoth and Yantek, 1983, pp. 786–89, 798–803).

The second model tested is a simple alternative to the revised Tufte model, but we believe it better accounts for direct-effects voting than does either version of his. Here, the economic variable is change in the same income index from the first three months of the pre-election year to the last full quarter before the midterm. A third quarter end point leaves just over a month remaining before election day, of course, but October–December data pose a worse problem, in that conditions during almost two-thirds of this latter interval would not yet be experienced by the time of balloting. Our assumption that voters are more likely to consider economic performance across 18 rather than 12 months strikes us as reasonable, since it permits the initial quarter under each new administration to serve as a natural comparative baseline. In selecting a presidential popularity indicator, we are not constrained by the need to work only with quarterly measures organized as above, which are imposed by the form of existing income data. Hence, while a three-month interval will be used to calculate average Gallup Poll approval, it will instead run from August to October, so that the campaign period itself can more fully be covered.

Table 1. A Test of the Explanatory Power of the Three Midterm Models, Based on National Election Results, 1946-1982

Coefficient	Model 1	Model 2	Model 3
Income change	.692* (.201) .741	.659** (.180) .777	.596** (.153) .776
Presidential approval	.086 (.051) .366	.051 (.040) .271	.078 (.050) .310
Constant	-7.959	-6.588	-7.971
R^2	.675	.684	.722
\bar{R}^2	.582	.594	.643

Note: Entries on first line are unstandardized regression coefficients and their standard errors (in parentheses); those on second line are standardized regression coefficients. Models 1-3, respectively, are Tufte's (based on 12-month, midyear-to-midyear income change and presidential approval in poll nearest to September 1), Jacobson and Kernell's (based on 12-month, first quarter-to-first quarter income change and average January-March presidential approval), and our own (based on 18-month, first quarter-to-third quarter income change and average August-October presidential approval).

*Significant at .05 level (two-tail t-test).

**Significant at .01 level (two-tail t-test).

Finally, we replicate Jacobson and Kernell's model by including first quarter-to-first quarter income change³ and the president's average January to March popularity.⁴ Income change values vary somewhat from theirs, in that the latest Bureau of Economic Analysis revisions are employed here.⁵ Additionally, we rely on the longitudinal Gallup Poll approval data reported in the October-November 1980 *Gallup Opinion Index* and the December 1983 *Gallup Report*. (Jacobson and Kernell apparently took most of the 1946-78 data used in their test from another Gallup compilation that was less comprehensive for a number of years.)

The results of regressing the standardized administration party vote on the independent variables are in Table 1. Contrary to Jacobson and Kernell's claim, their model does not perform very impressively compared with those postulating direct effects of national concerns. The R^2 of their equation (.684) just barely exceeds Tufte's R^2 of .675, while falling short of our own value of .722.

A second major criterion for evaluating

time-series models, of course, is predictive ability. In this regard, we contrast the models using an *ex post facto* forecasting scheme, where each election in the 1966-82 period is predicted according to the regression equation based on all preceding midterms. Two indices gauge each model's aggregate success: (1) the mean absolute prediction error and (2) Theil's inequality coefficient (U), which has the advantage of being restricted to a range of zero to one, with lower values signifying greater accuracy.

As seen in Table 2, the "strategic politicians" model has a mean absolute error of 2.35%, by far the worst. Our own 1.21% error is the smallest, below Tufte's 1.41%. The U values tell the same story, with the first model again demonstrating the weakest correspondence between predictions and actualities.⁶

Also important in Table 2 are the R^2 trends as more midterms are added. If we consider as well the Table 1 equations based on all the 1946-82 data, and regress the R^2 values on time ($t = 1$ for 1946-62, and 6 for 1946-82), the resulting slopes (b)

Table 2. The Forecasting Accuracy of the Three Midterm Models Based on National Election Results, 1966-1982

Midterm Election being Forecast	Midterms Used in Prediction Equation	Model 1			Model 2			Model 3		
		Prediction Error Percentage	R^2 (\bar{R}^2) of Prediction Equation		Prediction Error Percentage	R^2 (\bar{R}^2) of Prediction Equation		Prediction Error Percentage	R^2 (\bar{R}^2) of Prediction Equation	
1966	1946-62	-.45	.812 (.624)		1.73	.930 (.860)		1.63	.818 (.637)	
1970	1946-66	-1.11	.817 (.695)		-2.03	.893 (.822)		-.00	.797 (.662)	
1974	1946-70	.14	.815 (.722)		-4.46	.835 (.753)		.15	.817 (.725)	
1978	1946-74	-2.76	.826 (.757)		-1.32	.735 (.629)		-1.28	.828 (.759)	
1982	1946-78	-2.58	.760 (.680)		-2.21	.748 (.665)		-2.97	.833 (.777)	
Mean										
Absolute Error		1.41			2.35			1.21		
Theil's Inequality Coefficient (U)		.291			.351			.280		

Note: Models 1-3, respectively, are Tufte's (based on 12-month, midyear-to-midyear income change and presidential approval in poll nearest to September 1), Jacobson and Kernell's (based on 12-month, first quarter-to-first quarter income change and average January-March presidential approval), and our own (based on 18-month, first quarter-to-third quarter income change and average August-October presidential approval).

show a slight and moderate reduction in effectiveness for our model ($b = -.009$) and Tufte's ($b = -.024$), but a precipitous decline for Jacobson and Kernell's ($b = -.05$). This last, large negative slope is very troublesome, since, as argued above, a truly candidate-centered model should manifest precisely the opposite pattern—that is, relatively weak explanation before the mid-sixties, followed by improved performance as more midterms are included from the period when the personal dimension of electoral standing grew in importance.⁷

Finally, the predictive abilities of the models in 1982 deserve special attention. Before this latest midterm Jacobson and Kernell (1982, pp. 425–27) accurately prophesied that Republican losses would be much lower than Tufte's original 1946–78 equation predicted. They also were correct, however, in warning that their own model was unlikely to do better, owing to unusually aggressive Republican recruiting and fundraising efforts designed to offset the deflationary impact of early-year recession and Reagan's unpopularity. Although a specific alternative prediction derived from this argument is not presented, it still is possible to do so by employing a variant of their model, which should be able to account, at least in part, for such party activism arising independently of first-quarter national conditions. Here, in a 1946–78 equation using only non-southern election returns, Jacobson (1983a, pp. 142–50) adds a third independent variable equaling the northern state difference between the percentages of administration and opposition party challengers with elected office experience. Ironically, though, this fuller equation yields a less precise forecast of the 1982 northern vote, 2.48% below the actual Republican value, compared with a 2.2% underestimate for his corresponding two-variable equation.⁸ Thus, neither the original "strategic politicians" model nor

the effort to render it even more explicitly candidate-centered seems able to account for this rather enigmatic outcome.

While the performance of Jacobson and Kernell's model is comparatively unimposing, their focus on first quarter-based analysis admittedly is only an indirect test of the theorized electoral role of early strategic decision making. We now take the less circuitous route of determining what influence quality challengers have on individual votes. Such analysis, of course, cannot shed light on whether prospective challengers' candidacy decisions are substantially rooted in their evaluations of national conditions, but failure to uncover a causal link between quality challengers and voting behavior would alone be sufficient to undermine their theory.

Individual Electoral Choice: The Impact of Challenger Quality Versus Economic and Presidential Evaluations

As noted above, the cross-sectional data presented by Jacobson and Kernell to demonstrate an immediate electoral effect of challenger quality are very sketchy. The only other evidence bearing on this matter turns up in an analysis of the 1974 midterm, in which they assess the combined influence of officeholding experience and campaign spending on challengers' margins. While the coefficient of the former falls a bit short of significance (Jacobson and Kernell, 1981, p. 46), they go on to argue that the full impact failed to register because challenger spending is itself related to having held office. A fairer test of the force of challenger quality thus would exclude spending information, but another problem would remain. Signs of pending incumbent vulnerability in the election ahead likely will improve the odds of a stronger challenger emerging, thereby posing a reciprocity danger

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which could inflate the values of the challenger quality coefficient. The customary two-stage corrective technique adopted here is to construct first an instrumental variable for quality and then substitute it in the final estimation.

Jacobson and Kernell's supposition about the electoral appeal of officeholding is tested with data from the National Election Studies (of the University of Michigan's Center for Political Studies) for all midterms since 1966. At the same time, this effect is contrasted with that arising from perceptions of the economy and the president. Here, variables derived from all economically relevant survey items repeated across the five midterms are included, as well as the presidential thermometer question asked in all years but 1966. The equation estimated for each election for voters in districts with an opposed incumbent is

$$\begin{aligned} CVOTE = & a_1 + b_1QUAL + b_2PID \\ & + b_3CPMARG + b_4UN + b_5INF \\ & + b_6PASTYR + b_7NEXTYR \\ & + b_8PRES + u, \end{aligned} \quad (1)$$

where

CVOTE is 1 if a pro-challenger vote is cast, and 0 otherwise;

QUAL is 1 if the challenger has held elected office, and 0 otherwise;

P is 1 if the challenger is Democratic, and 0 if Republican;

PID is 1 for voters sharing the challenger's party affiliation, 0 for independents, and -1 for incumbent party identifiers (independent leaners are grouped with weak and strong partisans);

CPMARG is the two-party House vote proportion for the challenger's party in the last election (voters in districts with uncontested races in this election are not analyzed);

UN is 1 for voters living in districts with a nonadministration party challenger and citing unemployment as the most

important problem, -1 for those having an administration party challenger and citing this problem, and 0 for those not citing it;

INF is coded like *UN*, except that the problem is inflation;⁹

PASTYR is 1 for voters living in districts with a nonadministration (administration) party challenger and experiencing a worsened (improved) personal financial situation over the past year, -1 for those having an administration (nonadministration) party challenger and experiencing a worsened (improved) situation, and 0 for those seeing no change;

NEXTYR is coded like *PASTYR*, except that the evaluations concern the expected financial situation over the next year;

PRES is the president's thermometer rating for voters in districts with an administration party challenger, and 100° minus the rating for those with a nonadministration party challenger; and *u* is the error term.

The case for including the controls *P* and *PID* is obvious; *CPMARG* accounts for the likelihood that a weak prior showing by the incumbent will in itself be related to backing the opposition in the forthcoming election, while also encouraging a quality challenger to emerge.

Dependable instrumental variables for *QUAL* could be created only in 1966 and 1982. Predicted probabilities of having an experienced challenger in one's district (*QUAL*) are calculated by performing a probit analysis of *QUAL* on the remaining independent variables, plus the natural log of the incumbent's number of terms and eight dummy variables delineating U.S. census regions.¹⁰ The resulting predictions are transformed into probabilities by means of the cumulative standard normal distribution. Seniority had a positive effect ($p < .001$) on the odds of a quality

challenger in the former election, but its maximum likelihood estimate (MLE) in 1982 was in the opposite direction ($p < .01$). Conceivably, it took time before politicians recognized that a junior member, if spared defeat, could turn his or her district into a formidable election fortress. Back in 1966, when incumbency was just beginning to take on substantial electoral importance, there might still have been greater tolerance for delaying challenges until more senior members showed signs of flagging vigor and constituency concern. Experienced challengers also varied considerably by region; overall, the Mountain state reference group differed significantly (at $p < .05$ or better) from two-thirds of the other regions. In general, regions with less heavily populated states tended to have greater percentages of experienced challengers, probably because of higher ratios of state legislators and local government officials to the number of House seats.¹¹ Predictions of actual *QUAL* values based on the 1966 and 1982 instrumental equations were 14.1% and 16.6% more accurate, respectively, than those where all cases were predicted to be the more common zero value of *QUAL*.

The same instrumental equations estimated for the other three midterms, however, were much less powerful. Even including additional exogenous variables failed to produce a sufficiently strong instrument.¹² Consequently, estimation of the *CVOTE* equation here must rely only on the original *QUAL* values.

Table 3 contains the one-stage probit MLEs for all elections, plus the two-stage estimates for 1966 and 1982. The results clearly are not encouraging for the "strategic politicians" theory. In the one-stage analyses, no *QUAL* MLE is significant at the .05 level, and only three even have the hypothesized positive sign. Furthermore, in conformity with our speculation about reciprocity bias in one-stage relationships between challenger experi-

ence and incumbent vulnerability, the 1966 and 1982 impacts of quality are sizably reduced in the two-stage reestimations.

The case for voters responding to national issues and presidential standing is, to be sure, also less than overpowering. Slightly over half of all economic MLEs, whether the one- or two-stage results are considered, are negative. Still, there are positive signs each of the four times when an economic coefficient is at least twice the size of its standard error. Moreover, all four presidential MLEs are positive, and three are significant.¹³ It seems, then, that Jacobson and Kernell's theory inverts the true relative importance of challenger quality and national-level forces as direct causes of voting behavior.¹⁴

A final question concerns the extent to which each kind of individual voting accounts for the movements in the off-year vote analyzed in time-series studies. A variable's impact on an election, of course, depends on the distribution of its values as well as its MLE. Our strategy, which has been applied before by researchers (Abramowitz, 1984, pp. 715-19; Kiewiet, 1983, pp. 118-24), first generates a respondent's predicted probability of supporting the challenger using the relevant Table 3 equation and actual values of the right-hand variables. Next, individual probabilities are recalculated by assigning "neutral" positions to each case on all economic and presidential evaluation variables; that is, actual values are replaced by zeros on *UN*, *INF*, *PASTYR*, and *NEXTYR*, and by 50° on *PRES*. A third series of probabilities similarly is produced employing the actual values of all variables save *QUAL* (or *QUAL*), which instead is assigned zeros. By then separately subtracting the latter two probability sets from the first, we are left with the individual-level impacts of national concerns and challenger quality, respectively, on challenger support. Final-

Table 3. A Probit Analysis of the Effects of Challenger Quality and Economic and Presidential Evaluations on Individual Voting in Five Midterms

Midterm Election Maximum Likelihood Estimates											
		1966	1970	1974	1978	1982					
Quality challenger	One-stage	.242	(.187)	-.168	(.267)	-.068	(.194)	.052	(.198)	.289	(.180)
	Two-stage	.067	(.279)	—	—	—	—	—	—	-.162	(.382)
Party	One-stage	-.484*	(.193)	-.134	(.269)	.214	(.256)	-.347	(.260)	.069	(.241)
	Two-stage	-.522**	(.200)	—	—	—	—	—	—	.242	(.271)
Party identity	One-stage	.949***	(.083)	1.333***	(.138)	.813***	(.088)	.721***	(.090)	.827***	(.112)
	Two-stage	.943***	(.083)	—	—	—	—	—	—	.805***	(.111)
Past challenger party vote	One-stage	-.048	(.991)	2.583	(1.691)	3.264**	(1.162)	2.808**	(.860)	2.018*	(.933)
	Two-stage	.390	(1.118)	—	—	—	—	—	—	2.273*	(.977)
Unemployment citation	One-stage	1.476*	(.738)	-.500	(.454)	.942*	(.364)	-.091	(.466)	.095	(.176)
	Two-stage	1.511*	(.739)	—	—	—	—	—	—	.096	(.176)
Inflation citation	One-stage	-.087	(.283)	1.040*	(.408)	.129	(.157)	-.039	(.150)	-.222	(.421)
	Two-stage	-.085	(.283)	—	—	—	—	—	—	-.128	(.429)
Finances over last year	One-stage	-.138	(.104)	.131	(.135)	.227*	(.097)	.029	(.094)	-.075	(.108)
	Two-stage	-.145	(.104)	—	—	—	—	—	—	-.081	(.107)
Finances over next year	One-stage	.137	(.123)	-.218	(.171)	-.224	(.118)	-.066	(.114)	-.063	(.145)
	Two-stage	.139	(.123)	—	—	—	—	—	—	-.048	(.145)
Presidential thermometer	One-stage	—	—	.012*	(.005)	.008*	(.004)	.006	(.004)	.016***	(.004)
	Two-stage	—	—	—	—	—	—	—	—	.016***	(.004)
Constant	One-stage	4.709	2.897	2.868	2.951	2.673	2.652	2.673	2.652	2.673	2.652
	Two-stage	4.604	—	—	—	—	—	—	—	—	—
R ²	One-stage	.463	.637	.497	.413	.583	.577	.583	.577	.583	.577
	Two-stage	.459	—	—	—	—	—	—	—	—	—
Percent correctly predicted by equation	One-stage	79.3	83.3	79.9	81.0	83.0	82.7	83.0	82.7	83.0	82.7
	Two-stage	79.3	—	—	—	—	—	—	—	—	—

Table 3 (continued)

		Midterm Election Maximum Likelihood Estimates				
		1966	1970	1974	1978	1982
Percent correctly predicted by "null" model	One-stage	64.5	64.4	62.3	78.9	68.9
	Two-stage	64.5	—	—	—	68.9
N		420	289	398	506	411

Note: Entries are maximum likelihood estimates of probit equations and their standard errors (in parentheses). Ns in 1970 and 1974 are weighted numbers of respondents. The 1970 analysis excludes Form 2 whites, who were not asked either question concerning personal finances.

*Significant at .05 level (two-tail t-test).

**Significant at .01 level (two-tail t-test).

***Significant at .001 level (two-tail t-test).

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Table 4. The Net Impacts of Challenger Quality and Economic and Presidential Evaluations on the National Administration Party Midterm Vote

Midterm Election	Standardized Administration Party Percentage of National Two-Party Vote Predicted from Probit Equations	Net Percentage Impact Owing to Economic and Presidential Evaluations	Net Percentage Impact Owing to Challenger Quality
1966 One-stage	-1.21	.12	-.87
Two-stage	-1.21	.09	-.25
1970 One-stage	3.27	1.56	-.26
1974 One-stage	-3.70	.04	.21
1978 One-stage	-.76	2.49	.18
1982 One-stage	.44	2.50	-1.40
Two-stage	.45	2.56	.80

Note: Standardized vote percentages and net impacts are calculated from Table 3 probit equations.

ly, each impact's net contribution to the national administration party vote is estimated by averaging across respondents (before averaging, impact values are reversed in sign for those in districts with nonadministration party challengers).

Besides the two net impacts, we include in Table 4 as the analogue of the dependent variable used in time-series analyses the standardized national vote of the administration party predicted according to the true values of all independent variables (as before, the standardization subtracts from the predicted percentage the actual mean received during the past eight elections). Across the period considered, it is evident that the more the administration party is favored by economic and presidential evaluation impacts, the stronger is its showing; the equation for the vote regressed on net impact (*NI*) incorporating the two-stage 1966 and 1982 results is $Y = -1.875 + 1.102NI$, $R^2 = .287$ (if only one-stage results are included, $Y = -1.905 + 1.127NI$, $R^2 = .29$). Net impacts of challenger quality, however, have virtually no effect, and the little that exists is in the wrong direction: $Y = -.239 - 1.109NI$, $R^2 = .036$ (with only one-stage results,

$$Y = -.869 - 1.114NI, R^2 = .093).$$

While a direct causal link thus exists between aggregate election results and individual voting based on economic and presidential assessments, one should not overdo the substantive importance of this linkage. Net impacts owing to these assessments, after all, explain under one-third the variance in our standardized vote estimates. Many individual-level factors responsible for quadrennial electoral movement consequently remain to be identified, but at least we can say that support for experienced challengers is not one of them.

Summary and Conclusions

Jacobson and Kernell's midterm theory has not fared well in our study. This holds true when their time-series model incorporating early-year economic conditions and presidential approval is contrasted with direct-effects models using later measurements of these variables, as well as when the immediate effect of challengers' elected office experience on individual voting is analyzed.

Perhaps, though, the focus in appraising the quality of a challenge to an incum-

bent should be shifted from characteristics of contenders per se (i.e., experience) to those of campaigns (i.e., spending levels). Expenditures are of equal intrinsic importance to their theory, playing a lesser role in their actual analysis simply because the relevant data exist only since 1972. Within this restricted period, challenger spending certainly has been shown to Affect the House vote significantly. (As noted above, however, Abramson et al. [1983] argue that spending meant less before the mid-1960s.) Could challenger spending, rather than experience, thus be the pivotal intermediary force responsible for much of the aggregate relationship between national conditions and election outcomes left unexplained by direct-effects voting?

We can attempt a very tentative stab at an answer by regressing the administration party's standardized vote on the ratio of its average challenger spending to that of the opposition. If all six 1972-82 elections are used in order to circumvent the problem of just three applicable midterms, the R^2 is only .365; moreover, without 1974—decidedly the worst year for the administration party on both variables—it drops to a scant .069. This is small compared with relationships involving other aggregate-level indicators used to predict election outcomes. The corresponding R^2 of the standardized vote regressed on the difference between administration and opposition party percentages of experienced challengers is .455 (.215 with 1974 excluded), and that for the regression on annual midyear-to-midyear change in real disposable personal income per capita is an even heftier .649 (.47 without 1974).

Thus, we doubt whether officeholding credentials or expenditures really hold the key to explicating the underlying dynamics of midterm vote fluctuations. Political elites may well predicate personal strategy on their reading of the prevailing partisan winds, but there is little reason to think

that the collective consequence of such discrete actions makes much difference on where these winds carry the parties as of election day.

Notes

The survey data analyzed here were made available by the Inter-University Consortium for Political and Social Research (ICPSR).

1. While Tufte states that various causal mechanisms may be responsible for his findings, Jacobson and Kernell (1981, p. 8) argue that the assumption of individual "referendum" voting at least implicitly underlies his model. They thus treat it as a direct-effects model, as shall we.

2. For the latter variable, however, Tufte (1978, p. 111) makes an exception in 1974 by entering Ford's 55% mid-October rating, presumably to sidestep the inflated 66% figure in the pre-pardon "honeymoon" period of early September. We shall follow the same course.

3. Quarterly data on real disposable personal income per capita begin only with the first quarter of 1947; hence, like Jacobson and Kernell, we estimate all models with the 1945-46 income change based on midyear-to-midyear data.

4. In their book, the authors actually calculate average presidential support from March to May, rather than over the January to March interval corresponding to their first-quarter income variable. No explanation for this oddity is provided; certainly, Gallup Poll frequencies during these periods have been similar since 1946. At any rate, Jacobson's later analysis based only on northern election results (see n. 6) uses first-quarter data for both variables.

5. In particular, 1945-74 data are from *The National Income and Product Accounts of the United States, 1929-76, Statistical Tables* (1981), while those for 1977-78 and 1981-82 come, respectively, from the July 1982 and July 1984 issues of *Survey of Current Business*.

6. To account for the South's greater insulation from the national forces affecting strategic decisions, Jacobson (1981; 1983a, pp. 142-50), in a more recent reworking of his model, transforms the dependent variable to equal the northern standardized vote only. The same right-hand variables as before are used, however, since complementary regional data do not exist across the full period studied; thus, it is questionable whether any gains from analyzing more competitive elections really outweigh the asymmetry of explaining solely northern outcomes with data partly reflecting southern conditions. In any event, when we make this change in testing the three models above, the "strategic politicians" equation definitely emerges with the largest 1946-82 R^2 , but still has by far the worst 1966-82 prediction

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error. Furthermore, the "strategic politicians" R^2 s continue to diminish much more rapidly over time than those of the other two models.

7. In the "strategic politicians" model and our own, the presidential approval variable excludes surveys continuing across two months, only one of which is within the stipulated three-month interval. This is necessary, because for a number of earlier elections, it is impossible to tell what percentage of such interviews was done in each adjacent month. The one case where this procedure works against the former model is 1946, when Truman's 63% approval in January was well above the 50% recorded in the succeeding March-April poll. If the average of these two polls is substituted in the "strategic politicians" analyses above, there is some improvement in performance; however, the model's R^2 in the analysis of Table 1 remains below our own, and its prediction error is still the worst of the three models in Table 2. Likewise, its R^2 values over time decline at about the same pace as before.

8. Adding challenger quality yields a lower Republican vote estimate, simply because the party's northern state disadvantage (14.6%) is unusually large; i.e., it may well have had more such candidates than could be expected given national conditions, but then so did the Democrats. (All challenger quality data used here and below come from *Congressional Quarterly Weekly Report*.)

9. Responses involving national problems were classified under the headings of unemployment or inflation using Kiewiet's coding scheme (1983, p. 83).

10. Owing to small numbers of respondents, West South Central voters were combined with those from West North Central states in 1966 (furthermore, no useable East South Central cases existed), while in 1982, East South Central and South Atlantic states were merged, as were New England and Middle Atlantic states.

11. For evidence of substantial variation by state in the appeal of House nominations to state legislators, as well as an inverse relationship between such appeal and state population, see Robeck (1982, pp. 508-509).

12. Following Bianco (1984, pp. 352-61), we tried the annual change in state per capita income and the prior district margin of the presidential candidate of the challenger's party. We may differ in not finding them to be helpful predictors, in that he employs district-level data pooled across all four 1974-80 elections.

13. Removing CPMARG from the Table 3 equations does not result in a significant MLE for either quality variable. The poor performance of challenger experience thus is not a function of any collinearity here.

14. Kramer (1983), however, has warned that cross-sectional estimates of economic voting effects, such as those calculated here, might well be biased, owing to measurement error in the relevant varia-

bles (e.g., perceptions of personal financial gains may poorly tap the underlying theoretical phenomenon—that part of such improvement actually induced by government policy).

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WHO OVERREPORTS VOTING?

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The effects of respondent characteristics with regard to the propensity of nonvoters to report that they voted are examined by analyzing the vote validation studies conducted by the University of Michigan Survey Research Center in 1964, 1976, and 1980. Previous research has suggested that vote overreporting derives from the respondent's wish to appear to engage in socially desirable behavior. This earlier research suggests that the only respondent characteristic that is strongly related to overreporting is race; measures of socioeconomic status and of general political attitudes are said to be at most weakly related to the tendency to exaggerate voting. These earlier conclusions are incorrect. We measure the extent of overreporting for the population "at risk" of overreporting voting: those who did not actually vote. Respondents most inclined to overreport their voting are those who are highly educated, those most supportive of the regime norm of voting, and those to whom the norm of voting is most salient—the same characteristics that are related to the probability that a person actually votes. Blacks are only slightly more likely to overreport voting than whites. The pattern of relations between education and vote overreporting is opposite what would be found if those who falsely reported voting fit the typical image of the uneducated, uninvolved, "acquiescent" respondent who is concerned primarily with pleasing the interviewer.

Many American nonvoters report to survey organizations that they voted. In the four vote validation studies conducted by the University of Michigan Survey Research Center (SRC) and Center for Political Studies, a large proportion of respondents who did not vote, according to checks of local registration and voting records, claimed that they voted: 27.4% in 1964, 31.4% in 1976, 22.6% in 1978, and 27.4% in 1980.

The generally accepted explanation for vote overreporting is that it is an artifact of the interview. Overreporting is seen to

result from the respondent's desire to please the interviewer and to appear to engage in socially desirable behavior. According to Sudman and Bradburn (1974; see also Bradburn and Sudman, 1979), altering the "characteristics of the interview task" by using telephone or mail surveys or random response techniques does not markedly reduce the tendency to give socially desirable responses. Eliminating third parties from the interview also has no consistent effect on vote overreporting (Silver, Abramson, and Anderson, 1986).

Earlier research also claims that the propensity to overreport voting is not related to respondent characteristics such as socioeconomic status, age, and sex, or to political attitudes such as feelings of political efficacy or political trust, strength of partisanship, or sense of citizen duty. Only race, it is argued, is related to the propensity to overreport voting; blacks are more likely to overstate voting than whites (Abramson, Aldrich, and Rohde, 1983; Abramson and Claggett, 1984; Katosh and Traugott, 1981; Sigelman, 1982; Traugott and Katosh, 1979).

Voting is positively associated with respondent characteristics such as education, income, interest in politics, feelings of political efficacy, sense of citizen duty, concern with the election outcome, and strength of partisan identification. In short, Americans who feel more strongly attached to the established political order are more likely to vote. Some popular texts on American political behavior have accepted the conclusion that Americans whose stake in society ought to make them want to appear to be in conformity with social norms are not more likely to overreport voting than those who are less motivated to appear to conform (Flanigan and Zingale, 1983, p. 185; Wolfinger and Rosenstone, 1980, p. 118).

Our research challenges this conclusion. We find that Americans who are more highly educated and more politically efficacious, who have a stronger sense of citizen duty and stronger partisan attachments, and who are more concerned about the outcome of the election, are also more likely to overreport voting.

Data and Measures

We use election and vote validation data from the National Election Studies (NES) of the University of Michigan Center for Political Studies for 1964, 1976, and 1980.¹ A small proportion of

validated nonvoters are actual voters (Abramson and Claggett, 1984). Errors arise primarily when the field staff does not find an actual voter's name on a list of registered voters. To the extent that such errors result from poor maintenance of local registration records, errors in validation will tend to underestimate the participation of voters who live in poor communities, such as blacks and whites with low levels of education and income. This possible bias in the validated vote measure runs against our main thesis—that higher-status respondents are more likely to overreport voting.

Dependent Variable

Previous research has concluded that it makes little difference in the pattern of relations whether the reported or the validated vote is used as the dependent variable. This is not surprising in light of the high correlation between the self-reported and the actual, validated vote.² However, most of this earlier research does not address the question of what the relation is between respondent characteristics and the tendency to overreport voting.

Most researchers have not used an indicator of vote overreporting that is appropriate for understanding what causes an individual to overreport. Only about 1% of respondents who actually voted (according to official records) mistakenly reported that they did not vote. Between 90% and 96% of all vote misreports are by people who did not actually vote. Even so, some researchers analyzing the vote validation data have used as their main measure of vote misreporting the proportion of all respondents whose self-report does not correspond with the official voting records (e.g., Katosh and Traugott, 1981; Traugott and Katosh, 1979; Wolfinger and Rosenstone, 1980, p. 118). Others have looked at the proportion of those who claimed to have voted but did not actually vote (Abramson and

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Claggett, 1984, p. 721; Cahalan, 1968-69; Sigelman, 1982, p. 49).³

At issue in the choice of a measure of overreporting is not the actual distribution of voters and nonvoters, but rather the choice of the appropriate denominator for calculating the proportion misreporting: (1) all respondents, (2) respondents who claimed they voted, or (3) respondents who did not actually vote. Since virtually all misreporting is by actual nonvoters who claim they voted, the third denominator is the most appropriate for representing the population that is "at risk" of misreporting: those who did not actually vote.⁴

The other two approaches reflect the aggregate amount of misreporting of voting, but are not good indicators of the individual behavioral propensity to misreport, since they do not use the appropriate population at risk. Also, they are sensitive to the marginal distribution of actual voters and nonvoters. The larger the proportion of nonvoters among the respondents or in any subset of respondents, the greater the proportion of respondents who are available to misreport that they voted. Other things being equal, the larger the proportion of nonvoters, the higher the total proportion of misreporters will be. This elementary fact was pointed out by Parry and Crossley (1950, p. 75) 35 years ago. If one analyzes misreporting among actual nonvoters, no such sensitivity to the marginal distribution of voters and nonvoters exists. Accordingly, in crosstabulations we use as the dependent variable the *percentage of those who did not actually vote who reported that they voted*.

Independent Variables

We focus on factors related to the normative commitment to voting. We use respondent's education as an indicator of socioeconomic status. We also examine the relation between the tendency to over-

report voting and other measures of the respondent's "motivation to vote": external political efficacy, sense of citizen duty, strength of partisanship, concern about the outcome of the election, and political interest.⁵ Earlier research based on the 1980 NES (Silver, Abramson, and Anderson, 1986) showed that whether nonvoters stated in the pre-election survey that they expected to vote was an extremely strong predictor of whether they claimed that they voted. Thus, we also examine pre-election expectation to vote.

Results

Education and Vote Overreporting

In contrast to previous research, we find that education is positively related to vote overreporting (see Table 1). The relation is weak in 1964, a high-stimulus election which mobilized low-status Americans. Nonetheless, in all three surveys respondents who were college graduates were the most likely to overreport voting, and those who had not finished high school were the least likely to overreport voting. Over time, the relation between vote overreporting and education has become stronger.

Thus, nonvoters who are most likely to overreport voting come from the same educational groups as those who are most likely to vote. Low-status respondents, who are said to be especially inclined to give socially desirable responses to interviewers, particularly when the issue addressed is not very salient (Schuman and Presser, 1981, ch. 8), are not more inclined to overreport voting.

Support for Civic Norms and Vote Overreporting

Since education is related to political attitudes that have been linked with political participation, such as political

Table 1. Validated Nonvoters Who Said They Voted, by Level of Education, 1964, 1976, and 1980

Year	Level of Education				tau _c
	Some High School or Less ^a (%)	Completed High School (%)	Some College (%)	Completed College (%)	
1964	22.3 (184)	34.3 (102)	28.6 (28)	34.8 (23)	.106*
1976	23.5 (356)	28.3 (329)	45.5 (132)	53.1 (89)	.200**
1980	16.7 (174)	27.2 (191)	34.8 (89)	56.1 (41)	.220**

Note: Figures in parentheses are base Ns for the percentages immediately above.

^a"Some High School or Less" includes respondents with up to 12 years of schooling but no high school diploma.

*Statistically significant at or below $p = .05$.

**Statistically significant at or below $p = .01$.

efficacy and level of political interest, these attitudes are likely to be positively related to vote overreporting. Also, if vote overreporters come disproportionately from people who have a strong motivation to vote, then attitudes such as strength of party identification, which are related to the motivation to vote but are not strongly related to education, should also be related to overreporting.

An important indicator of the saliency of voting is whether the respondent stated in the pre-election survey that he or she expected to vote in the forthcoming election. The responses should be closely related to whether the respondent actually voted. In fact, this is so. For the three elections, the Kendall's tau_b coefficients between stated expectation and actual, validated voting range from .55 to .60. However, pre-election expectation is even more strongly related to the self-reported vote; the tau_b coefficients range from .70 to .73. In 1964, 49.1% of validated nonvoters who had stated that they expected to vote falsely reported that they had voted; the corresponding figures in 1976

and 1980 were 57.0% and 51.2%. In contrast, only between 1.5% and 3.5% of nonvoters who had stated that they did not expect to vote claimed later that they voted.

The consistency between pre- and post-election reports about voting (pre-election declaration of intention; post-election self-report of actual behavior) does not seem to be an artifact of the interview-reinterview survey method, for two reasons. First, substantial vote overreporting has been found in numerous other studies in which only a single interview was conducted (e.g., Bradburn and Sudman, 1980; Katosh and Traugott, 1981; Parry and Crossley, 1950; Weiss, 1968-69). Second, in the 1964, 1976, and 1980 SRC election studies there is no relation between whether the respondents gave consistent responses and whether the same interviewer administered both the pre- and post-election surveys (see Silver, Anderson, and Abramson, 1985).

Table 2 shows that in all three elections, the tendency to overreport voting is positively and significantly related to the

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Table 2. Validated Nonvoters Who Said They Voted, by Measures of Respondents' Political Attitudes, 1964, 1976 and 1980

	1964			1976			1980		
	Low (in percentages)	Medium (in percentages)	High (in percentages)	Low (in percentages)	Medium (in percentages)	High (in percentages)	Low (in percentages)	Medium (in percentages)	High (in percentages)
External Efficacy	24.3 (107)	24.7 (150)	36.6 (82)	24.9 (408)	31.4 (234)	43.0 (253)	19.0 (205)	26.4 (148)	41.7 (132)
Tau _c		.091*			.158**			.193**	
Partisan Strength	11.8 (51)	28.0 (182)	34.3 (102)	21.2 (194)	30.8 (557)	49.8 (146)	11.9 (109)	26.2 (294)	50.5 (91)
Tau _c		.137**			.169**			.245**	
Citizen Duty ^a	—	—	—	14.2 (247)	29.9 (349)	48.4 (299)	15.6 (109)	24.1 (191)	39.1 (179)
Tau _c					.290**			.200*	
Concern for Outcome	18.6 (140)		33.5 (182)	23.8 (454)		41.0 (419)	22.4 (241)		34.9 (218)
Kendall's tau _b		.158**			.170**			.132**	
Interest in Campaign	18.2 (121)	30.2 (126)	35.2 (91)	15.6 (266)	31.4 (392)	48.1 (249)	11.1 (190)	32.9 (207)	51.8 (83)
Tau _c		.150**			.264**			.317**	

Note: Cell entries are the percentages of validated nonvoters who falsely reported after the election that they had voted. Figures in parentheses are base Ns for the percentages immediately above. The wording of the items and definitions of the categories used for each measure are given in Silver, Anderson, Abramson (1985).

^aThe citizen duty items were not included in the 1964 SRC National Election Study.

*Statistically significant at or below $p = .05$.

**Statistically significant at or below $p = .01$.

respondent's sense of political efficacy, strength of partisan attachment, concern with the electoral outcome, and interest in the campaign; it is also positively and significantly related to the respondent's sense of citizen duty in both years (1976 and 1980) in which this attitude can be measured. The relation between overreporting and these political attitudes is not as strong as the relation between overreporting and the respondent's expectation of voting, but the evidence refutes the argument that overreporting of voting is unrelated to the respondent's support for civic norms. Further evidence that respondents who are strongly motivated to vote are even more strongly motivated to say they voted is that the relation between the attitude measures and the respondent's self-reported vote is stronger than the

relation between these attitudes and the respondent's actual vote.⁶

Table 3 tests whether the relation between these political attitudes and vote overreporting is completely filtered through the "expectation of voting" variable. Panel A shows the relations between vote overreporting and the measures of political attitudes among respondents who stated in the pre-election survey that they expected to vote in the November election. Panel B shows the relations among respondents who stated that they did not expect to vote.

Vote expectation is a powerful filter for the effects of general political attitudes on vote overreporting. The relations in Panel B are all very weak, and only two of the 14 tau_c coefficients are statistically significant at the .05 level. Equally striking are

Table 3. Validated Nonvoters Who Said They Voted, by Measures of Respondents' Motivation to Vote and Whether Respondents Said They Expected to Vote, 1964, 1976, and 1980

	1964			1976			1980		
Voting Expectations and Attitude Measures	Low	Medium	High	Low	Medium	High	Low	Medium	High
	(in percentages)			(in percentages)			(in percentages)		
<i>A. Nonvoters Who Said That They Expected to Vote</i>									
External Efficacy	43.5 (46)	47.4 (78)	56.9 (51)	49.7 (165)	57.8 (116)	65.3 (185)	41.6 (89)	48.6 (72)	65.0 (80)
Tau _c		.109			.146**			.211**	
Partisan Strength	33.3 (15)	51.7 (87)	49.3 (71)	52.9 (70)	54.4 (298)	68.0 (102)	30.6 (36)	50.0 (148)	66.7 (63)
Tau _c		.035			.100*			.224**	
Citizen Duty	—	—	—	39.6 (75)	52.8 (188)	66.6 (208)	40.0 (40)	47.3 (93)	58.3 (115)
Tau _c					.206**			.148*	
Concern for Outcome	42.6 (54)		51.8 (112)	54.2 (183)		59.8 (274)	51.5 (97)		51.4 (144)
Kendall's tau _b		.072			.061**			— .002	
Interest in Campaign	46.3 (41)	47.3 (74)	53.4 (58)	42.3 (91)	57.5 (204)	63.9 (175)	29.0 (62)	51.6 (126)	72.9 (59)
Tau _c		.062			.155**			.324**	
<i>B. Nonvoters Who Said That They Did Not Expect to Vote</i>									
External Efficacy	7.8 (51)	0.0 (63)	3.4 (29)	3.5 (200)	3.9 (103)	1.3 (78)	1.1 (94)	1.7 (58)	2.3 (44)
Tau _c		-.050			-.013			.010	
Partisan Strength	0.0 (32)	6.0 (83)	0.0 (26)	0.9 (110)	3.7 (232)	6.4 (39)	1.6 (62)	0.8 (119)	5.9 (17)
Tau _c		.006			.033*			.009	
Citizen Duty	—	—	—	2.6 (155)	2.1 (141)	6.0 (84)	0.0 (55)	1.2 (85)	3.6 (55)
Tau _c					.022			.029	
Concern for Outcome	2.7 (74)		3.2 (63)	3.4 (239)		3.1 (131)	1.6 (127)		1.7 (59)
Kendall's tau _b		.013			-.012			.001	
Interest in Campaign	2.9 (70)	4.4 (45)	3.4 (29)	1.0 (155)	3.4 (174)	7.6 (60)	0.9 (114)	3.1 (64)	0.0 (19)
Tau _c		.009			.045**			.011	

Note: Figures in parentheses are base Ns for the percentages immediately above.

*Statistically significant at or below $p = .05$.

**Statistically significant at or below $p = .01$.

the extraordinarily low levels of vote overreporting among those who did not expect to vote. In contrast, in Panel A, not only are the levels of vote overreporting much higher than those in Panel B,

but most of the political attitude measures have moderate and statistically significant relations with vote overreporting. Prior attitudes appear to be partially filtered through the vote expectation measure: 11

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of the 14 tau coefficients in Panel A are weaker than the bivariate relations between attitudes and overreporting (Table 2). Analogous results obtain when we examine the relation between vote overreporting and education while controlling for vote expectation.

Race, Sex, Age and Vote Overreporting

Given the conclusions of previous research, it is important to examine whether our measure of vote overreporting shows large differences in race. Although a higher proportion of black nonvoters than white reported that they voted, only in 1976 is the racial difference in overreporting statistically significant. Even for 1976, these differences should not be overinterpreted. Differences in overreporting related to race may be partly an artifact of error in the vote validation process (Abramson and Claggett, 1984), of differences in the quality of interviewing, or of sampling error or differential response rates of blacks and whites.⁷ Earlier conclusions that blacks are much more inclined than whites to report falsely that they voted (see especially Hill and Hurley, 1984; Sigelman, 1982) are not supported when the appropriate population at risk is analyzed.

In 1964, men were more likely to overreport voting than women, but this difference was eliminated by 1980, probably because of the virtual elimination of sex differences in reported voter turnout. The pattern of vote overreporting by age follows the trajectory of actual voting by age. The youngest and oldest nonvoters are the least likely to overreport voting.

Multivariate Logit Regression

We use logit regression analysis to summarize the effects of education and support for civic norms on the probability that nonvoters will falsely claim that they voted, and to determine whether these effects remain after the respondent's age,

sex, and race are taken into account. To define the dependent variable (false reporting), actual nonvoters who say they voted are coded 1, and those who say they did not vote are coded 0. The analysis estimates the probability that an actual nonvoter will claim to have voted.

Interpreting the effect of differences in individual logit regression coefficients is not straightforward. However, as in ordinary least-squares (OLS) regression, larger logit regression coefficients imply larger effects on the dependent variable than smaller logit regression coefficients. Also, the test of significance of individual logit regression coefficients is analogous to that used in OLS regression (Aldrich and Nelson, 1984, pp. 54-55).

The test for the goodness of fit of the model as a whole is a test of the significance of the difference between the chi-square for the specified model and the chi-square for the model that includes only the grand mean. Since there is no direct analogue to the R^2 statistic of OLS regression, we report a pseudo- R^2 approximation proposed by Aldrich and Nelson (1984, p. 57).

Model Specification

For each National Election Study, we first specify a "base model" in which the dependent variable, false reporting (*FR*), is a function of whether the respondent declared that he or she *expected* to vote (*EXPECT*, coded 1 or 0) and of the respondent's *education*, expressed by the following dummy variables: "Completed High School" (*HSED*), "Some College" (*SCED*), and "Completed College" (*CCED*).⁸ Since education only affects the propensity to overreport voting among respondents who declared before the election that they expected to vote, we specify the joint effects of education and expectation as interaction terms. The base model is:

$$\text{logit}(\text{FR}) = b_0 + b_1\text{EXPECT}$$

$$\begin{aligned} &+ b_2(EXPECT \times HSED) \\ &+ b_3(EXPECT \times SCED) \\ &+ b_4(EXPECT \times CCED) + e. \end{aligned} \quad (1)$$

All variables in the base model are included in each regression equation. As with education, all other independent variables are expressed as interactions with vote expectation. Recall that we interpret the expectation variable as an indicator of the saliency of voting to the respondent.

The results of the logit regression are shown in Table 4. Three models are specified for each election year. In each year, the base model as a whole is statistically significant at $p < .001$. In each year also, the main effect of the *EXPECT* variable is significant, as is the interaction between *EXPECT* and at least one of the education categories.

The second model for each year adds to the base model terms reflecting race, sex, and age, labelled collectively as "covariates." Based on preliminary analysis, the effect of age is expressed as an interaction term between *EXPECT* and a dummy variable that takes the value of 1 if the respondent was between age 18 and age 30, and the value of 0 if the respondent was older. The 18-30 year-olds stand out because of their low propensity to overreport voting, despite their high level of education.

Three measures of political attitudes are included in the third equation for each year: (1) high political interest (*EXPECT* \times *HIGH INTEREST*), (2) high external political efficacy (*EXPECT* \times *HIGH EFFICACY*), (3) high citizen duty (*EXPECT* \times *HIGH DUTY*). When all of the political attitude variables examined in the cross-tabulations are entered simultaneously in the regression equation, none of them is statistically significant in the logit regression. This is because of multicollinearity, and because there is a large penalty in degrees of freedom from including non-

significant variables in logit regression. Although some alternative combinations of variables might have worked nearly as well, the three variables that were included were the ones that showed the strongest effects of the political attitude variables in combination with the base model and covariates.

Discussion

In 1964, neither the three covariates nor the additional political attitude variables improves the goodness of fit of the model. In 1976 and in 1980, however, both the covariates and the political attitude variables add significantly to the goodness of fit. Judging from the pseudo- R^2 , the base model variables and the base model plus covariates do better in accounting for overreporting in 1976 and 1980 than in 1964. Even more importantly, additional motivating factors come into play in later elections. Although the best individual predictor differs between 1976 and 1980, in each succeeding election the effects of the political attitude variables become stronger. This is consistent with the increasing importance of political efficacy and strength of partisanship in accounting for voter turnout in recent presidential elections (Abramson and Aldrich, 1982).

Conclusion

We have shown that the tendency to overreport voting is related to respondent characteristics. Thus, measures of the relation between respondent characteristics and self-reported voting will overestimate the strength of the relation between the dependent and independent variables.⁹ Therefore, Sigelman's (1982) and Katosh and Traugott's (1981) conclusions about the comparability of analyses of the social and political correlates of voting using the validated and self-reported voting data are not sound for all types of analyses. Wolfinger and Rosen-

Table 4. Logit Regression for Base Model, Base Model with Covariates, and Model with Political Attitude Variables, 1964, 1976, and 1980

Variables	1964			1976			1980		
	Base Model ^a	Base Plus Covariates	Base Plus Covariates and Political Motiv.	Base Model	Base Plus Covariates	Base Plus Covariates and Political Motiv.	Base Model	Base Plus Covariates	Base Plus Covariates and Political Motiv.
Constant	-3.273* (7.30)	-3.273* (7.30)	-3.273* (7.30)	-3.403* (11.60)	-3.401* (11.88)	-3.401* (11.88)	-4.179* (7.31)	-4.179* (7.31)	-4.179* (7.31)
<i>Base Model Variables</i>									
Expected to Vote	2.810* (5.16)	2.689* (4.50)	2.695* (4.23)	3.494* (9.82)	3.125* (8.03)	2.380* (5.37)	3.594* (5.51)	4.015* (5.72)	3.158* (4.06)
Expected to Vote X Completed College	1.715* (2.00)	1.838* (2.10)	1.845 (1.93)	.678 (1.54)	.962* (1.98)	.414 (.77)	2.541* (2.28)	3.649* (2.93)	2.857* (2.09)
Expected to Vote X Some College Education	.057 (.09)	.188 (.30)	.192 (.28)	.508 (1.31)	.975* (2.26)	.547 (1.19)	1.231* (2.38)	2.271* (3.31)	1.977* (2.65)
Expected to Vote X High School Education	.955 (1.94)	.996 (1.96)	.997 (1.90)	.114 (.38)	.541 (1.59)	.390 (1.09)	.652 (1.42)	1.075* (2.04)	1.034 (1.82)
<i>Covariates</i>									
Expected to Vote X Black	—	1.038 (1.67)	1.044 (1.61)	—	.995* (2.56)	.974* (2.38)	—	-.351 (.64)	-.462 (.73)
Expected to Vote X Male	—	-.125 (.29)	-.126 (.29)	—	.495 (1.77)	.506 (1.72)	—	-.195 (.45)	-.558 (1.14)
Expected to Vote X Age 18-30	—	-.106 (.21)	-.107 (.20)	—	-.721* (2.39)	-.556 (1.76)	—	-2.034* (3.57)	-1.958* (3.16)
<i>Political Attitude Variables</i>									
Expected to Vote X High Interest in Politics	—	—	-.016 (.03)	—	—	.547 (1.84)	—	—	1.393* (2.87)
Expected to Vote X High External Political Efficacy	—	—	.003 (.01)	—	—	.434 (1.34)	—	—	1.126* (1.98)
Expected to Vote X High Citizen Duty ^b	—	—	—	—	—	.942* (3.25)	—	—	.414 (.87)

Table 4 (continued)

Variables	1964			1976			1980		
	Base Model ^a	Base Plus Covariates	Base Plus Covariates and Political Motiv.	Base Model	Base Plus Covariates	Base Plus Covariates and Political Motiv.	Base Model	Base Plus Covariates	Base Plus Covariates and Political Motiv.
Statistical Significance of Specified Models									
Comparison with Constant (grand mean)									
Chi-square	80.6	83.7	83.7	252.4	267.6	285.0	131.0	148.2	165.2
d.f.	4	7	9	4	7	10	4	7	10
p-value	< .001	< .001	< .001	< .001	< .001	< .001	< .001	< .001	< .001
Comparison with Base Model									
Chi-square	—	3.1	3.1	—	15.2	32.6	—	17.2	34.2
d.f.	—	3	5	—	3	6	—	3	6
p-value	—	.39	.69	—	< .001	< .001	—	< .001	< .001
Comparison with Base Model Plus Covariates									
Chi-square	—	—	0	—	—	17.4	—	—	17.0
d.f.	—	—	2	—	—	3	—	—	3
p-value	—	—	1.00	—	—	< .001	—	—	< .001
Total N ^c	234	234	234	470	470	470	313	313	313
Residual N	229	226	224	465	462	459	308	305	302
Aldrich-Nelson ^d									
Pseudo-R ²	.256	.263	.263	.349	.363	.377	.295	.321	.345

^aThe t-ratios are given in parentheses. The tests are based on a two-tailed test of the t-ratio (ratio between the regression coefficient and its standard error).

^bThe citizen duty items were not included in the 1964 National Election Study of the University of Michigan Survey Research Center.

^cLike the cross-tabular analysis, the LOGIT regression analysis for 1976 is based on weighted data. For the sake of comparability, the logit regression is performed only on cases for which there are no missing data on any of the variables in the three equations shown for the given year.

^dThe pseudo-R² statistic used here is defined by Aldrich and Nelson (1984, p. 57).

*Significant at the .05 level.

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stone's (1980, p. 118) declaration that "misreporting is not strongly related to demographic characteristics, although it is a bit more common among the very young and the very poor" is incorrect.

Our results have theoretical implications for the study of American political attitudes. By measuring overreporting of voting as individual rather than aggregate political behavior, the pattern of overreporting is shown to be consistent with our understanding of the individual motivational bases of political participation. This approach also gives a more accurate picture of the social bases of support for regime norms.

There is an analogy between our results and studies of such values as support for civil liberties and tolerance of minorities, nonconformists, and political extremists. Several scholars have pointed out that since respondents who are more highly educated are more likely to be aware of the "correct" or socially approved responses, their responses may stem from the desire to provide socially approved answers. Jackman and Muha (1984) argue, however, that higher-status respondents are not giving superficial answers that do not reflect a behavioral commitment; instead, higher-status respondents give answers to questions dealing with social issues that are consistent with their class interests and that express their satisfaction with the status quo. Both interpretations fit our results, which show that more highly-educated respondents are more likely than less-educated respondents to try to appear to be in conformity with the regime norm of voting, even when their actual behavior is inconsistent with this norm.

Notes

The data for this research were made available by the Inter-University Consortium for Political and

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1. We recode the vote validation data in the SRC studies following the procedures reported in Abramson and Claggett (1984). We do not include the 1978 SRC post-election survey, since it provides no measure of vote expectation.

2. The Kendall's tau, coefficients between whether or not respondents said they voted and whether or not they actually voted is .784 in the 1964 SRC election study, .725 in the 1976 study, and .778 in the 1980 study.

3. Abramson and Claggett (1984) refer to racial differences in the percentage of nonvoters who acknowledged they did not vote, but this is not the main dependent variable in their analysis.

4. For further discussion of the importance of defining the population at risk of misreporting voting, see Anderson and Silver (1986) and Silver, Anderson, and Abramson (1985).

5. The recoding and construction of these measures from the SRC survey results follow standard procedures in previous research, and are described in detail elsewhere (Silver, Anderson, and Abramson, 1985).

6. We compared all pairs of tau coefficients between the two measures of whether the respondent voted and each of the political attitude measures. In 13 of the 14 paired comparisons the coefficient between the attitude and self-reported vote is larger than the coefficient between the attitude and the validated vote.

7. This argument is consistent with Weiss's (1968-69) study of welfare mothers, which showed that those who were more involved in the community were more likely to overreport voting. There is also evidence that response rates in SRC surveys have declined in recent years, particularly in large cities (Steeh, 1981). For further discussion of differences regarding race, see Silver, Anderson, and Abramson (1985).

8. "Some High School or Less" is the omitted category.

9. This point was made by Weiss (1968-69, pp. 627-28), but seems to have been overlooked by later researchers.

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REVIEW ESSAY

Texts in Context: Revisionist Methods for Studying the History of Political Theory.
By David Boucher. (Dordrecht: Martinus Nijhoff, 1985. Pp. vii + 280. \$39.90.)

The Status and Appraisal of Classic Texts: An Essay on Political Theory, Its Inheritance, and the History of Ideas. by Conal Condren. (Princeton: Princeton University Press, 1985. Pp. xiii + 303. \$27.50.)

The study of the history of political theory largely defined the subfield of political theory in American political science for well over half a century, and it was an integral aspect of the discipline's vision of the scientific study of politics. During the behavioral revolution, and evolution, the discipline and its subfield engaged in a bitter quarrel. The post-behavioral era has been marked by a detente. While the history of political theory remains a sub-subfield of political science, it has little to say to the mainstream of the discipline, which, in turn, has afforded it limited cognizance and concern. Some have argued that such a divorce, or at least a state of permanent separation, is the preferred condition, and one that is of mutual benefit—most have at least been comfortable with the estrangement. It might also be argued, however, that each has been impoverished both by the loss of a dimension of critical exposure and by a narrowing of their field of action.

These are complicated issues, but even to broach them requires a more detailed grasp of what has happened to the study of the history of political theory since it cut loose from issues defined by political science and by the conflict with behavioralism. During the past 15 years, the study of the history of political theory has been steadily transformed. The principal development that contributed to changing the contours and internal constitution of the field was the appearance of a critique that alleged a failure on the part of past research to be adequately historical, and which has been accompanied by the emergence of a body of research

that in various ways and degrees has been associated with this critique. This has been conventionally dubbed the "new history of political theory" or, more ambiguously, the "new historicism."

Boucher's and Condren's books represent and address the transformation in question, and text and context in this case are inseparable. To situate this work properly would require a definitive account of the study of the history of political theory during the past three decades, and this, in turn, would require nothing less than writing a history of academic political theory; but it is important to sharpen our sense of the character and location of this literature.

The appearance of the new history has been associated prominently with the criticisms advanced by Quentin Skinner and J. G. A. Pocock, beginning in the late 1960s, and with the historical research which they and individuals of a similar persuasion, have accomplished. Accounts of that persuasion have been related in many places, including the works reviewed here, and an extensive—and maybe excessive—critical literature has evolved around this work. But the very fact that the issues have occasioned an amount of methodological discussion that sometimes seems out of proportion to the research programs involved signals some underlying concerns about the current status of political theory in general. A number of factors are relevant in explaining the strong reactions that this work has evoked. Most obvious is its trenchant critique of past scholarship, but the crux of the controversy lies deeper and involves a more subtle issue. It is not simply that the critics attacked past work as failing to achieve historicity, but that much of this work did not understand itself as mere history, taking its relationship to politics as generic and fundamental to the field.

The story of the new history has been told in different ways, depending on the critical or apologetic note being sounded. There has been

a tendency, however, both among critics and defenders, to picture an old and a new genre, and to consider the virtues, or deficiencies of each. In some respects this approach is reasonable, but it also fails to touch many of the broader and deeper issues involved. Political theory as a whole, and even the history of political theory, was not—despite its scholarly and educational paradigms (or totems)—a highly integrated field. Furthermore, no authoritative disciplinary matrix has arisen from the disintegration of past practices and the appearance of new modes of research. Although some of the regulative assumptions of earlier forms of study have been theoretically devastated, they, and their literary residue, persist alongside methodological and substantive works that reject the old aims and premises and articulate and represent new ones.

Since the "history of political theory" (philosophy, ideas, and thought) refers to both a subject matter and its investigation, there is an inherent ambiguity in the designation. There is another dimension of ambiguity, however, that stems from the fact that in its latter mode it is characteristically used both to talk about a certain analytically distinguishable kind of research and to refer to the practice of a particular discipline or subdiscipline. It is not uncommon for works from a variety of fields to be subsumed as contributions to the history of political theory, and for the latter in turn to be understood as part of the history of ideas or an aspect of intellectual history. This is quite innocuous, unless these categorical terms are reified and it is assumed that there is, in fact, a distinct conventional practice such as the history of ideas, of which the history of political ideas, as a specific field, is a tributary. This problem has appeared from time to time in the methodological arguments of the new historians, who seek to legislate what they claim to be a truly historical form of inquiry. However, the distinction is also important for understanding the general outlines of the evolution of the history of political theory as a field of study.

If we speak generically about the study of the history of political theory or the history of political ideas, we can reasonably claim to discern instances in the early to mid-nineteenth century in various fields of scholarship and forms of political commentary. Although the

actual historical relationship between such analytically specified forebears and later, more disciplined forms of inquiry is complex, the connections are significant. No matter where we locate its ancestors and relatives, either categorically or historically, there is an important sense in which the history of political theory did not emerge as a distinct genre and field of study until it found a home in American political science. Beginning in the late 1800s, but most significantly taking form in the work of William Dunning at the turn of the century, and extending through the period of George Sabine's extraordinary influence, it had a special relationship with this discipline. It would not be an exaggeration to suggest that the history of political theory as we know it, both as a subject matter and a field, has been an invention of American political science.

From the late nineteenth century through the 1940s, the subfield of political theory in political science was essentially the study of the history of political theory. Despite some instances of conflict, there was, for the most part—and in the view of such major proponents of a scientific study of politics as Charles Merriam—a fundamental complementarity between this form of historical inquiry and the pursuit of a theoretical and practical science of politics. However, a fundamental transformation in this relationship began during the rise of the behavioral movement.

The behavioral revolution largely defined itself in terms of a rejection of the study of history of political theory, and posed a fundamental antagonism between historical and scientific theory. This conflict was more complex than is often assumed. Although, in retrospect, this shift has often been viewed as natural and incremental, it was in fact sudden and anomalous. There were many reasons—both internal and contextual, pragmatic and principled—for the behavioral critique of historical political theory. It was, in an important respect, an intellectual and (in a lexical sense) conservative reaction to new influences in the literature of political theory that challenged the dominant values of both American political culture and the American science of politics.

Beginning in the late 1930s and early 1940s, the philosophical histories of émigré scholars such as Leo Strauss, Hannah Arendt, Eric Voegelin, and Herbert Marcuse—as well as

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certain sympathizers such as John Hallowell—represented, at least implicitly, a fundamental break with the assumptions that had governed the field and assured the unity of historical and scientific approaches to theory. The point is not that earlier histories of political theory “from Plato to NATO” (Condren, p. 253) were not philosophically inspired and shaped, but rather that the new literature, whatever its ideological bent (which ranged from Christian to Marxist), implicitly, and often explicitly, confronted and contradicted some of the basic beliefs and commitments that had traditionally informed the discipline.

Through the 1940s, the textbooks and scholarship in the history of political theory had been devoted largely to telling the story of the evolution of the idea and institution of liberal democracy and suggesting—even avowing—that it was underwritten by scientific understanding. This was the meaning conferred on the great tradition that was both constitutive of and constituted by the classic texts that had become the basic subject matter of historical interpretation and evaluation. By the early 1950s a different perspective, forged in the context of European philosophy and the experience of totalitarianism, had, through the work of the refugees, begun to gain a voice within the discipline. Although still not highly visible in the literature, its claims presented a different version of the tradition of political theory—one that contrasted sharply with dominant assumptions and particularly with the increasing commitment of the discipline to pure science and a distinction between empirical and normative inquiry.

The account of the tradition was transformed into one of decline culminating in a crisis of modernity rooted in the pathology of modern political thought, particularly those ideas that gave rise to scientism and liberalism. This alleged intellectual and political crisis of the West was reflected, it was claimed, not only in the rise of totalitarianism, but in the antihumanistic positivism of contemporary social science and the practical and theoretical deficiencies of liberalism to which it contributed. The basic givens of the liberal/pragmatic consensus were challenged by transcendental claims and world-historical visions that both in style and substance contradicted the images of political science and political theory that had prevailed for more than half a century.

Before behavioralism had selected the history of political theory as a contrast model, the foundations of the critique of behavioralism had already been laid down. It was not that political scientists and earlier historians of political theory had not acknowledged a modern crisis reflected in the war and other aspects of social upheaval, but rather that they believed it was an aberration that would be dissipated through historical progress assured by and manifest in science and liberalism. Behavioralism was not, at least in principle, as revolutionary as its proponents claimed. There was no element of the behavioral credo that had not been articulated by earlier representatives of the discipline such as Merriam. Nor was it the doctrine of complacency that some of its opponents suggested. It was aggressive in maintaining and defending the liberal/scientific faith.

The behavioral claim that the study of the history of political theory had failed to contribute either to the scientific understanding of politics or to relevant value theory (e.g., David Easton) served more to segment than initiate the polarities. Indigenous historians, long attached to the traditional liberal/pragmatic goals of political science, became tarred with the same brush as those who espoused the still somewhat elusive arguments of the new wave of European thought. For the next two decades, the critique of behavioralism was a principal commitment of the literature associated with the history of political theory, and mainstream political science increasingly defined itself in terms of its distance from historical and normative theorizing. This conflict was marked by ultimately untenable claims about both science and the great tradition, but it largely came to define the discourse of political theory in political science.

There were many factors that contributed, by the early 1970s, to the emergence of political theory as an autonomous interdisciplinary field including elements of political science, philosophy, and history. One cause was the increasingly uncomfortable position of the history of political theory and allied concerns in the discipline of political science as constituted by the behavioral hegemony. Despite tensions within the camp of the history of political theory, Sheldon Wolin (1969) spoke for a generation when he defended “political theory as a vocation” against what he charged was the antihistorical and apolitical

scientism of behavioral political science. Although it has been tempting to suggest that political theory was ostracised by political science, it is necessary to note the degree to which separatism had been rooted in the basic sentiments of the literature that had come to dominate this subfield.

By the late 1970s, the critique of behavioralism had essentially passed into other hands, and the emerging post-behavioral climate was ecumenical and tolerant of diverse modes of theorizing. The history of political theory continued to find a home in political science, but many of those once most actively involved in criticizing behavioralism increasingly became distanced from issues in the discipline and reflected concerns defined by the wider and burgeoning field of political theory.

It would be a mistake to suggest that the appearance of the internal methodological critique of the study of the history of political theory that began to emerge in the late 1960s can be explained simply as a result of the disengagement of the field from political science. Many of the principal sources, such as the work of Skinner, had never been closely tied to American political science, while some criticisms of past work were in fact rooted in the discursive context of American political science. Apart from the intrinsic significance of the issues that were raised and the power of the arguments advanced, much of the impact of this literature must be attributed to this disengagement.

As long as the study of the history of political theory was shaped by its relationship to political science, there was little self-reflection and impetus for critical examination. Some of the most extravagant and vulnerable claims about the meaning and significance of the classic literature and the activity of studying it were generated under these conditions. Autonomy occasioned self-consciousness about historicity and about the assumptions that had informed past endeavors. The claims fell upon a field that had been absorbed with "political" and disciplinary rhetoric. Now, separated from the context of the debate about behavioralism, the study of the history of political theory was—maybe almost inevitably—forced to address issues regarding the nature of its own activity and its subject matter.

One of the characteristics of the study of the history of political theory—including many of

the earliest examples of the genre, and certainly beginning with its more overtly ideological forms in the 1930s—was that often neither the concern nor the approach was essentially historical. This is not to suggest that the criteria of "historical" are uncontested, or that the new history had any special authority to specify them or success in meeting them, but simply to indicate that much of the literature in the field, despite its historical form, belonged to a distinct scholarly/rhetorical genre of political/philosophical analysis and commentary. Often neither the behavioralists who criticized it as historical antiquarianism nor the new historians who deemed it inadequate intellectual history gave sufficient attention to its practical intention and purpose, and to its actual form and context.

Although there is opportunity and ground for a critique of the ideological premises and implications of the new history, it represents a decisive chapter in the academization and professionalization of political theory. This is really what much of the controversy has been all about, even though disguised in terms of arguments about method and proper subject matter. Although the study of the history of political theory has long been basically an academic activity, its understanding of itself, both before and after the transformation effected by the émigrés, reflected its mid-nineteenth century origins and contemporary aspirations as a contribution to, and even a form of, political discourse.

The new history, despite more continuities with the past than it might wish to acknowledge, represents a distinct break in terms of its institutional separation from political science; its attachment to the interdisciplinary field of political theory; its methodological commitment to a "truly" historical account of political ideas, traditions, and texts; and its substantive research programs. In its claims there are intimations of significance beyond the sphere of accurately recounting the past, but its self-defined goal is to separate understanding and critique and give priority to the former. This is often accompanied by the argument that relevant critique must be preceded by an accurate understanding of what is being criticized, but for the most part the emphasis has been on the theory and practice of making political theory historical, as opposed to practical and philosophical.

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This, as well as the methodological emphasis, is not an uncommon pattern in the development of academic disciplines, and it offers some interesting parallels with the evolution of political science as a whole. The study of the history of political theory, despite its Europeanization, in some respects sustained the sentiments of the pre-behavioral political science that favored practical political engagement and sought an image of world-historical significance. There are hints (and even explicit claims) that the new history, like the policy turn in post-behavioralism, having pursued the distinction between explanation and application, may be toying with the issue of how once again to join what has been deemed separate and equal. As with the case of political theory in general, questions about the relationship of the new history to both politics and political science must be a matter of concern, particularly in view of the break with past commitments and assumptions. In this respect, the two books under consideration do not offer much explicit direction, but they do provide a basis for talking about the issues.

One perspective for relating these works is to view Boucher's book as a descriptive, explanatory, and critical account of the methodological claims of the new historians, and to approach Condren's book as an attempt to refine and extend some of the critical themes associated with this literature. This is a somewhat limiting approach, but it largely reflects what the authors see themselves as about, taking account of the fact that the treatments are basically complementary and markedly different in style and scholarship.

Boucher has provided a useful and relatively exhaustive account of the work of some of the principal individuals associated with the new history. Greenleaf, Pocock, and Skinner are discussed in three consecutive chapters. The arguments of John Dunn, which Boucher believes are substantially the same as those of Skinner, are also given considerable attention. Although Boucher has embellished and extended some of the criticisms and analyses that have been directed toward this literature, little in the book can be designated as original or of great value to those already familiar with the literature or involved in the dialogue. Its principal virtue is that it presents a consolidated account that should be very helpful to individuals who wish to gain a comprehensive

sense of the theory and practice of the new history, its intellectual patrimony, and its general relationship to the field of the history of political theory. Although the basic posture of the book is critical and takes account of the considerable body of critical literature that has developed around the arguments of the new history during the past 15 years or so, Boucher does not deal either particularly well or fully with this material and its relationship to his claims.

The principal scholarly contributions of Boucher's book include sorting out the differences and similarities between the arguments of individuals such as Pocock and Skinner, who are too often lumped together; extending the discussion of Greenleaf's work, which has received less attention; emphasizing prominently Michael Oakeshott and his relationship to this literature; extending the analysis of the concept of tradition, which has been so central to the issues; and providing a valuable account of British Idealism and the philosophy of history and its relationship to the new history. The discussion of idealism is a little disappointing. While this philosophical tradition is obviously important, since, as the author suggests, "it informs and inspires much of the recent methodological literature" (p. 39), the discussion of Collingwood, Oakeshott, and others is juxtaposed rather than related in an explanatory way to the arguments in question. There are similar difficulties with somewhat more truncated treatments of philosophical hermeneutics and other issues.

Boucher argues that the appearance of the new history and its "preoccupation with method" was both a reaction to "orthodoxy" in the field regarding the how and why of studying past texts, and a response to those who, during the 1950s and 1960s, either deprecated the study of the history of political theory or lamented, from various perspectives, the "decline" of the field. Despite some theoretical differences, the arguments of Greenleaf, Skinner, and Pocock—all practicing but theoretically inclined historians—converged in the thesis that it was necessary to become methodologically self-conscious and to seek an understanding of past texts in their historical—intellectual, linguistic, and social—context. Although this general characterization is defensible and accurate in many respects, Boucher devotes a less than satisfying chapter

to attempting to relate this work to the development of the Anglo-American study of the history of political theory.

The arguments of the new historians are, as Boucher emphasizes, not only closely related to the Idealist school, but even more immediately and obviously to the work of Wittgenstein, Austin, Kuhn, and post-positivist work in philosophy and social science. Such influence is apparent in claims regarding issues such as the need to situate texts in the world of ideas to which they originally belonged; the emphasis on the identity or parallel between ideas and language; the defense of the autonomy of historical understanding (*vis à vis* science); and the priority of objectivity achieved through avoiding anachronism and eschewing, at least initially, philosophical and ideological suppositions and concerns in favor of recovering an author's intention and the original meaning of the work. Boucher is not at his best in dealing with the character of such intellectual connections, but one reasonable summary conclusion that his discussion might imply is that the new historians have drawn upon more recent philosophy in an attempt to rehabilitate or reconstitute some of the more characteristically vulnerable and contested claims about historical understanding associated with the Idealist tradition.

To the extent that the book has a central theme, it is the claim that while the arguments of Greenleaf, Pocock, and Skinner represent successive moves away from the concerns and philosophical attachments of previous historians of political thought, in the end the authors "fail to maintain what they assert" (p. 5), or to achieve the standard of historicity to which they aspire. While Greenleaf emphasizes the importance of traditions as an object of study and the appropriate context for interpreting a text, Pocock stresses the constitution and evolution of paradigmatic languages, to the extent that particular texts are important only insofar as they represent or transform such languages. Skinner's intentionalist approach, in turn, focuses less on the linguistic context than on what a particular author says and does with language.

This kind of distinction may have some heuristic value as an organizing perspective, but has limited use as a basis for comparing these authors and understanding the differences between their claims. Boucher does

well when he describes the individual positions, but falls into difficulties when he attempts to join the arguments. The charge that the new history has failed to practice what it preaches is a common one, and Boucher's discussion neither adds to nor adequately conveys the existing literature on the matter. When he engages some of the more complex philosophical and theoretical issues regarding such matters as relativism and objectivity, linguistic conventions and intentionality, and the relevance of hermeneutical arguments such as those of H-G. Gadamer and Paul Ricoeur to the issues at hand, he seems less than completely at home.

Boucher concludes that the principal contribution of these historical revisionists is that they have created a new self-consciousness among students of political thought about the methods and assumptions to which they have hitherto subscribed. Yet, he suggests, this work has not had the impact its proponents had hoped. In some sense, there is no doubt that both these claims are correct, but Boucher does not explore either assertion very fully. He points to some of the work that professes to have been inspired by the revisionists, but fails to discuss much of the most significant research that has been generated within what could be construed as the intellectual climate that the new history has helped create.

Boucher correctly points out that it is difficult to ascertain what kind of historical practice can be accommodated under the methodological principles advanced by the revisionists, just as it is difficult to determine when the practitioners actually, if ever, live up to the principles adduced. However, he does not explore the general problem of the relationship between philosophical methodology and research involved here, and what parts such metatheory plays, and has played, in the study of the history of political thought. He concludes that the impact of the revisionists has not been as radical as often claimed, and that "the relationship between the new and the old histories of political thought is not one of rejection and replacement, but one of continuation and refinement" (p. 258). In the end, "in making a commitment to understand texts historically, we find that we have committed to very little" (p. 271). These are relatively safe conclusions, but they do not carry us very far in assessing the work of the revisionists and its

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relationship to past and future scholarship.

Ironically, what the revisionists have often failed to take into account when castigating past research was its intellectual context and the intentions of authors. They have treated it as bad history according to their criteria, and even assumed that earlier authors had simply failed to live up to universally specifiable standards of historical knowledge. To some extent this has probably been more a rhetorical strategy than an oversight, and Boucher probably overemphasizes their claim to historical purism by suggesting that they view criticism and philosophical purposes as illegitimate. It would be more accurate to say that they insist on the separation of historical understanding and explanation from normative claims about subject matter. There is no doubt that they have fostered what might reasonably be construed as a more historical (academic) attitude. This suggests definite questions about the relationship between historical and political discourse and about the role of political theory in the university, which, today as well as in the past, has characteristically understood itself as more than merely a professional scholarly activity.

Condren provides little help in addressing this problem, since, like the new historians' criticism of earlier work, he pays scant attention to its actual purpose and self-image. Yet the myths and rhetorical moves that characterized much of this work and its "historical" claims cannot be absolved simply because its authors understood themselves as engaged in such relatively cosmic endeavors as seeking political truth and the reformation of modern social science. Herein lies one of the fundamental dilemmas of academic political theory: to attempt, either consciously or unconsciously, to speak politically in the language of scholarship, or to conduct scholarship according to the form of political argument. The outcome is usually failure at both.

Condren understands himself as engaging in "a study of academic mythologies—in part, a professional act of deconstruction" (p. ix) aimed at an examination of the "institutionalized study of a handful of classic texts and the rhetoric of their promotion" (p. x). He is eminently successful in exorcizing the vestiges of the dubious claims that for so long have governed the history of political theory, but that require exposure if there is to be any

serious consideration of the reconstitution of this field so that it can deal seriously with its relationship to politics on the one hand, and political science and philosophy on the other.

One of the difficulties with the methodological critiques advanced by individuals such as Skinner was that they were intricately bound up with, although not limited to, the defense and propagation of a certain kind of research program. This methodology was not in fact the articulation of a method as much as the strategic deployment of philosophy in the service of certain modes of research. This is one reason why so many have noted a disjunction between the principles advanced and the history practiced. It would be fair to say that, despite the fact that he sees this work as a "clearing operation" to be succeeded by better history, Condren—at least here and I suspect by inclination—is much more the pure methodologist. This is far from an unproblematic position to occupy, but he is sensitive both to the fact that *homo methodologicus* operates in a "dubious region" (p. 7), and to the many paradoxes and ironies involved in a vocation that tends "to hover (Socratically in a basket?) at the nexus of extrinsic and intrinsic inquiry" (p. 20), lacking unequivocal membership in any community.

Condren has written an uncommonly literate, erudite, and even entertaining book that manifests a thorough knowledge both of the history of political theory as a field of study and of its subject matter. The discussions are also firmly grounded in philosophy and literary criticism. It is a significant demonstration that tough-minded analysis is alive in the field and can be executed with grace and wit. There are times when the book seems excessively annotated and cluttered with asides, but the breadth and depth of scholarship is impressive. The author is in complete command of his subject. In the course of illustrating some points about textual exegesis, he also offers some compelling interpretations of portions of the work of such classic authors as Plato, Marsilius, Machiavelli, Hobbes, and Locke. In the end, Condren is not able to conjure up much in the way of cures for the disabilities in the field he diagnoses, but he is aware that metatheory is neither the basic repository of answers to substantive problems nor a source of automatic authority regarding the activities it brings under its gaze. There is a dimmer but

persistent sense that what is often posed here as a historiographical problem really has no historiographical answer.

This is the problem of what are, and have been, the criteria by which a text achieves the status of a classic. This turns out to be somewhat more fundamental than it might appear, since, in the case of the history of political theory, the whole enterprise has almost uniquely revolved around studying a set of texts it has designated as classics, and has identified itself in terms of its special relationship to these works. Condren's conclusion, in short, is that "beyond the level of evocative rhetoric, common standards of textual appraisal specific to political theory are chimerical" (p. 7), and that the field has rested on very shaky foundations indeed.

The problem is in part a consequence of the particularly complex and fluid structure of this community of scholarship, which has often been rife with disputation. But the two "articles of faith" that have been most peculiar to the community and most universally accepted within it have failed to yield a basis for an appraisal of the classic texts. The "issue-orthodoxy," or the idea that politics consists of a set of universal and perennial issues, that constitutes the subject matter of political theory, and the "myth of the tradition," or the belief that the classic texts are elements of a genuine historical "tradition," have come under severe attack in recent years, and Condren demonstrates that these dogmas have little power to explain why a classic is a classic.

Condren argues that it is necessary, then, to look at the more general "appraisive field" of textual analysis as a whole, to which the concept of "classic" belongs and on which the political theory community ultimately relies. This he takes to be a cluster of notions which include originality, coherence, contribution and influence, and ambiguity. Despite the somewhat unstable character of the political theory community, it does have internal elements of identity and cohesion, no matter how shaky they may appear when subject to scrutiny. However, the community (and here he plays with the political/religious analogue) also participates in a wider appraisive (ethical) structure constituted by these concepts. He treats these categories consecutively on an ascending scale of what he claims to be their rhetorical/explanatory power.

His discussion of this conceptual field often strays—but very fruitfully—beyond the literature of political theory. Although the implication is that the argument is moving in a more positive direction, the analysis of political theory in terms of its understanding and application of terms such as *originality*, *contribution*, *influence*, and *coherence* is, in fact, as destructive as the critique of the issues/tradition nexus. He concludes that "they provide little more than a superficial rhetoric of rationalizations" (p. 167). In the last third of the book, Condren turns to an excellent extended discussion of ambiguity that has more to offer as a piece of independent analysis than it does as an answer to the basic problem posed. He claims that it suggests the basis for a "reformed appraisive field" that would "provide a more viable framework for understanding the actual history of given texts in political theory than any reliance upon the established field can provide" (p. 10). Here Condren inevitably, but nobly, fails in his search for why a classic is a classic in political theory.

He claims that his typology of ambiguity provides a way of looking at the texts comprising the classic canon that—as a matter of historical fact, and contrary to the dogma of the field—"do not conform to any single conventional pattern of discourse" (p. 189). Condren argues that classic status is essentially a function of the degree to which a text offers sufficient ambiguities for exploitation as an authority, and that this, in turn, is often a consequence of the polemical context (of religious and political controversy) in which they were rooted and of their "rhetorical and ideological resonance." He stresses that his claim is not simply that these books became classics because they were "pathologically ambiguous" (p. 261), but rather that the notions of authority, exploitation, and ambiguity provide a conceptual framework for explaining why certain texts have emerged as a classical tradition from what an early historian of political thought referred to as the "promiscuous heap" of available works.

Although Condren has indicated an important characteristic of the classic texts, and possibly even a necessary condition of classic status, it is not a sufficient explanation of why they became classics. However, he has also, by this point, shifted the problem from why the political theory community considers them

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classics to why they became classics before being bequeathed to or appropriated by this community. The problems overlap, as he makes clear in his allusions to the similarities between political and academic discourse, but they are not the same. The discussion of ambiguity simply cannot carry the burden assigned to it, and Condren, in fact, seems to sense the limits of his attempt to rationalize classicalness. He notes that such an analysis cannot substitute for looking in detail at the actual history of particular texts and of the intellectual community conferring the classic status. There is no analytical answer to what is really a particular historical and, at least academically, ideological problem. His final, short answer is the best, and actually is implicit throughout: "What then gives texts classic status? At its simplest, the intellectual communities that need them: they have been fashioned as man's gods and ancestors have been in his image and likeness" (p. 284).

Condren's work is eminently successful as a deconstructive effort that extends and deepens the critique of the less than tenable basis on which much of the work in the history of political theory has been conducted. Paralleling that critique has been the appearance during the past 15 years of significant research that has attempted to disengage itself from the difficulties of those who propagated, and were mesmerized by, the spell of the great tradition and other mythologies. This work, in both theory and practice, has hardly been free from intrinsic problems, but most of the dissatisfaction has been of a different nature. It has been rooted in the fact that this work does not replace in kind what has been cast aside, and does not fulfill the expectations and purposes that in part defined the old endeavor. More specifically, it fails, for example, to absorb itself sufficiently within the traditional classic canon and to accept the premises attaching to that definition of the subject matter, and it steps aside from political commentary that ascribes to itself world-historical significance. In fine, much of the criticism has simply been

rooted in nostalgia for an obsolescent genre, and in the residue of old commitments and beliefs.

The study of the history of political theory, organized around the notion of a tradition containing and constituted by the classic canon and explaining the present, was once understood as the theoretical core of political science. During the behavioral era it was gradually displaced from that matrix, but that alienation exposed its fragility. The accompanying internal transformation, initiated by the émigrés and their increased philosophization of the subject matter, eventually, and ironically, made it alien to the very structure it contributed largely to creating: autonomous academic historical political theory. The study of the history of political theory will remain part of the scholarly and educational structure of political science, not only because of its relevance to and historical roots in that field, but simply because many of its most significant practitioners are professional political scientists. However, its intellectual evolution will continue primarily to depend, at least in the near future, on its place in the interdisciplinary field of political theory and on the kind of discussions initiated by the new history.

With regard to its relationship to politics, there is little to suggest that the study of the history of political theory is any kind of special case, despite vestigial hopes and claims. Its future in this respect is tied to more general questions about the relationship of the fields of political science and political theory to politics—that is, the general relationship between academic and public discourse. The new history has had little to say about this, but at least it has avoided some of the dubious answers emanating from various other quarters as diverse as public policy analysis and critical theory.

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POLITICAL
THEORY

The Democratic State. Edited by Roger Benjamin and Stephen L. Elkin. (Lawrence, KS: University Press of Kansas, 1985. Pp. vii + 275. \$30.00 cloth; \$12.95 paper.)

The notion of the state has always been difficult to reconcile with liberal democratic thought, especially its Anglo-American pluralist variants. Until recently, most discussion of the state had been rooted either in the Weberian model of permanent bureaucratic administration or in the Marxian concept of the state as an instrument of class repression. Most democratic theorists have rejected the idea of an autonomous state on principle (usually contract theory) or empirically: representative governments are bound by electoral preferences and group interests in the underlying civil society.

Liberal pluralist doctrine of this kind has come under increasing attack from both the Left and Right in recent years. Neo-Marxist theorists of advanced capitalism have revived the Gramscian idea of a relatively autonomous state apparatus that promotes the interests of capital apart from the organized expression of class interests by the ruling class itself. On the other side, the new political economists of the public choice school have deduced increasingly elaborate logical hypotheses from microeconomics and game theory which purport to demonstrate that democratic political processes cannot fairly and efficiently aggregate public preferences.

The Democratic State is a welcome effort to capture the main currents in this debate and, at the same time, to rescue the concept of the state from the extremes and incorporate it into a new, more catholic theory of liberal democratic governance. Although the book does not fully succeed in the latter regard, it does provide a stimulating set of essays on the political economy of capitalism and a biting critique of mainstream political science that ignores "the theoretical challenge posed by the increasing centrality of the state in the life of liberal democratic societies" (p. 12).

An early chapter by Roger Benjamin and

Raymond Duvall does an excellent job of dissecting various definitions of "the state" and in showing that its role and degree of autonomy cannot be divorced from its structural context. Specifically, they argue that the state plays a completely different role in advanced-capitalist or postindustrial society than in the dependent-industrializing capitalism of the third world. While the resulting typology is interesting, one wonders whether both post-industrial and Marxist structural concepts can be so conveniently accommodated as suggested here. Surely the range of structural alternatives is far richer than the ideal types presented.

The next two chapters present Marxist and public choice perspectives on capitalist democracy. David Braybrooke discusses classical and contemporary Marxist conceptions of the state and the possibilities for its transformation under predicted capitalist crises. After reviewing and questioning the views of Mandel, O'Connor, Poulantzas, and Habermas, he suggests that the late capitalist state—with its relatively autonomous bureaucracy and capacity for crisis management—could undergo gradual "self-transformation" into a benign, decentralized form of socialism. In contrast, the lengthy (91-page) chapter by Peter Aranson and Peter Ordeshook decries the "welfare-degrading tendencies of representative democracies" (p. 142), i.e., their tendency to produce an excessive public sector. The public choice argument is essentially that much of what passes for public good is in fact collective provision of concentrated private benefits to special interests. There is rarely any genuine welfare justification for state action in this view. The formal logic supporting this position is set out in considerable detail, with reference to electoral and interest group incentives and the consequent functioning of legislatures, bureaucracies, and even courts. Although the chapter fits awkwardly into the book and will be heavy going for readers unschooled in economics, it represents a good introduction to public choice analysis.

But the most valuable contribution is

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Stephen Elkin's eloquent essay entitled "Pluralism in Its Place: State and Regime in Liberal Democracy." Beginning with the Lindblomian observation that "the distinguishing feature of liberal democracies is the division of labor between market and state" (p. 179), Elkin argues that both state and economy are parts of (and thus shaped by) the more fundamental social and philosophical "regime" in which they are imbedded. Rejecting both pluralist and elitist conceptions of democracy, he holds that the liberal state is neither a neutral power arena nor a tool of class domination, but rather a "mixed" state in which the needs of business and popular control are both accommodated. In essence, he carries Lindblom's analysis of politics and markets a step further by showing how the dependence of democratic governments on private business performance can be legitimated by the acts of the state itself in balancing competing concepts of justice.

The final chapter on "Political Futures" by Norman Furniss rounds out the book with some empirical observations and philosophical reflections on the challenges posed by the international economic "regime crisis" for the further development of the social democratic state. Although retrenchment of the welfare state is occurring in the 1980s, Furniss discusses the alternatives of supply-side "socialization of production," as in France, and "autogestionnaire democracy" built around citizen and worker movements in other countries.

This volume obviously does not produce a coherent liberal democratic theory of state but does confirm that the concept of the state is a rich vein to be mined in political science. *The Democratic State* belongs in all academic libraries and will provide essential supplementary reading for many political science and political economy courses.

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Force or Freedom? The Paradox in Modern Political Thought. By William T. Bluhm. (New Haven: Yale University Press, 1984. Pp. xvii + 322. \$25.00, cloth.)

This work is an articulation and criticism of the confusions in political life deriving from the Cartesian subjectivism that lies at the foun-

dation of all major representatives of modern political thought. In Bluhm's view, Cartesianism provides the metaphysical source of the problematic or contradictory political ideas of the modern era. He indicts Hobbes, Locke, Rousseau, Kant, Hegel, Marx, and Nietzsche with internal contradictions that have their origins in the worldview of Descartes. One example is his view that Locke brought Hobbes's Leviathan within a constitutional order, but merely substituted an absolute social majority for an absolute crown. The irony of what is intended compared to what is produced in the case of each of these thinkers is what Bluhm refers to in the book's subtitle, "The Paradox in Modern Political Thought."

Bluhm introduces his theme in chapter one in terms of the dichotomy between various interpretations of force and freedom in recent controversies about the modern classics. According to Bluhm, commentators have tried over a period of 200 years to resolve this paradox in modern theories of politics in favor of either "libertarian" or "authoritarian" interpretations of the classics. Before going into his own analysis of each of the classic moderns, Bluhm, in chapter two, analyzes the nature of Cartesian dualism (subject/object, mind/body, reality/knowledge). In short, the whole of subsequent modern political thought was constructed upon a philosophical fault that still underlies major contemporary ideologies. Bluhm's treatment of Descartes is necessarily brief, but I have no quarrel with his sketch of Descartes' metaphysics and epistemology. Indeed, Bluhm performs a useful function in succinctly articulating the significance of a thinker usually considered non-political for the core ideas or basic categories of otherwise highly diverse political thinkers from Hobbes to Nietzsche.

The central chapters, three through five, document the contradictions and ambiguities between what I will briefly term the libertarian and authoritarian strains inherent in the ideas of Hobbes, Locke, Rousseau, Kant, Hegel, Marx, Mill, and Nietzsche, and argue their grounding in a common metaphysics rooted in Descartes. The analysis here is succinct, persuasive, and clearly informed with a coherent, interpretational perspective derived from years of reflection.

In chapter six Bluhm reviews several different twentieth-century attempts to replace

Descartes' dualism with holistic philosophies that might eliminate the contradictions of modern political thought. "With Nietzsche's death," writes Bluhm, "all the possibilities implicit in the Cartesian world-view had been stated" (p. 237). But Bluhm assumes here that no worldview has taken the place of Cartesian dualism, and that we are still living under the stresses of political cultures dominated by these Cartesian possibilities. Contemporary thought seems to be looking for a "new way," according to Bluhm, but the tensions between force and freedom still dominate the recent efforts of even those who have tried to develop a "holistic understanding of social reality."

Bluhm touches briefly on existentialist thought, but concentrates his analysis on the works of Wittgenstein and Winch as representatives of ordinary language philosophy, and on reinterpretations of Marxism by Sartre and Kolakowski. Included is a section sketching very briefly some contemporary criticisms of ordinary language philosophy and phenomenology, especially in respect to their attempts to develop new philosophies of social and political science. Neither of them, in Bluhm's eyes, succeed in providing a cognitive ground for human personality without abandoning the contributions of science. That is, they fail to contribute to the development of a humanistic social science.

The last effort at finding a new way begins from the categories of Marxism. From Bluhm's viewpoint, Sartre's work ends in a contradiction between his analysis of human freedom (in "the project") and his understanding of historical process as necessary. Similarly, Kolakowski never moves beyond the contradiction inherent in his views of freedom and determinism.

All contemporary movements of thought for Bluhm have thus failed to solve the force/freedom problem, have failed "to place force, even in idea, at the service of human freedom. . ." (p. 273). In his final chapter Bluhm attempts to suggest a way beyond the dualist categories of Descartes' definition of reality. He suggests that it is not just flawed argument, but "existential incoherence" that pushes us towards this new outlook. Bluhm aligns himself with Alasdair MacIntyre's rejection of the common, total *ethos* of both liberal individualism and Marxism, and with his thesis that "our world of values is in disorder today" to the

point where we have no way of testing various modes of moral argument or ways of life. As part of an overall "working hypothesis," Bluhm rejects two notions: (1) that history can be represented in terms of necessary laws (which is consistent with the abandonment by natural science of necessity for probability), and (2), that the terms mind and body are useful categories as starting points for theorizing (which also have been destroyed by science). In place of the Cartesian dichotomy of sense experience and private minds, Bluhm asks the reader to consider whether we do not have access to a "public mind," a mind embodied in human experience or common-sense reality. Such a public mind might tell us what mankind has historically and universally defined as good and evil. Following MacIntyre, he rejects the concept of abstract principle in favor of a neo-Aristotelian concept of the virtues, some of which apparently are universally accepted. These virtues—such as truthfulness, justice, and courage—are embodied in concrete social practices and in particular communities. Bluhm is not saying there are any shared ideals of humanity that might replace the "problematical ontologies" criticized in this book. What he does suggest is that there are "common themes" that cut across the "plurality of ideas."

One commonality is the obvious historical dialogue "between hedonistic self-interest and inherent or absolute right as fundamental motives" (p. 288). In every time and place, he suggests, these have "had to come to terms with each other." He then proceeds to sketch very briefly the various modes in which this dialogue of right and interest has been expressed in Western biblical, ancient Greek, Oriental, Indian, and Islamic writings. He seems at times to suggest that inherent right and enlightened (not radical, or short-term) self-interest converge or become identical in these historical worldviews. While hinting at only a few political implications from this fusion of morality and self-interest (p. 304), Bluhm concludes with the suggestion that "the moral experience of mankind, sketched here," while requiring more extensive empirical comparative inquiry, does suggest certain universal norms as "parameters to the freedom of individuals," while allowing "leeway for variety in life style and in the structures of political order" (p. 308). Thus, the gaps

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between mind and body, and between fact and value, dissolve as we recognize that we live in a world defined by both hedonic and moral values that shape and restrain us, but also that we create ourselves because we are "symbolic entities." The force/freedom paradox disappears in the symbolic, normative practice of mankind, which negatively sets only parameters to the freedom of individuals while allowing positive freedom for concrete, substantive decisions regarding lifestyles and political structures. Undoubtedly, many political theorists will find unsatisfactory Bluhm's new way out of the contradiction created by the Cartesian dualism that underlies modern political thought, but it is very tentatively suggested at the end of what I consider a very perceptive and persuasive diagnosis of the problems of modernity. His analysis of modern thought leading to that diagnosis makes this book well worth reading and very useful, I should think, as challenging ancillary reading in graduate courses in modern political theory.

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The Nature of Social Laws: Machiavelli to Mill. By Robert Brown. (London: Cambridge University Press, 1984. Pp. ix + 270. \$39.50, cloth.)

In this ambitious book, Robert Brown aims "to trace, and to some extent criticize, the intellectual sources and development of the . . . view, first made explicit during the Enlightenment, that social life can, and should, be studied by the same methods as those of the natural sciences" (p. 251). Brown begins by pointing out that only isolated empirical generalizations were stated by pre-seventeenth century thinkers (introduction and chap. 1). He goes on to suggest that the first connected system of empirical generalizations was the notion of a self-regulating "circle of commerce" pioneered by seventeenth century political economists such as Mun, Misselden, North and Locke, and subsequently elaborated by later economists including Quesnay, Turgot, Hume, Smith, Condorcet, Say, Malthus, Ricardo, Senior, and Mill (all of whom are briefly discussed in chapters 2-5)

Brown then argues that the development of political economy gave rise to a secondary literature devoted to methodological issues in the study of social life, engaging not only the economists themselves, but also historians and philosophers such as Vico, Turnbull, Comte, and Whewell (each of whom is also discussed in chapters 3-8). The secondary literature was "stimulated by the prospect of finding in social processes the analogues of Newton's laws of physical processes" (p. 71). In this regard, Mill's *System of Logic* (1843) is taken by Brown to represent the zenith of the Enlightenment conception of social science, "subsequent elaboration [having added] nothing essential" (p. 5).

Brown concludes his critical survey by strongly affirming that "we cannot realistically hope to find precise social laws concerning the sequence of phases of group life" (p. 261). "In this respect, there are not, and cannot be, scientific laws of society as there are scientific laws of nature. Hence there will never be trustworthy, accurate and wide-ranging social theories comparable to that of Newtonian mechanics" (p. 262). This conclusion, it should be noted, might possibly be interpreted to be compatible with, say, Mill's conception of social science, since Mill certainly does not claim that social science can achieve the predictive accuracy of the Newtonian model. Rather, he emphasizes that social phenomena are extremely diverse and constantly changing, not least because human volition is itself a causal antecedent, so that unforeseen changes of circumstances will in practice often violate the *ceteris paribus* clause implicit in the statement of any social science theorem. In other words, the influence of any one social law will often be counteracted in unpredictable ways by the influence of some other social law as circumstances change (*Logic*, book 6, esp. chap. 6). But Brown apparently seeks to present a much stronger conclusion than this familiar caveat about the level of guidance to be expected from social science. He suggests that we should confine our attention to finding "laws that connect social factors which are contemporaneous—Mill's laws of co-existence" (p. 262). However, as Mill makes clear in the *Logic* (bk. 3, chap. 22), such laws (whether considered ultimate or derivative from unknown causal laws) are merely empirical generalizations rather than scientific

theorems (except when confirmed by an extremely wide induction by simple enumeration, akin to that underlying the universal law of causation). In effect, then, Brown defends some form of casual empiricism (tied to our "common-sense vocabulary") against a properly scientific approach to social phenomena. Indeed, his view is that "the social studies will not suffer much if there are no laws of social life" (p. 263).

Space limitations confine me to four brief comments. First, Brown's technique of sketching the ideas of a wide variety of classical thinkers, while impressive in scope, seems inadequate for his critical purposes. Given his view that Mill brought the Enlightenment conception "to a point sufficient for us to fully understand and appraise it" (p. 5), a more careful study of Mill's work on methodology (including the essays on Comte and Hamilton, as well as the *Logic*) should perhaps have been the focus.

Second, and related to the first point, serious doubts can be raised about Brown's interpretations of several thinkers. He seems to misinterpret various aspects of Mill's thought, for example, including the role of *ceteris paribus* clauses in the statement of causal laws (pp. 150, 230-32); the justification for regarding "laws of large numbers" to be causal laws rather than mere empirical generalizations (pp. 232-33); and the ways of determining whether known causal laws are ultimate or derived from more general causal laws (pp. 226-30).

Third, while Brown focuses rightly on "psychological reductionism" as a central tenet of the Enlightenment approach, his arguments certainly do not destroy the idea that scientific explanations of social life must ultimately be based on laws of individual psychology (pp. 49-96, 187-93, 255-57). In this regard, his failure to consider the writings of Mill and Bain on "association psychology," vitiates his criticisms (pp. 241-44) of Mill's distinction between the abstract laws of psychology (invariant across all societies) and the concrete laws of ethology (specific to the material circumstances of the given society). Moreover, his familiar suggestion (pp. 60-61) that the self-regulating behavior of free markets cannot be reduced to the intentions of individual buyers and sellers is not persuasive, as Amartya Sen has recently argued in another context (1983. *The Profit Motive*, *Lloyds Bank Review*, 147:

1-20). Perhaps justification rather than explanation of the market might rest in part on "unintended consequences," but even this is contestable since the efficiency of market equilibria rests entirely on the harmony which the price system brings about among the selfish intentions of different individuals.

Finally, Brown makes no attempt to distinguish scientific determinism from fatalism, and sometimes writes as if he believes there is no distinction: "Social laws. . . , if they exist, can be obeyed for only one reason, namely that the question of choice does not arise" (p. 254). But Mill took great pains in the *Hamilton* (chap. 28) to clarify the scientific "doctrine of causation," arguing (pace Berlin) that the doctrine is quite compatible with our feelings of moral freedom and responsibility. In this regard, Brown's rather curt dismissal (pp. 5-6) of Marx's views seems unfortunate. A comparison of Marx and Mill might well have proved illuminating, if only to clarify why Mill in the *Logic* (bk. 6, chap. 10) generally defends Comte's empirical law of social development as having "a real scientific basis" in the laws of psychology and ethology, whereas Mill in the *Chapters on Socialism* rejects any grand law of social development of the Marxist variety as unscientific fatalism.

JONATHAN RILEY

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The State. By Anthony de Jasay. (New York: Basil Blackwell, 1985. Pp. 256. \$24.95.)

De Jasay begins his book by saying that he is going to consider the action of the state, "as if it were a real entity, as if it had a will and were capable of reasoned decisions about the means to its ends. Hence [I try] to explain the state's contact towards us in terms of *what it could be expected to do*, in successive historical situations if it rationally pursued ends that it could plausibly be supposed to have" (p. 1).

There are a number of problems with this, the first and obvious one of course is that the state is in fact a collection of human beings and has goals of its own only insofar as its collection of human beings more or less pushes in the same direction. Further, one might well believe the different states, insofar as they are pur-

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suing particular aims, are pursuing different ones.

Nevertheless, this kind of simplification of the real world is frequently valuable. Taking some collection of human beings, whether it is a large corporation, a branch of government, or perhaps simply a group of people who happen to have the same economic interest—the farmers for example—and considering how they can achieve goals which one might impute to that group is frequently a worthwhile exercise. It should of course always be followed by empirical investigation to find out whether the theory fits the real world. De Jasay makes no such investigation, but there is no reason why we should not have a division of labor. His theory could well be tested by other people.

The problem with his theory as far as I can see is that it fails this empirical test. De Jasay's model is one of steady government aggrandizement. The bureaucrats are assumed (quite plausibly) to continuously want to increase their power, and he assumes that they succeed. His last chapter is called, "State Capitalism" and the last section of it is subtitled, "On the Plantation." He thinks that the logical development of the state is a situation in which government administrators control everyone else for their own benefit.

It is easy to see why somebody looking at the present-day world might reach this conclusion. The growth of government is fairly close to an omnipresent phenomenon today. The problem with it as a theory is that this was not always true. In the nineteenth century the English state in essence dismantled itself. The elaborate collection of mercantilist controls, administered by a very well paid bureaucracy, with which England entered the nineteenth century was to all intents and purposes gone by 1870. The United States did not experience a similar history because it entered the nineteenth century with very little government, but certainly there was not the slightest trace of growth of government during the century. Indeed, except in war-time, the government shrank fairly consistently as a share of the total national economy. The fact that the U.S. ended the century with about the same portion of GNP absorbed by government as in 1800, is essentially an effect of the immense expansion of the government during the Civil War.

As can be seen, I think the major theme of de Jasay's book is simply wrong. Governments

have historically had the common tendency to aggrandize themselves, but not always—and a theory which implies that they always do is clearly refuted by the facts. However, if you turn to other aspects of the book it is indeed very interesting and one which I can conscientiously recommend. De Jasay has read an immense amount of recent writings, given them careful thought, and comments on them in an intelligent and highly critical way. Particularly impressive is his critique of Rawls.

His point of view is that of a French "liberal" which insofar as it has an American counterpart, would be called "conservative." There are however, considerable differences between the French liberal approach and the American conservative approach, and de Jasay exhibits the advantages of a French education. To sum it up, there are many acute critical comments in this book, but its main theme is, I believe, incorrect.

GORDON TULLOCK

George Mason University

Sour Grapes: Studies in the Subversion of Rationality. By Jon Elster. (Cambridge: Cambridge University Press, 1983. Pp. viii + 177. \$29.95.)

Sour Grapes should not disappoint anyone who is acquainted with Jon Elster's earlier works. There is the same startling ability to illuminate social life by bringing disparate phenomena into a common focus and viewing the everyday from an unfamiliar perspective. In this book Elster has turned his attention to forms of individual and collective irrationality. Beginning with an introductory discussion of thin and broad theories of rationality, he then devotes the next three chapters to three types of irrational behavior: (1) intending directly to bring about mental or social states that are essentially by-products of actions undertaken for other ends, (2) changing one's preferences to accord with one's possibilities by despising the unattainable as "sour grapes," and (3) arriving at beliefs on the basis of ideology. This last chapter contains a number of useful cautions against the vogue of "ideological critique," but I shall discuss only the other chapters, since they contain the most novel and provocative ideas.

A "thin" theory of rationality, says Elster in the first chapter, places only a minimal constraint upon an agent's beliefs and desires: they must be logically consistent with one another. He is right to find this approach inadequate. A "broad" theory, by contrast, would issue in stronger constraints than mere consistency, although without making the excessive requirement that rational beliefs and desires be true or morally best. His general proposal is that "we should evaluate the broad rationality of beliefs and desires by looking at the way in which they are shaped" (p. 15). This retrospective model, according to which the rationality of a belief or desire depends essentially on its *origins*, is a very old and scarcely implausible one. But it has some disadvantages. Many of our present and deepest convictions arose, not from reasoning, but from childhood training and the early imitation of others. This is an indispensable way in which tradition is passed on, and to mark it as irrational is undesirable if that means that it ought not to occur or that each of us, as Descartes urged, should begin again from scratch and build up all his beliefs and desires rationally. Elster might have considered the somewhat different view that rationality is primarily a future-oriented matter—that beliefs and desires are rational to the extent that (whatever their origin) we are willing to elaborate and revise them suitably in the light of experience. Fortunately, his discussion of the concrete forms of irrationality is largely independent of this more abstract controversy (even the future-oriented model will be concerned with how revisions of beliefs and desires are shaped).

In the second and most delightful chapter, numerous examples from Donne and Stendhal to recent cognitive psychology, serve to make palpable the irrationality of trying to bring about directly certain desirable states of mind which are essentially by-products. Being natural or spontaneous, for instance, is not something we can become simply by trying to be so; indeed, the very attempt makes it impossible (pp. 44ff.). Of course, there sometimes exist, Elster notes, "technologies for self-management," indirect methods for becoming what we cannot directly become: although I may be unable to fall asleep merely by deciding to do so, I can successfully pursue that end by deciding to take a sleeping pill (p. 53). But he also wisely suggests that some states of

mind (e.g., spontaneity) may be unattainable even by a roundabout method, since keeping in mind or remembering the use of such a method may be incompatible with being in that state (pp. 57–58). There seem thus to be at least two sorts of by-product states, those which can be pursued knowingly (though not directly) and those which cannot.

Further classifications of this sort would have been helpful, especially in his subsequent discussion of "self-defeating political theories" (pp. 91ff.). There his concern is with theories which seek publicly to justify political institutions by effects that are essentially by-products (p. 92). One of Elster's examples is Mill's idea that popular participation in political decision making is to be prized because it develops character and public spirit (p. 97). He also draws some interesting examples from Tocqueville. Now the way in which such theories would be self-defeating cannot simply be, as he first seems to suggest (p. 92), that they violate the "publicity constraint" which Kant and Rawls place upon political argumentation. It is not that no one can argue publicly (that is, knowingly and with the hope that others will knowingly do so) for democracy on the grounds of its educative effects, without undercutting himself. One surely can. Instead, such theories turn self-defeating, as Elster himself later observes (pp. 96, 98), only if they make such by-products the only public justification for a political arrangement. Politics builds character only if people are interested, not just in that, but also in making the correct decisions. This gives us a third category of states of mind that are essentially by-products—those we can knowingly achieve only if we are pursuing some other, further goal as well.

Chapter three deals with how people can irrationally adjust their preferences to their possibilities, rejecting what they cannot have as not worth having. Elster rightly insists that this phenomenon poses a deep, but neglected problem for the usual sorts of social choice theory which take preferences as simply "given" and make satisfaction of preferences the decisive criterion. Should we really say that someone becomes better off by spurning the unavailable as "sour grapes" (pp. 109, 133ff.)? His own analysis of sour grapes develops a very interesting hypothesis. Wishing to distinguish it from rational forms of adapting preferences to possibilities (such as

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Stoicism), he says that "sour grapes" characteristically tends to downgrade the inaccessible, instead of upgrading the accessible (as the Stoic would counsel), precisely because it is an unconscious response, rather than a deliberate plan (pp. 117-19).

This is a book chiefly about irrationality. But it also points to why certain widespread notions about rationality—most of all, the presumption that reason entails self-mastery—must be reconsidered. It is to be hoped that Elster will now push forward with this positive enterprise.

CHARLES LARMORE

Columbia University

A Matter of Principle. By Ronald Dworkin. (Cambridge: Harvard University Press, 1985. Pp. 425. \$25.00.)

These previously published essays are strongly continuous with Dworkin's earlier collection, *Taking Rights Seriously* (Cambridge, MA: Harvard University Press, 1977). There is once again a merging of jurisprudence with moral and political philosophy, and issues in these fields are pursued by consideration of court cases and other legal materials as well as through more abstract argumentation engaging the work of other philosophers. Thematically, Dworkin returns repeatedly to his distinction between principle (rights) and policy (goals), to his claim that political theory can give right answers in "hard cases," and to the elaboration and defense of egalitarian liberalism. Application of the first distinction to questions about reverse discrimination and free expression again tends to yield the conclusion that prominent recent claims to rights in these areas are without foundation, that the questions at issue in these controversies are of policy, not of principle. On the other hand, the focus of the discussions of judicial reasoning and legal theory (or meta-theory) has now shifted from criticism of legal positivism to a defense of Dworkin's own theory, especially against charges that he has embraced an untenable version of views against which legal positivism was a reaction. A welcome new feature is a section trenchantly criticizing the "economic analysis of the law," particularly

as practiced by Richard Posner and Guido Calabresi.

Terminologically, much of the continuity among the essays is given by Dworkin's repeated use of the distinctions between policy and principle, or goals and rights. But these are terms of art and Dworkin's reliance upon them reflects his singularly optimistic version of liberalism and his soaring ambitions for political theory.

Dworkin's liberalism at least appears to include the latitudinarian tendency concerning ends, purposes, and goals that is familiar from the work of a number of self-designated liberal writers. There is rarely an indisputable answer to such questions, and this in itself is reason to welcome diversity and experimentation concerning them. Dworkin is not, however, agnostic or even notably skeptical concerning these matters. He thinks they can be intelligently and profitably discussed and that there are often better as opposed to worse answers to them. For this (and perhaps for more communalist or communitarian) reason(s), he favors an active democratic politics in which such questions are debated and collectively resolved. The kinds of bias against collective decision making and against politics that are often associated with more robustly skeptical views (for example, because ends and purposes are matters of opinion they should be left as far as possible to individuals) and more dogmatic ones (because there are right answers to such questions they should not be left to the uncertainties of democratic politics) make little or no appearance in this collection.

Accordingly, and despite his allegiance to a rigorist view of rights, Dworkin frequently argues against right claims which would restrict the democratic process and the use of the authority of government to pursue desired social goals. The fact that affirmative action programs have defeated the expectations of people such as Allan Bakke and Brian Weber is not sufficient reason to prohibit or restrict them. Again, the extensions of First Amendment rights proposed by journalists such as Myron Farber are, "from the standpoint of democracy," double-edged swords. If they enhance democracy by increasing public information they also contract it "because any constitutional right disables the popularly elected legislature from enacting some legislation it might otherwise wish to enact, and decreases

the general power of the public" (p. 391). Generally, theories that forbid "the majority the use of politics and the law, even the criminal law" to "influence the conditions in which they must try to thrive" are "at least *prima facie* self-defeating" (p. 349).

But these views do not reflect any disposition on Dworkin's part to leave matters of principle and of rights to the majority, to received conventions, or to a market or other procedure allegedly neutral among possible resolutions of them. "Liberalism cannot be based on skepticism. Its constitutive morality provides that human beings must be treated as equals by their government, not because there is no right and wrong in political morality, but because that is what is right" (p. 203). While it is not impossible that the views of the majority or the results of market exchanges will be approximate to or even coincide with correct principles and the proper distribution of rights, they cannot be the test or criterion of political morality. Rather, the task of delineating and defending governmental principles and rights falls to those capable of the demanding tasks of political theory (cf. p. 408, n. 1, par. 1). Struggling "against all the impulses that drag us back into our own culture," struggling "toward generality and some reflective basis for deciding which of our traditional distinctions and discriminations are genuine and which spurious, which contribute to the flourishing of the ideals we want, after reflection to embrace" (pp. 219-20), political theorists must discover the truth and must "construct political institutions" (p. 195) that embody that truth.

The most general affirmative truth that Dworkin has discovered makes up "the principle fundamental to liberalism based on equality, the principle that people must be treated with equal concern" (p. 210). Although admittedly "abstract" (p. 191), Dworkin confidently affirms that this "is not an empty requirement" (p. 190). Because it is a constitutive truth to be "valued for its own sake" (p. 408, n. 1), further truths are to be sought primarily by elaborating upon it, comparing it with proposed alternative foundational principles, and so forth.

It is through this process of elaboration that the distinctions between principles and policies, and goals and rights are given definite shape. Claims (such as Bakke's and Weber's) to rights that cannot be derived from equal con-

cern or some principle of comparable standing are rejected. Others (such as a "right to moral independence" that "requires [among other things] a permissive legal attitude toward the consumption of pornography in private" [p. 358]) are at least tentatively endorsed. And while Dworkin allows that such reasoning will often be disputed (p. 313), even that his most fundamental claims "may in the end prove to be wrong" (p. 372), he throughout maintains that there are right answers to the deepest questions of political morality and often writes as if he has produced at least some of those answers.

Taken seriously, these latter among Dworkin's views imply that democratic (or, presumably, any other) politics are acceptable only if ordered by a constitutive political morality known—by at least some political theorists—to be true by criteria independent of such politics and indeed independent of the culture and traditions of the society in which those politics occur. The political theorists of liberalism must provide us with "convictions that we do not hold contingently" (p. 352).

Read as essays in practical reasoning about difficult and pressing issues in American and British public life, several of these papers (most particularly numbers 4, 11, and 14-19) repay close attention. Perhaps this is how Dworkin intends them to be read. As some of his meta-level remarks (especially in part two) might suggest, perhaps his talk of uncontingent truths, of right answers, and of natural rights, above all his view of political theorists as the legislators and guardians of political morality, are ways of urging all of us to vigorously self-critical thinking about our morals and our politics. However this may be, the discussions I have just summarized present views that are at once philosophically untenable and politically objectionable.

RICHARD E. FLATHMAN

The Johns Hopkins University

Morality and the Bomb: An Ethical Assessment of Nuclear Deterrence. By David Fisher. (New York: St. Martin's Press, 1985. Pp. 136. \$25.00.)

It is not surprising that Fisher is an articulate exponent of the just war tradition. His book

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was written while he was on leave at Oxford from the British Ministry of Defence, and most of the West's more conscientious soldiers and military officials are guided by the tradition's principles.

These principles are summarized in the first few chapters of this slender volume: There is a moral presumption against war; resort to war is only proper under sharply limited conditions; the proper conduct of warfare is constrained by the requirements of proportionality and discrimination (non-combatant immunity). Absolutists say that statesmen and warriors must comply with these norms, no matter what. Some utilitarians and self-styled "realists" dismiss the rules ("war is hell, anything goes"). Fisher prefers an intermediate position he calls "principled consequentialism" (p. 43); the rules are important and should be overridden only in extraordinary situations—situations apt to arise, alas, in the extraordinarily morally complex "post-lapsarian world we actually inhabit" (p. 44). Thus, for example, Fisher suggests softening the principle of non-combatant immunity to "the need to minimise non-combatant casualties" (pp. 49–50).

What about nuclear weapons? Do they, as Michael Walzer says, explode just-war theory? In the second half of his book, Fisher tries to apply his principled consequentialism. He can imagine some limited uses of nuclear weapons that would be licit (against combatant ships at sea, against important military targets on land). Some civilians would die, yes; but they die amidst great destruction in twentieth-century "conventional" warfare too. Their deaths could be "minimised." The leaders could conceivably keep the fracas from escalating to wanton extremes. If limited use of nuclear weapons was "the *only* way to prevent a very great harm" (p. 58, author's emphasis), then the leaders might in good conscience use them.

Fisher clearly recognizes that it would be preferable to avoid their use. This brings him to an assessment of nuclear deterrence as the cornerstone of western policy. He tries to rescue nuclear deterrence from moral condemnation by arguing, first, that there are no satisfactory alternatives. Pacifism would invite conquest or blackmail; reliance on conventional defense against a nuclear foe would result in sure defeat; defenses against nuclear-

tipped missiles will not work; nuclear disarmament by the powers is unlikely; transformation of the world political structure is a utopian vision. There is no examination of civilian resistance as an intriguing alternative to nuclear deterrence. More importantly, there is too little attention paid to the political content of the East/West relationship and to the approach of rediscovering politics and employing diplomacy more continuously and effectively to improve the relationship so that both sides could move beyond reliance on primitive threats.

Second, he says, deterrence might be licit since the western leaders may not really be intending to carry out the threatened genocidal retaliation. Maybe they are just signalling to Moscow that they have not completely ruled out retaliation. So maybe they have not improperly formed a conditional intention to do an immoral act. This seems to rescue deterrence via too-clever verbal maneuvering, though, given the mountainous arsenal and the frequent restatement of the threat. And even if they have not formed the intention, the leaders have implicated thousands of subordinates on down the chain of command, people ordered to form an intention to carry out the retaliation, people wrongly ordered into an immoral posture.

JAMES A. STEGENGA

Purdue University

Benjamin Constant and the Making of Modern Liberalism. Stephen Holmes. (New Haven: Yale University Press, 1984. Pp. vii + 337. \$27.50.)

In this brilliant book Stephen Holmes sets himself two rescue missions, saving Constant from the myth surrounding his theory of ancient and modern liberty and saving modern liberalism from its hosts of critics, be they conservative, romantic, Marxist, Strausian, participatory democrat, or *res publica* humanist. Holmes does both superbly. He illuminates the complexity and significance of Constant as a political theorist with painstaking care and in the course of doing this makes a moving case for liberal political ideas.

Mythic Constant as presented by Plamenatz, Talmon, and Berlin is an anti-democrat, whose

famous essay comparing ancient and modern liberty is read as a repudiation of the classical ideal of civic participation in the exercise of public power. In its stead mythical Constant is depicted as the champion of modern liberty, understood as the peaceful and secure enjoyment of private independence and autonomy. To be sure, there is much in Constant's writings to justify this reading. Indeed, Holmes mercilessly reveals Constant as the gravedigger of the ancient city. We are given pages of Constant's deromanticization of the polis and its ideal of teleological politics. Constant's rejection of the primacy of politics, what Holmes calls "decentering politics," is seen most vividly in Constant's critique of Rousseau and his vision of a resurrected Spartan citizenry, perpetually sacrificing individual ego by ceaseless involvement in public life. Constant was too skeptical about moral certainty to see individual choices about the good life sacrificed to either the general will or to public virtue. The nightmare of Jacobin terror in the name of virtue and democracy haunted Constant.

Holmes makes clear, however, that this is but a partial reading of Constant. There is much that he admired in Rousseau, for example, his egalitarianism and his repudiation of hierarchy. Most significantly, Holmes gives us a Constant who also insists on the importance of political liberty, of civic involvement and public participation. Far from advocating only private enjoyment of individual rights, Constant insists that personal security and independence can only be guaranteed through political participation. Channeling all one's energy into private life serves the cause of tyrants and oppressors. Here Constant has in mind Napoleon and the ultrarightists of the Restoration. Constant, Holmes makes clear, goes even further on occasion. Not only does participation ensure independence, it also is an ethical end in itself—contributing to self-perfection. Here, too, one sees his ambivalence towards Rousseau.

Holmes is particularly good at showing how firmly Constant's notion of liberty and its transformation over time is grounded in historical and institutional context. He sees the content of liberty changing relative to the existence of slavery, the size and population of states, the rise of commerce, and the division of labor. But what gives this book its real cutting edge is that through Constant, Holmes

offers a moving defense of contemporary liberalism.

It is often unclear in the text whether one is reading Constant or Holmes, whether the defense of modern liberty is being directed against early nineteenth century anti-liberals, like Bonald or Fichte, or against twentieth century anti-liberals, like MacIntyre, Wolin, Arendt, Taylor, or Wolff. (This is made all the more problematic because of the significance of Holmes's footnotes.) Few have written such moving praise for liberalism's egalitarian and progressive roots—its anti-aristocratic, anti-clerical, anti-militaristic ideals—as Holmes. Few have written so movingly on the perennial values of a moderate, non-teleological state whose ambitions are not to create moral consensus and educate virtuous citizens, but to develop rules and procedures for settling conflicts, solving problems, and adjusting rights and duties. What "we learn from Constant," according to Holmes, is that "a good society is one in which public success is judged by uninspiring standards such as peace, order, efficiency, impartiality, openness to criticism and procedural justice. Private life can and should embody any number of expressivist ideals: bliss, creativity, friendship, sensitivity, salvation, genius, and love. But these are best excluded from the public realm, at least for the most part" (p. 273). Whether one agrees or disagrees with him, it is clear that through Constant, Holmes has offered a resounding liberal reply to liberalism's critics.

ISAAC KRAMNICK

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Science, Language and the Human Condition.

By Morton A. Kaplan. (New York: Paragon House, 1984. Pp. 394. \$27.95.)

Professor Kaplan has written an ambitious book. As he tells us in the introduction, "I hope to show, through an inquiry into science and language, that the human race has a meaningful place in the world, that similar general methods of inquiry are appropriate to the sciences and the social sciences, and that the method of *praxis*, or assessment, relates neighboring realms of knowledge" (p. 1). The key element of this project turns out, not surprisingly, to be the attempt to establish that

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moral judgments are not different in kind from scientific judgments, or at least from the kind of judgments deployed in producing scientific knowledge.

The best feature of the book is its praiseworthy aim. Kaplan wishes to establish the intellectual authority of a concept of science broad enough to include reasoning about "values," which means about what is good and bad for human beings. Although "restore" would be a better description than "establish," Kaplan does not mention the fact. He sees clearly, and opposes, the tendency of recent accounts of scientific method to "formalize" one or another aspect of the real activity of scientists, and then to use the formal account critically against other parts of scientific procedure. The activity of scientists, as Kaplan notes, includes what he calls "praxical" reasoning—assessments of "fit" or "coherence" which are left out of the formalized accounts. This general argument of Kaplan's cannot be repeated too often in the current state of our discipline.

But despite Kaplan's immersion—sometimes submersion—in contemporary and recent literature on science, he seems to be strangely innocent of other, perhaps more compelling attempts to come to terms with scientific reason, most notably the efforts of Hume and Kant. The book is marred by Kaplan's failure to avail himself of the deepest thinkers, or indeed of the thought of anyone earlier than Marx. And even with regard to Marx we are told that "Marx wrote before we were able to recognize that all complex systems designs, even the best, involve compromises among the goals to be achieved or restrictions on tendencies inherent in parts of the system" (p. 315). A statement this extraordinarily silly is not, fortunately, typical of the book.

There are serious substantive flaws; however. Two examples will suffice. Kaplan spends a good deal of space on Wittgenstein and his notion of language games. If I read Wittgenstein correctly, the great point of his notion of language games is precisely to prevent single-use accounts of language (e.g., language is a communications code, or language is a tool, etc.) from controlling our understanding and thus blocking from view the multiplicity of ways we use language. Yet here is Kaplan: "Language is merely an instrument designed to elicit meaning, to place

meaning within a framework that shapes it" (p. 171). It is difficult to believe that anyone who had studied Wittgenstein seriously could conclude a chapter on him with this casual and dogmatic assertion. Kaplan's arguments are often sound, but he seems to make the very mistake he repeatedly complains about. He takes a part for the whole, discussing Wittgenstein's concept of language games alone as if it were the center of his thought. (Wittgenstein's notion of grammar, to suggest only one, is at least as central.)

For Kaplan there are no metaphysical traps or puzzles. "We know that others see what we see," he assures us (p. 158), or later, "adequate recognition of the correlative tasks of theory and praxis is all that is required to combat the fallacy of essentialism and to permit a consistent pragmatist interpretation of the world" (p. 171). According to Wittgenstein, however, philosophy itself is necessary only because of the human tendency to become entangled in paradoxes or puzzles. "Philosophy is a battle against the bewitchment of our intelligence by means of language" (*Philosophical Investigations*, p. 109). Kaplan has no need of such philosophy. On the basis of a comment by Wittgenstein to the effect that we solve problems "not by giving new information, but by arranging what we have always known," he accuses Wittgenstein of failing to distinguish properly between description, on the one hand, and empirical investigation on the basis of hypotheses on the other. "Would Wittgenstein have argued that if a caveman lived long enough, by a constant rearranging of what he knew, he would become aware of electrons, quasars, quarks, cloud chambers, electron microscopes, microbes, and modern philosophy?" (p. 149). This is embarrassing. Wittgenstein never says that we do *science* by "rearranging what we have always known." That is how we do philosophy. The very passage Kaplan quotes is preceded by Wittgenstein's careful distinction between science and philosophy, and two lines before the quoted passage he had said "philosophical problems . . . are, of course, not empirical problems" (*Philosophical Investigations*, p. 109). The bedrock of Kaplan's understanding of these issues is only too clear. It is suggested nicely by a passage in which we are told that "the science game . . . is the master game" (p. 154). Such a conviction, however, vitiates the study's per-

suasiveness to those for whom the *status* of science (as well as its procedure) is part of the question.

Numerous other problems might be cited, including the fact that the lengthy chapters on Marx and Gramsci (part 4) do not seem well integrated with the earlier argument. At any rate, notwithstanding the importance of the task Kaplan sets himself the book does not live up to its promise. While it might serve as an introduction to the state of twentieth-century philosophical argument concerning science and language, it is difficult to imagine many political scientists willing to work through the long discussions of philosophical *arcana*, of Kripke, Putnam, Gödel, Linsky, Blanshard, Weiss, Glover, Donagan, Gewirth, *inter alia*. One is forced to suggest that only those unable to take seriously anything written before 1901 will find this book to be worth the effort, but since that includes many political scientists, the book deserves an audience.

JOHN W. DANFORD

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Liberalism and its Critics. By Kirk F. Koerner.
(New York: St. Martin's Press, 1985. Pp. iv
+ 396. \$39.95.)

For years liberalism has been under attack. Critics from the left and the right have severely questioned the intellectual credibility, moral legitimacy, and contemporary relevance of liberal political theory and liberal-democratic politics. It indeed requires no small measure of courage to declare oneself a liberal in these ideologically-charged and increasingly intolerant times.

Kirk F. Koerner has dared to go beyond a mere profession of intellectual faith. He has written a book defending liberalism and criticizing its most persuasive anti-liberal critics. Liberals, of course, have worked hard to restore the credibility of their doctrine. Koerner describes how "neoclassical liberals" such as Milton Friedman and Robert Nozick call for a renewed emphasis on liberty and thus advocate a return to *laissez-faire* while "contemporary positive liberals" like John Chapman and John Rawls seek to shift the prescriptive focus of liberalism toward distributive justice and greater economic equality. Yet,

Koerner argues, these liberals have not confronted "the deeper problem of justifying liberal democracy itself" (p. 317). For this, liberals must first answer directly those who have denied the very possibility of such justification and demonstrate that their critiques of liberalism are "unconvincing" (p. 2).

Accordingly, Koerner selects four of the most influential contemporary critics of liberalism, representing "persistent and deeply-rooted anti-liberal tendencies in Western political thought" (p. 2). C. B. Macpherson, Herbert Marcuse, Leo Strauss, and Michael Oakeshott hold liberalism "in large part responsible for most of our present problems" and claim that "it offers little of value to modern man" (p. 18). Koerner evaluates the merits of this anti-liberal position by examining their writings in impressive detail.

Liberalism and its Critics provides new evidence to support some familiar objections to the arguments of Strauss and Macpherson. Thus it further confirms that Strauss's view of the "modern project" is only "a caricature of the liberal tradition" (p. 251), and that Macpherson fails to prove the decisive acceptance of market assumptions and of "possessive individualism" by Hobbes, the Levellers, and Locke (p. 76). Koerner finds "evident exaggerations" in Marcuse's interpretation of liberalism, including an "erroneous, perverse, and misleading" identification of liberalism with fascism (pp. 154-55). And turning finally to liberalism conceived pejoratively as "rationalism in politics," Koerner discovers only a "fictitious adversary" (p. 282). None of the liberals indicated by Oakeshott actually subscribed to the essential tenets of the rationalist creed (p. 295). These four critics, Koerner concludes, have "seriously misinterpreted" the textual origins and subsequent evolution of liberalism (p. 309).

Koerner also considers the kinds of political arrangements that these critics believe ought to replace those of liberal democracy. Marcuse's "general call for a 'non-repressive' society seems utopian, tainted by elitism and enmeshed in a theoretical inconsistency" (p. 173). Macpherson proposes a "participatory democracy" in which "both democracy and freedom may be effectively proscribed or postponed indefinitely" (p. 105). And Oakeshott's conservative "alternative to liberalism . . . provides no way of distinguishing desirable and

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undesirable elements in political experience" (p. 297). On the other hand, all four critics fail to acknowledge the decisive contributions of liberalism to the cause of human freedom; but Koerner serves that cause well indeed by recalling liberalism's "monumental" achievements (pp. 2-6).

The book concludes with a call for a "universal liberal world-order"—what Koerner himself terms "an impossible dream" (p. 330). Nevertheless, liberalism does offer "the only certain theoretical guarantee that freedom will be preserved," and for this reason "the liberal ideals of liberty, tolerance, and diversity . . . must once again become revolutionary in their force and implications" (p. 322). It is precisely liberalism's "fundamental, deeply-rooted, and unshakeable commitment to individual liberty and self-determination" (p. 309) that provides the most compelling justification for liberal democracy in modern times.

STEVEN MICHAEL DWORETZ

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Churchmen and Philosophers: From Jonathan Edwards to John Dewey. By Bruce Kuklick. (New Haven: Yale University Press, 1985. Pp. xx + 311. \$27.50, cloth.)

In this work Bruce Kuklick has presented the basis for a major reassessment of the American intellectual heritage. By rejecting the standard interpretations of nineteenth-century American thought, which he believes to be heavily biased by twentieth-century secularist predispositions, and by explaining intellectual controversies within the context of their era, Kuklick is able to provide a coherent explanation of the development of welfare liberalism as an indigenous American ideology. Thus, he contends that the important intellectual questions for most of the nineteenth century stemmed from Calvinist doctrine and that the instrumentalist philosophy of John Dewey and the social sciences which it justified had their origins in these theological conflicts.

Kuklick argues that the eighteenth-century thought of Jonathan Edwards provided the framework for much of the intellectual activity of the following century. Within the bounds of Calvinist belief, Edwards formulated a sophis-

ticated rationalist metaphysics that was to serve as the basis for the New England Theology, in Kuklick's opinion "the most sustained intellectual tradition in the United States" (p. 43), lasting approximately 125 years. In a tightly argued and detailed fashion, Kuklick traces the controversies and schisms that occurred within this tradition, focusing primarily on debates about regeneration, free will, and theodicy. Because they discussed the issues thought to be important at the time and were associated with major theological institutions and schools of thought, thinkers like Nathaniel William Taylor, Horace Bushnell, and Edwards Amasa Park are treated as the prominent intellectuals of their day. To substantiate his argument further Kuklick accompanies his exegesis of the positions of these and other thinkers with descriptions of the roles of Harvard, Princeton, Yale, and the Andover Theological Seminary in influencing and propagating variants of Calvinism within the New England Theology.

Despite their early dominance, theologians found their status as public spokesmen eroding by the middle of the nineteenth century. The Scottish common-sense philosophy, which had supported most of the Calvinist theological positions, began rapidly to lose support to organic, historical arguments from Germany that were more compatible with new ideas in geology and biology. Equally important to the weakening of the New England Theology was the growing professionalization of religious study. Kuklick contends that the establishment of separate schools of theology was a critical turning point in the evolution of American thought generally in that it was "an early and potent symbol of the fragmentation of knowledge and culture" (p. 87). More immediately, theologians found themselves arguing increasingly over esoteric theological points while those thinkers engaged in philosophy began to establish themselves as competent to address important public issues and "emerged as the guardians of the character of college-age youth" (p. 119). Thus, the encroachments of German thought, the acceptance of evolutionary science, the narrowness of theological debate, and the social needs of the time all conspired to weaken the intellectual vigor and the social status of the disputants within the New England Theology. In Kuklick's view the end of the New England Theology as a viable intel-

lectual force came in 1881 with the removal of Park from his post at the Andover Theological Seminary by the Andover Liberals, who espoused the New Theology, or Progressive Orthodoxy.

Kuklick argues that the success of the proponents of the New Theology had profound ramifications for the content of liberal reform in the twentieth century. The harsh Calvinist ethic of individual striving and responsibility fashioned in response to concerns about original sin and salvation was never defeated in intellectual debate. It was simply ignored because the questions around which it was framed were deemed to be no longer relevant to social issues. As Kuklick puts it, "personal responsibility for depravity" became an "unhelpful way to think about the social causation of untoward behavior" (p. 229). Instead, impressed by evolutionary science and the gravity of social problems, theologians turned to religious positions based on experiential justifications that entailed the acceptance of community responsibility for social ills.

It was this latter theological context, Kuklick contends, that influenced John Dewey extensively and was the source of essential elements of his instrumentalism. For the first 30 years of his life, Dewey was deeply concerned about religious issues. His mother was a strict Calvinist, and Dewey himself underwent a religious conversion experience in his twenties. Even more to the point, he became involved in the theological controversies emanating from Andover and wrote pieces for *Bibliotheca Sacra* and the *Andover Review*, both journals housed at Andover. Kuklick concludes that it was from the theological liberals at Andover that Dewey derived his belief in the need to ground thought in natural experience, as well as much of his attachment to the scientific approach. He further suggests that Dewey's sympathy with the New Theology led him to assume that the laws of righteousness found their being in the natural world, and that it was but a matter of applying human intelligence for their discovery.

Kuklick has rendered an exceptionally valuable service to the study of the American intellectual tradition by demonstrating that there are still opportunities for fresh and stimulating work in the field. Many readers will find unsettling his conclusions that the Transcendental-

ists were of marginal importance, that William James and Charles Peirce had no significant formative influence on Dewey's instrumentalism, and that for most of the nineteenth century the dominant intellectual issues were theological. Yet, as a result of Kuklick's work, these and other possibilities must now be seriously considered if social scientists are to be reasonably confident that they have an accurate understanding of the past and an efficacious conception of the present.

ROBERT A. HEINEMAN

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Daniel Bell and the Agony of Modern Liberalism. By Nathan Liebowitz. (Westport, CT: Greenwood Press, 1985. Pp. ix + 293. \$29.95.)

Nathan Liebowitz has written a significant book for political and social theorists, coming to terms with the crisis of contemporary liberal thought. Set in the framework of an analysis of Daniel Bell's writings, the book explores two major themes. First, it addresses the question: "[I]s Bell an ideologist of the end of ideology who is attempting through the development of the theory of the postindustrial society to rationalize the ideological perspectives embedded in the end of ideology?" (p. 203). Second, the book highlights the contradictions and tensions in Bell's efforts to develop a theory of contemporary American society, a theory of the postindustrial society.

Much of the book discusses the end of ideology thesis. Bell's own contribution (*The End of Ideology*) is seen as reflecting his experiences with socialism, journalism, and academia in the 1930s and 1940s. In that work, he rejects his earlier vulgar Marxism as well as leftist political and economic analyses. He is skeptical about the capabilities of reason and planning, and he doubts the merits of a liberalism that fails to understand the vicissitudes of American society. Above all, he asserts that ideological politics has no place in the complex modern world. Bell's effort to finalize the ideological debates of his youth eventually ends in a call for a limited form of planning. As Liebowitz notes: "The solution of pragmatic planning thus enables Bell not to reject the

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heritage of liberal humanitarianism in response to his horror at the inhumanity of totalitarian planning."

This conclusion regarding the end of ideology highlights the major tension Liebowitz discovers in Bell's thought. Bell's work constitutes an effort to reconcile the rationalist activism and optimism of John Dewey with the theological restraint and pessimism of Reinhold Niebuhr. Nowhere does this effort appear more clearly than in Bell's more recent work on the postindustrial society and its problems—problems related to the development of a communal, knowledge society within the context of corporate capitalism and an adversary culture.

In Bell's analysis, American society faces a crisis of belief. The New Deal liberalism designed to legitimate society has instead undermined it in various ways. It has fostered a revolution of rising entitlements that encourages groups to make ever-increasing demands for social and political rights. It has given birth to a new sensibility that relies on feelings as the touchstone of experience and judgment, that abjures rational thought and criticism. Most importantly, liberalism's focus on economic growth has left it unable to provide any mechanism for limiting private wants for the sake of the public good—for allocating benefits and burdens across society. In short, liberalism has failed to develop a new integrative public philosophy, a philosophy of the public household.

Once Liebowitz has sketched the outlines of Bell's views on the end of ideology and on the problems of postindustrial society, he moves to answer the question of whether or not Bell is an ideologist. Bell justifies the need for a new public philosophy on grounds provided by his analysis of postindustrial society. Scarcity, human nature, the limits of reason and planning, as well as the need for civility prohibit any political economy more radical than the welfare state in the United States. Hence, Bell is more than an ideologist, for his prime concern is to lay the conceptual foundations for generating a new integrative public philosophy (p. 243). Yet in the final analysis, the results of Liebowitz's investigation "indicate that Bell is an ideologist because his writings . . . serve to rationalize the necessity for creating a new public philosophy" (p. 249).

But as Bell attempts to justify that new

public philosophy, his work develops many contradictions and tensions. Chief among them is the tension between Deweyian optimism and Niebuhrian pessimism, between the Hellenic ("spontaneity of consciousness") and the Hebraic ("strictness of conscience") traditions. Throughout his intellectual odyssey, Bell remains a conciliator. He is an intellectual who has tirelessly worked at reconciling contradictions and tensions in his own life and in American society, culture, and politics.

Despite the importance of its subject matter, Liebowitz's book suffers from several flaws. First, the book's focus is not always clear. For example, in his introduction, Liebowitz first calls the question of whether Bell is an ideologist the "central problem" and then a few pages later describes it as "not the most important" question (pp. 1, 7). Then, near the end of the book, he says the question has relevance "only to the extent that efforts to resolve it provide a useful and (probably) necessary framework for discerning the contradictions and tensions that frame [Bell's] efforts to develop a philosophy of the public household. . . ." (p. 243). And at the end, Liebowitz acknowledges that his study is "aimed less toward the resolution of the problem of whether Bell is an ideologist and more toward understanding the contradictions and tensions that frame his work. . . ." (p. 249). But why not proceed to analyze the tensions rather than bothering with a question which does not really matter? Indeed, when Liebowitz focuses on the question of Bell's ideologism he often loses the point and falls into conceptual nit-picking about the terms "ideology" and "utopia," as well as truisms about the ideological nature of social and political thought.

Second, Liebowitz fails to be sufficiently critical of Bell. He does not fully explore some of the contradictions that tend to undermine Bell's theoretical perspective. For instance, how does Bell's antipathy for the revolution of rising entitlements fit with his acceptance of pluralism, planning, and the broker state? And how can Bell legitimately claim to be a "socialist in economics" when welfare state capitalism is all we can (or even should) hope for?

Despite the flaws, Liebowitz's book helps set the terms of debate for theorists pondering the fate of contemporary liberalism. Indeed, the virtue of the book is that it provides an explication of some leading ideas (shared by both neoconservatives and neoliberals) that

must be acknowledged as we search for a new public philosophy.

LEONARD WILLIAMS

Manchester College

The Rise and Fall of Economic Justice, and Other Essays. By C. B. Macpherson. (New York: Oxford University Press, 1985. Pp. iv + 154. \$19.95.)

These essays from the past decade are all "concerned in one way or another with state, class, and property—the essential constituents of the theory of possessive individualism." This statement from the author's preface might lead the casual reader to expect what is by now the standard Marxian critique of liberalism. In fact, the essays rather highlight C. B. Macpherson's original contribution to democratic theory; that is, the uniquely distinctive blend of Marxism and classical liberalism that should properly be associated with his name.

In an essay on theories of the state, Macpherson divides political theorists into three categories: (1) "those who on the whole accept and uphold the existing liberal-democratic society and state. . ."; (2) "those who accept and would promote the normative values that were read into the liberal-democratic society and stated by J. S. Mill and the nineteenth- and twentieth-century idealist theorists, but who reject the present liberal-democratic society and state for having failed to live up to those values"; and (3) "those who reject both the idealist normative theory and the present liberal-democratic society and state, and would replace both of them totally by Marxian theory and practice" (p. 56). His own message to political scientists, theorists, and citizens is simple and clear: "I place myself in category 2, and because I believe that some contemporary liberal theorists are inclined to move from category 1 to 2 . . . I shall . . . preach to them. A preacher must have a message. My message is, learn from those in category 3" (p. 62).

Almost every one of these 12 essays constitutes an effort to show idealist liberals what can be learned from Marx and various contemporary Marxians, whether the topic is private property, the state, human rights, economic democracy, pluralism, or neoclassical economics. In a sense these are all the same topic. Mac-

pherson's chief complaint against contemporary liberalism of all kinds is its immersion in irrelevantly conceived categories drawn from what Marx called bourgeois economics, such as the concept of the rational economic decision maker, or of "trade-offs" between social utilities, or of equilibrium models, or of market relations as a description of social reality. "Twentieth-century economics has . . . rendered itself incapable of illuminating political theory. Economic ideas which are confined to relations between things, or to relations between disembodied persons who appear only as the holders of demand schedules, cannot enter into political theory at any fundamental level, since political theory is about relations of dependence and control between people" (p. 102). Specifically, idealist liberals are generally in agreement (with Marx) that "the human essence is to be realized fully only in free, conscious, creative activity"; and that "human beings have a greater capacity for this than has ever hitherto been allowed to develop." But from Mill onward they have failed to grasp "that a capitalist society denies this essential humanity to most of its inhabitants, in that it reduces human capacities to a commodity which, even when it fetches its exchange value in a free competitive market, receives less than it adds to the value of the product, thus increasing the mass of capital, and capital's ability to dominate those whose labour it buys" (p. 64).

What Macpherson brings from liberalism to Marx is just as important as what he brings from Marx to liberalism, in this version of democratic theory. The most significant elements are an emphasis (shared with Christian Bay) on human rights and civil liberties; a vindication of property right legitimated as a human (welfare) right in a broad Lockean sense ("lives, liberties, and estates") rather than in the narrowly exclusivist and possessive form it has since taken on; and an idealist-liberal version of participatory democracy, which he calls "developmental pluralism," and which would signify "the transcendence of capitalism."

It would be misleading not to mention the serious difficulty for historical and theoretical analysis inherent in this blending of disparate, even opposed, elements. Macpherson himself notes that "twentieth-century liberal-democratic theory comes off badly" because it

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fails to think "in terms of laws of motion." He never makes this mistake, and each of his essays on the revival of some component of idealist liberalism concludes with an evaluation of whether "the laws of motion" are moving capitalist (or non-capitalist) societies in the desired direction. But his conclusions are neither very helpful nor always consistent. With respect to the broadening of property right, his contention that courts and legislatures in various capitalist societies have been engaged in piecemeal extensions of its sphere is familiar by now. However, the implication he draws from that material has been sharply questioned by many jurisprudentialists, who would argue that the expansive tendency is already in decline, and that furthermore any version of property right in a capitalist society is in the end supportive more of the worldview of unrestrained accumulation than of the idealist liberal version of "human rights." Otherwise, Macpherson's laws of motion give us—with respect to, for example, human rights—the prognosis that "the prospects are not good anywhere . . . there is only one ray of hope for the future of human rights. That is that we in the West may realize before it is too late that the expectation of our sustained economic growth is a delusion." As for industrial democracy, its future depends, "directly or indirectly, on the probability of the increasing incompetence of capitalism."

Again, "can any of the values of original seventeenth-century liberalism be saved?" Only if "the strength and mobilization of the democratic forces" prevents "relapse into a less democratic or anti-democratic state." And again, "it would be idle to speculate on the possible extent and timing of what I have loosely called the transcendence of capitalism," but "the incompetence of late twentieth-century capitalism works for us." As laws of motion go, in sum, this one is less than powerful. To be sure, these are only occasional essays; none of them is a full-length analysis of its subject. But it does not seem likely that, even at greater length, Macpherson would have a more satisfying account than we find here, of a specifically progressive dynamic inherent in "incompetent capitalism"; a dynamic that might lead us toward a more human society, rather than a competently bureaucratic autocracy or a catastrophic descent into barbarism.

Of course this difficulty is not Macpherson's alone. It is common to all contemporary democratic theorists who are not orthodox Marxists. Marx himself had a dogmatic and obsolescent understanding of the scientific character of "scientific laws of motion," but he derived from it a surpassingly coherent and inspiring theory of social change. His discussion of how capitalism, in the course of its development, gestates a protosocialist society that will ultimately rupture and transcend it may have been (so far) inaccurate, but it remains the model of analysis for all visionary social thought. Contemporary democratic theorists have achieved a more sophisticated understanding of what constitutes "science," but at the cost of being left with a much cloudier and much less compelling vision of how progressive social change might come about. Given that for now insuperable limitation, Macpherson does democratic theory as excitingly as it has been done for several decades. Both those who have followed the previous trajectory of his work, and those to whom it is unfamiliar can only benefit from a reading of these essays.

PHILIP GREEN

Smith College

Inalienable Rights: A Defense. By Diana T. Meyers. (New York: Columbia University Press, 1985. Pp. ix + 215. \$35.00.)

This is a bold and ambitious book. While the prose is somewhat turgid, it is clearly argued and well organized. Without overreaching, the book shows how much can be done with some carefully pruned intuitions about particular cases—once the argument is raised to a certain level of abstraction.

Meyers begins with the notion of what would be required for a moral system not to be "self-defeating." She employs that starting point to develop two necessary conditions for inalienable rights: (1) an inalienable right has an object that it is never obligatory, though it may be supererogatory, to sacrifice altruistically; and (2) an inalienable right protects a good that individuals require in order to function as moral agents; that is, to choose and to adhere to a code of conduct sensitive to the interests of others (p. 52).

From these two conditions, she makes a plausible argument that there are at least four rights which should be considered inalienable: (1) the right to life, i.e., the right not to be killed; (2) the right to personal liberty—that is, the right not to be forced to execute another person's dictates; (3) the right to benign treatment—that is, the right not to suffer gratuitous acute pain; and (4) the right to satisfaction of basic needs—that is, the right to adequate food, water, clothing, shelter, and medical treatment for survival. Each of these rights seems to define a necessary condition for a person continuing to function as a moral agent. Furthermore, to give up any of them would seem to constitute a sacrifice so great that to do so would usually be considered heroic, supererogatory, or "beyond the call of duty."

However, the connection between these conclusions and the more basic requirement that a moral system not be self-defeating is open to serious question. Meyers tells us that one way in which a moral system can be self-defeating is when "its prescriptions eventuate in circumstances that preclude further moral activity altogether" (p. 37). To define "self-defeating" this broadly opens the door to major difficulties. Let us imagine that some systems include what might be called a system-saving duty; i.e., that when the society is in great peril, it becomes obligatory for anyone who can make a crucial difference to sacrifice, if necessary, any of his rights on Meyers's list. Under conditions of such danger, any moral system which did not include the system-saving duties would be self-defeating. But including such system-saving duties would render the sacrifice of those rights obligatory (or a matter of moral requirement) and not supererogatory (heroic) as Meyers requires. In other words, to accept Meyers's proposed rights as inalienable—so that they are invulnerable to sacrifice according to the system-saving duties—would, in itself, render the system self-defeating on this scenario. The self-defeating condition does not have the implications Meyers alleges. However, even if this critique is correct, there is a great deal of value in the book. It should be read by anyone who wants a fresh approach to the justification of human rights, natural rights, or as Meyers terms them, "inalienable rights."

JAMES S. FISHKIN

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Hegel's Dialectic and its Criticism. By Michael Rosen. (Cambridge: Cambridge University Press, 1982. Pp. xiii + 190. \$27.95, cloth; \$12.95, paper.)

Hegel's Dialectic and its Criticism is a rare and most welcome addition to the discussion of Hegelian philosophy—an argument against Hegel based on a careful and sympathetic reading of his most difficult and speculative texts. Michael Rosen's evaluation of Hegel's philosophy is clear and forceful. Hegel "has, in the highest measure, two out of the three cardinal philosophical virtues: he is *rigorous* and he is *original*. But he is *wrong*, and those very virtues ensure that he is thoroughly and consistently so" (p. 179). Rosen's appreciation of Hegel's rigour sets him apart from the majority of Hegel's detractors. His willingness to address and evaluate Hegel's systematic claims sets him apart from the majority of Hegel's contemporary admirers. His book is thus an original and refreshing re-evaluation of Hegel's philosophic enterprise.

Rosen makes his case against Hegel by trying to demonstrate that we cannot make sense of Hegelian dialectic apart from the speculative idealism upon which Hegel himself founded his arguments. The repeated attempts by neo-Marxists and contemporary hermeneuticists to separate the living kernels of Hegelian philosophy from its dead metaphysical husk are, he argues, doomed to incoherence and inconsistency. Of particular interest to students of social and political thought will be Rosen's powerful critique of Charles Taylor's "expressivist" interpretation of Hegel's philosophy of spirit (chap. 5) and of Theodor Adorno's attempt to construct a materialist "negative dialectics" (chap. 7). "The interpretation of Hegel is too important to be left to Hegelians," Rosen suggests; for, by attempting to demystify Hegel, his admirers only succeed in mystifying their own arguments (p. 180). The reluctance of Hegel's contemporary admirers to part with common sense may make their reformulations of Hegelian ideas less discordant to unmetaphysical modern ears, but only by rendering those ideas even less coherent and consistent than in their original formulation.

Rosen presents a very strong case against contemporary reappropriations of Hegel's arguments. That case may not be decisive, but nowhere near as strong or as rigorous a case has yet been made on the other side. All too

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often Hegel's revisers concentrate on realizing or demystifying Hegelian concepts without asking whether one can still invoke such concepts outside of an expressly idealistic system of thought. Hegel's contemporary admirers "have been reluctant to commit themselves to the full-blooded neo-Idealism that would make such ideas as 'milieu' or 'basic question' epistemologically fundamental, and so have tended to use the concepts without explicit justification, or else to salve their materialist consciences by renaming them (as *epistemes* or *problematiques*, for example)" (p. 8).

I find Rosen's critique of Hegel, however, less compelling than his critique of Hegel's interpreters. Rosen's interpretation of Hegel's philosophy is very persuasive. Indeed, this book provides a much better introduction to Hegel's works than most written by his admirers. But Rosen proceeds as if demonstrating that the fact that Hegelian philosophy is inextricably linked to a form of speculative idealism proves its inadequacy. I shall not dwell on this point, since most of Hegel's contemporary readers admire him in spite of, rather than because of his philosophic idealism.

But I also disagree with Rosen's claim that "nothing" in Hegelian philosophy is still "living" for those of us who do not subscribe to his brand of speculative idealism. Indeed, I would argue that we can learn more from Rosen's Hegel than from the less outrageous neo-Marxist or expressivist Hegel. The former challenges us with original ideas, while the latter tends to lend authority to familiar opinions. Nothing in Rosen's argument rules out the possibility that Hegel may have developed insights into ourselves and our world that we cannot obtain anywhere else. That he derives these insights by speculative means should lead us to wonder about the limitations of our own—presumably, non-speculative—standards of judgment. As long as Hegel's texts afford us such insights there will be something living in Hegelian philosophy.

By exposing the inconsistencies in the attempts to realize Hegelian dialectic, Rosen forces us to confront the Hegel who consciously sacrificed common sense in the name of philosophic consistency. In this way, Rosen serves Hegel far better than most of his friends and admirers. This bold and insightful book deserves attention from both Hegel's admirers

and his detractors. It has much to say to both groups.

BERNARD YACK

Princeton University

Policy, Power and Order: The Persistence of Economic Problems in Capitalist States. By Kerry Schott. (New Haven and London: Yale University Press, 1984. Pp. ix + 206. \$20.00, cloth).

Who can doubt the poverty of contemporary economic policy prescriptions for the advanced capitalist nations? Schott, however, is less interested in debunking the policies than in correcting the source of their failure. The result is a stunning combination of rational choice and class analysis and one of the best books recently published in political economy.

Schott begins with an attack on economists' theories of policy. She takes both the neo-classical, including monetarist, and neo-Keynesian advocates to task for failing to understand that the success of their policies depends crucially on the responsiveness of the economy, which in turn depends on the relative power and compliance of powerful interest groups in the society. Her assessment of changes in working-class power since World War II and her analysis of the links between those changes and macroeconomic performance lead her to conclude that the working class has grown more powerful in all the advanced capitalist countries and that its power varies across time and country. The first finding implies that wages and prices are likely to be "sticky in a downward direction" (p. 26), which means that neo-classical policies will be ineffective. Even in countries such as the U.S., where the working class is relatively weak, there will be little fall in prices or real wages. From the second finding she concludes that neo-Keynesian expansionary policies will be effective only where the working class consents to an incomes policy; that is, where the working class is *both* strong and centralized, where it has developed what Mancur Olson labeled encompassing interest groups.

Schott's major task is reconstruction, but she first debunks the current theories of the state on which so many economic policy pre-

scriptions rest. The contemporary policy model inconsistently assumes self-interested actors and a public-spirited state. The Marxists wrongly assume that all power rests with capital. The so-called mainstream economic theories, including public choice, do not pay "... heed to the importance of interest groups and their changing power resources over time and across countries" (p. 121).

Schott's alternative model draws on rational choice to analyze politics in which the major actors are interest groups: capitalists, workers, and the state. For Schott, the state's principal role is to promote social order, but it is also a self-interested actor in the policy game. Using a static, three-person, Nash bargaining model she illustrates how the major groups can maximize payoffs without conflict. She then develops a dynamic model of the trade-off between worker consumption and capitalist investment over time, drawing heavily on the work of Kevin Lancaster.

Her use of bargaining models produces some extremely interesting implications and hypotheses. First, she concludes that even a self-interested "state cannot be an unconstrained Leviathan, as such behavior would conflict with its overriding objective of order maintenance" (p. 159). Second, she argues that the more powerful the working class, the more likely it is to practice "rational quiescence," if it can expect long-term gains from capitalist investment and consequent economic growth.

Third, she hypothesizes a relationship between the levels of militancy and investment. Finally, she claims that a restructuring of the economy will be most possible where the working class is weak, but difficult even then due to the state's incapacities in a stagnant economy. Her economic predictions for the advanced capitalist world are dismal, except possibly where there is an encompassing working class; namely Austria, Norway, Sweden, Japan, and Switzerland.

Schott is not totally successful in linking working-class power and macroeconomic performance. She treats minor Marxist scholars and major writers equally while neglecting some of the more relevant American radicals scholars (e.g., Bowles, Edwards, Gordon, Reich, and Weisskopf). Most importantly, the state seems to drop from her dynamic analysis except as a mediator of conflict; it is no longer one of the key policy actors.

Kerry Schott has given us a book that is both numerate and literate. Statistical and mathematical analyses enrich the text while quotes from Auden set the tone for each chapter. But her most important contribution is a provocative new approach to understanding the economic problems of the advanced capitalist world.

MARGARET LEVI

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AMERICAN POLITICS

Assessing Tax Reform. By Henry J. Aaron and Harvey Galper. (Washington, D.C.: The Brookings Institution, 1985. Pp. xii + 145. \$22.95, cloth; \$8.95, paper.)

Political Origins of the U.S. Income Tax. By Jerold L. Waltman. (Jackson, Miss.: University Press of Mississippi, 1985. Pp. x + 124. \$12.50, cloth)

Jerold Waltman is a political scientist concerned with the politics of the early income tax. Henry Aaron and Harvey Galper are economists concerned with the complexity of

the contemporary income tax as a tool of fiscal policy. Although these perspectives and projects vary, the implications of their analyses ultimately converge on a fundamental issue for tax policy and politics.

Waltman's slim book leaves the reader wishing for more. Chapters 1-4 contain outlines of the politics surrounding the first four 20th-century revenue bills that included an income tax (1913, 1917, 1918, 1921). Chapter 5 provides the basic argument of the book. Waltman believes the income tax should be understood as a function of multiple political processes,

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and suggests different political models to explain different provisions of the internal revenue code. For example, the politics of a broad redistributive issue such as the setting of rates involves political actors situated at various points in the political system, and argumentation that is different from that involved in the opening of a hole in the regular tax structure to benefit a particular subset of taxpayers. Understanding the income tax consequently requires dividing the internal revenue code into types of provisions and exploring the varied political dynamics associated with each subset of the tax.

This conclusion is persuasive, hence the desire for more. An application of these models could be quite interesting. Waltman describes each of his models of tax politics in a paragraph and illustrates them with historical examples. These models could be the basis for a systematic breakdown of the internal revenue code, and analyzing the politics of each category could enrich the description as well as test the validity of the approach. Applications over time could reveal changes in the dynamic of tax politics and policy that would be a fundamental advance in our knowledge of the development of the income tax.

Aaron and Galper's book is inspired by recent proposals to reform taxes by trading off base-broadening for lower nominal rates. They describe the problems of the current tax in Chapter 1, then analyze criteria for reform and the proposals commonly referred to as Treasury I, Bradley-Gephardt, and Kemp-Kasten in Chapters 2 and 3. In Chapter 4 their alternative, the cash flow income tax, is outlined in some detail. Chapter 5 considers the expanded use of consumption taxes, Chapter 6 explores short term options for reducing the deficit. Chapter 7 is a primer on the realities of tax politics and the obstacles in the way of tax reform. Their argument is built on several observations: (1) tax reform cannot be a placebo for the deficit problem, and must address the shortfall of revenue in this country, (2) strong political forces oppose sensible tax reform and proposals must be devised to avoid the ill effects of these interests, (3) U.S. political institutions and procedures exacerbate the problems of tax reform and steps must be taken to ensure that any gains are not subsequently lost. These observations of political reality place this effort far above most discus-

sions of tax reform, but the authors are more cognizant of such obstacles in the status quo than in their own proposals.

The cash-flow concept aims to tax income when consumed, with the result that income is taxed no more than once during the recipient's lifetime, but all income is taxed at the full nominal rate. The authors argue that this approach will remove almost all of the current inequities and distortions of the income tax. Further, it will also reduce incentives to avoid taxation by reducing marginal rates. This is an attractive proposal, but it places tremendous weight on accurate recording and reporting of transactions. Historically, the effectiveness of the income tax in producing revenue in this country and elsewhere has been closely associated with developing systems of stoppage at source; i.e., by taking accounting and compliance decisions out of the hands of taxpayers. Avoidance and evasion are functions of opportunity as well as of marginal rates. Most of the current problems with avoidance and evasion occur where income is not subject to withholding, and resistance to extending this system suggests that the beneficiaries of these schemes understand the advantages of doing their own accounting. A complete accounting and reporting system involving all financial institutions would provide the information required by the cash flow income tax, but the enactment of such legislation is not likely. Without it this proposal may invite increased evasion and consequently decreased revenues.

A more fundamental issue lies beneath the surface of both books. The current eroded state of the tax base is explained by Waltman's analysis. There are many different tax politics operating simultaneously, hence the morass of tax policy is ultimately a function of lack of coordination. The Aaron and Galper proposal would simplify tax policy in a way that should force it into a single political arena. It would be as if Waltman's varieties of tax politics were collapsed into one. This is necessary for comprehensive tax policy making, but is it realistic given current political institutions? Aaron and Galper recognize the importance of unifying tax decision making, and suggest that the President must take a greater role, that reform must come in a single comprehensive bill, that limitations must be placed on amendments, etc. But will attention to marginal changes in procedures obviate the complexity of political

institutions that has produced the problems of tax policy in this country? The combination of these analyses suggests that fundamental reform of political institutions is a necessary prerequisite for meaningful tax reform. Aaron and Galper demonstrate that comprehensive plans for tax reform are available; what is lacking is a political system that can enact them.

JAMES L. CURTIS

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Religion on Capitol Hill: Myths and Realities.

By Peter L. Benson and Dorothy L. Williams. (San Francisco: Harper & Row, 1982. Pp. xii + 218. No price.)

Religious beliefs have predictable consequences for political attitudes and actions. This argument gained considerable credibility through the San Francisco Bay area studies of the 1960s, probably best known to political scientists from Charles Glock and Rodney Stark's *Religious Beliefs and Anti-Semitism*. Two decades of research have generally shown that the more orthodox the (Christian) religious beliefs, the more conservative the politics. Yet critics have noted a variety of measurement problems. When appropriate controls are exercised, Hoge argues in a recent summary of these two decades, we can clearly see that degree of orthodoxy and salience of religion are related to traditional sexual ethics and family values, but orthodoxy and salience show weak relationships to economic policies and political attitudes.

The fundamental problem in the Glock-Stark tradition, to paraphrase Yinger, is that scholars examine to what extent a person is religious, but fail to uncover *how* a person is religious. In short, they deal with the manifest doctrinal symbols of a person's public self, but fail to penetrate the foundational beliefs that shape perceptions of both the world and the "realities" that ultimately matter to people. About the same time Yinger wrote, Clifford Geertz, the eminent anthropologist, offered a conception of religious phenomena that some scholars tried to translate into appropriate measures.

This little book, *Religion on Capitol Hill: Myths and Realities* by Peter Benson and Dorothy Williams, is the first successful effort

to overcome the problems of the Glock-Stark studies, to put the Geertz conceptualization to work in measures and analytical reductions, and to link foundational religious beliefs with political ideology and public policy positions *through behavioral dependent variables*.

Benson and Williams toil at a Minneapolis-based research institute well known in church and sociology of religion circles but little known among political scientists. This study, done on a grant from the National Endowment for Humanities, involved 80 completed interviews with demographically controlled sample of Senators and members of the Congress in 1980. A ten-member advisory committee of respected Capitol Hill people helped them gain access to respondents. The semi-structured interviews lasted 35 minutes and were recorded. Information was coded into theoretically-informed categories, resulting in 124 variables including scales and typologies.

The book debunks a number of stereotypes offered by the New Religious Right: that Congress is a hotbed of secular humanism; that its members are less religious than the people they serve; that political conservatives are more religious than political liberals; that apart from abortion and school prayer, members' religious beliefs have little impact on how they vote; that Evangelical Christians form a united conservative front in the two houses; and that members of Congress who affirm fundamental Christian values and practices support the conservative legislative agenda of the New Religious Right, while secular humanists support liberal legislation. The design and data are quite convincing in dismissing these stereotypes and, thus, offer those politicians and members of the press corps who are interested in it an accurate picture of politics and religion on Capitol Hill. Manifest destiny and election by God do not yield a uniform legislative blueprint.

But that is not the significance of this book. Its principal contribution is to scholars who want to untangle the culture/religion/politics nexus. Through a series of interlocking chapters deeply rooted in Geertz' conception of religion, Benson and Williams develop a typology of six types of religionists: legalistic, self-concerned, integrated, people-concerned, non-traditional, and nominal. These typologies grow out of the respondents' pictures of God, descriptions of ethical principles and

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responsibilities, theological emphases, and the importance of religion and religious institutions in public life. Most importantly they derive from themes that appear in answers about the fundamental problems of human existence, the process of overcoming problems, and the outcomes. Themes recur along four dimensions: agentic, communal, vertical, horizontal, restricting, releasing, and comforting, challenging. Self-concerned religionists score high on the agentic-vertical-comforting end, for example, while integrated and people-concerned religionists score moderate or high in the opposite direction. (Interestingly, denominational affiliation predicts poorly to foundational beliefs; the latter exist beyond dogmatic symbols.) Benson and Williams then develop a series of theoretically-based predictions linking these types to political ideology and to votes on specific foreign and domestic legislation, identified by others as significant roll calls. Predictions are sustained, and the magnitude of relationships is rather large, suggesting that the results are more than a fluke of the sample. The authors conclude with a discussion of three theoretical frameworks that could interpret the language between religion and politics.

I look for this book to be seminal, a point of departure for those political scientists who think religion is every bit as important as class (or interest) in understanding American politics. There are nagging problems. The coding scheme does not always make intelligible to the reader the classification decisions. For those interested in replication on mass populations, the semi-structured and recorded interview procedure may be ineffective and inefficient. But it may be possible to translate the lessons of the study into standardized, yet ipsative instruments. And, if so, the case might eventually be mounted for their inclusion on national surveys.

The most serious problem, however, has been the unavailability of the book. The publisher, in effect, gave the market 15 months to buy, failed to advertise it widely among political scientists (note that only now is a late-1982 release being reviewed in the *APSR*), and pulled it off the market just before teachers could make classroom adoptions. Fortunately, the book will be rereleased in paperback by Oxford University Press (New York) in August 1986. It should receive wide classroom use not

only in religion and politics courses, but as collateral reading in legislative behavior, voting, and public opinion, and perhaps even American government course.

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Schooling and Work in the Democratic State.

By Martin Carnoy and Henry M. Levin.
(Stanford: Stanford University Press, 1984.
Pp. 307. \$32.50, cloth; \$10.95, paper.)

This book describes the relationship between schools and the workplace to each other and to the state. It seeks to generalize about these relationships in advanced industrial democracies, but restricts most of its illustrative material to the United States. Its theoretical predilections are to reject traditional and Marxian views. They believe traditional views consider schools as autonomous institutions whose goals (equality and social mobility) can be pursued despite the inequities of capitalist societies. Traditional Marxism holds that schools have a state designated function of providing labor to be exploited by capitalism.

I have no problem with the authors' description of what schools do according to traditional Marxism, but I do not believe that there is widespread support (among political scientists) for the "traditional" view. This view is more characteristic of the education "establishment," which argues polemically for independent schools. Political scientists, on the contrary, generally believe that schools do indeed perform political services for the state. They indoctrinate each generation with the values of the previous one (or at least try to); they introduce children to the essential idea that power is often arbitrary; and they train future elites and future followers, each according to his or her station.

The authors do not speak much of this sort of theory, preferring instead to argue about the state as an arena in which two politically opposed points of view compete for primacy. One set of political forces argues for equity and democracy, the other for the creation of wealth, irrespective of its distribution. They assert that at any given moment either side

may be dominant, thus negating static traditional or Marxist perspectives. During the great egalitarian reforms of the late 1960s, clearly the egalitarians were in command and schools were forced to become agents of social change, a role for which they were ill suited. During the 1980s, the second set of political values are making a comeback, and schools are being asked to stop seeking equity and start producing skilled workers. The battle is never completely won, and historically each group wins some and loses some. Here again I found much to be compelling and much to be troublesome. Take the values of the first group—equity and democracy. Although these two goals are frequently part of the same dogma, they are really incompatible. More democracy rarely leads to more equity.

The concept of work links schools, workplaces, and the state—a compelling way to describe the functions of each. Schools and workplaces are dominated by respect for expertise and for formal authority. However, the claim that schools are more democratic and more equal than the workplace seems a bit overdrawn. Schools are not governed very democratically, indeed the reform movement so well treated in this book made sure of that. The authors deal with virtually none of the governance literature. Had they done so, I suspect that they would not have been so generous in their praise. For example, they speak of schools as independent from “the direct control of capitalist firms” (p. 154), but do not carry the argument to its logical conclusion: schools are independent of political control too. True, they are not as independent as private corporations, but they are a lot more independent than many other public enterprises.

The analysis of the workplace is a good deal more rewarding, both because it is more comparative and because it is more informed by recent research. Students cannot strike, but employees can. Their prediction that national and industry-wide unions will diminish seems reasonable; the recent failure of nationally originated strikes supports the authors’ argument quite well. Especially intriguing is that the economic conditions of the 1970s demanded an increased role for the state. The failure of the state to alleviate economic problems contributed to the defeat of an incumbent president by Ronald Reagan: “The 1980 election

represented, by and large, not so much a rejection of the democratic and egalitarian gains of the previous decades as a demand for respite from their costs and a request for economic growth and jobs with stable prices. The two are coupled because Americans seem to believe that economic growth is still the single biggest contributor to solving social problems” (p. 253). I think that the authors are imposing an interpretation upon the election that substantially exaggerates the information-processing abilities of the average voter, but nevertheless it is plausible.

The authors believe that Reagan’s “neoconservative” policies strike “deep roots” in the American character. Unfortunately, the authors do not follow this point with evidence, but rather revert to the process of imposing sophisticated interpretations upon an unsophisticated electorate. Still, the arguments are intriguing. Take for example the conclusion that the Reagan administration’s determination to increase military expenditures favors white, male, highly skilled labor. The “militarization of the economy” (p. 257) means discrimination in the labor force—“racism and sexism.” This compelling argument has yet to penetrate the debate, but this book may remedy that situation. Once having made such a provocative argument, however, the authors once again slip into unsupported rhetoric.

The authors do not like Reaganomics and think that schools can be enlisted in the struggle against the obsessive interest of the current administration in wealth acquisition rather than its distribution. “Democratic struggles for just and meaningful schooling are effective counters to the economic forces that are attempting to gain primacy over American schools and the formation of our youth” (p. 367). This is so because Carnoy and Levin believe schools to be more democratic than the workplace and “many other institutions” (p. 108). Perhaps the argument is in need of a clear statement of what is meant by democracy. They believe democracy, at least in schools, to consist of the opportunity to participate in decisions and of the ability of schools to assist in upward mobility. Democracy is thus an internal process, one consequence of which is to challenge the capitalist quest for wealth and hence of exploitation. The most important role for schools is to remain independent of the “capitalist imperative.”

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Teachers and school administrators are not conspicuous in their advocacy of resistance to capitalist ideology, however, and today's college freshmen are a good deal less liberal than was the case a decade ago. Thus schools must form coalitions with other movements for schools cannot, by themselves, induce people to escape the trap of the "attitudes and values" of the workplace (p. 109).

This book is both argumentative and optimistic. Although it has its flaws, the overall result is pleasing. The linkage between the workplace, the school, and the state, portrayed as an ever-changing one, is a welcome modification to the rigidity of traditional class-based analysis of political and social institutions. It is an interpretation which makes good sense, and allows us to think in systemic terms.

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Women as Candidates in American Politics.

By Susan Carroll. (Bloomington: Indiana University Press, 1985. Pp. xv + 236. \$25.00.)

Sometimes a book is written that is based on data so unique and rich that it almost would not matter whether the book itself is good. *Women as Candidates in American Politics* is based on just such a data set. Happily, in addition, Susan Carroll has written a good book.

The data base is the 1,212 responses to a questionnaire sent to all 1,936 women who "ran as major party candidates for state legislative, statewide, and congressional offices in primaries and/or general elections in 1986" (p. 7). The book is set in the context of democratic theory and theories of representation and seeks to identify remaining impediments to the recruitment of women to elective office. Carroll focuses on both the characteristics of women who are candidates and the characteristics that distinguish winners from losers. In the tables and text Carroll consistently disaggregates the sample according to the office sought, thus allowing comparison of state house, state senate, statewide, and congressional candidates.

Carroll identifies and compares a number of competing explanations for the low levels of women in elective office. Among these are the

often explored "internal" aspects of women's own personalities, gender role socialization, and levels of ambition; various aspects of campaign structures, resources, and processes; and levels of support offered by political parties and the structural opportunities offered by the state of the political system. Her conclusions offer reason for both hope and dismay. On the hopeful side she finds, as others have, that women's own attitudes, personalities, and ambition do not explain low levels of recruitment. She also argues that campaign structure and party support do not go far to explain women's absence. The less happy side is that even if discrimination and harmful socialization effects were to disappear today (and she does not argue they have disappeared yet), the opportunity structures of elective office would make us wait a very long time to see parity of representation. For the most part, the "new kids in town" have to wait for open seats to appear in the right places to begin to take their place in great numbers. Without special efforts to recruit women into these positions progress will continue, but it will take considerable time.

The main problem with the book is that it obviously can only go part of the way toward explaining the relative degree of success of men and women. It is, after all, a survey just of women. Although this fact should lead us to take its conclusions with a degree of caution, it remains valuable, especially in the context of the extensive literature on political recruitment.

Women as Candidates in American Politics adds significantly to the literature on women and politics. It will prove useful in courses on women and politics or with themes devoted to this topic—particularly if the book were to be published in a paperback edition. This book, however, should gain wider attention and use than this. Although the sample on which the book is based is composed only of women, its comprehensiveness in terms of the number and types of candidates questioned should make it staple reading for those interested in political recruitment, elections, and candidacies in general.

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The New Directions in American Politics.

Edited by John E. Chubb and Paul E. Peterson. (Washington, D.C.: The Brookings Institution, 1985. Pp. xv + 409. \$26.95, cloth; \$9.95, paper.)

This book is interesting and valuable. It belongs in the library of every self-respecting student of American politics. It is among the best of a new class of books that identify an important development or issue for treatment by a distinguished group of scholars. The assignment for each contributor is to apply political science lore to a particular aspect of the book's basic theme so as to contribute to the understanding of a contemporary development.

Among the books of this class, *The New American Political System* (Washington: American Enterprise Institute, 1978), edited by Anthony King, has been widely acknowledged as outstanding. In fact, a new edition is presently in the works. But many others have been published in recent years, sponsored by various institutes, centers, and universities. The quality varies, of course, but many of these collections provide very useful stocktakings as well as impressive applications of political science to contemporary issues.

In the present volume, two Brookings staff members, John E. Chubb, senior fellow, and Paul E. Peterson, director of the Governmental Studies Program, oversee analyses of the Reagan era. They invited several scholars to join Brookings senior fellows and staff in producing essays that attempt to explore and explain the remarkable changes that are occurring in American politics, government, and policy making. According to Chubb and Peterson in their introduction: "The American political system, during the presidency of Ronald Reagan, has been transformed to an extent unknown since the days of Franklin Delano Roosevelt" (p. 1). Their purpose in this book is to cast light on the causes and dynamics of this transformation.

The book is divided into two parts. Part 1 focuses on voters and elections. It includes two essays on the national parties, two on the 1984 presidential election (one devoted exclusively to economics), and one each on congressional elections and the Republican advantage in campaign finance. They are all of high quality and confirm that there is, indeed, important

change in the land. The strength of the Republican Party is demonstrated and discussed in most of the essays yet the authors are reluctant to observe an all-out realignment. Properly enough it is left to the authors of the congressional elections chapter, John A. Ferejohn and Morris P. Fiorina, to refer to "split-level realignment" in which "some voters swing between parties at different levels of government" (p. 115). More than most accounts, this set of essays acknowledges the healthy signs in the party system rather than overgeneralizing from the problems the Democrats have experienced since 1968.

Given my positive reaction to these essays, I trust the editors will forgive me for registering a critical note. Several of the chapters cover the same ground. Each has an individual contribution to make, to be sure, but there is considerable overlapping of topics. The redundancy that results is a consequence of the some fine-line distinctions in chapter topics. Thus, for example, there is probably no way for separate authors to write chapters on "The New Two-Party System," "The Republican Advantage in Campaign Finance," and "The Rise of National Parties," without repeating material. Meanwhile other topics important for the subject are not included—most notably the changing role of the media but including as well interesting developments in group participation in elections (e.g., the contrasting roles of labor and the religious right in 1984). I understand, of course, that not all topics can be covered. My only point is that some points are covered too well and that different planning would have allowed additional topics.

Part 2 is devoted to selective topics on "institutions and policy." Two essays are on national political institutions, one each on Congress and the presidency; one essay analyzes the current status of federalism; two review developments in critically important policy areas—entitlements and national security; and one tackles deficit politics—the dominant force in policy making during the Reagan era.

Readers will have favorites among these excellent essays but they will find them all interesting and useful. Each is full of supporting data for well-developed central themes. And though one may wish to have additional topics covered (the Supreme Court doesn't even make the index), there is no redundancy in this

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second part. My particular favorites are Terry M. Moe's noble effort in "The Politicized Presidency" to identify a path of development for this important institution and Paul E. Peterson's superb review of "The New Politics of Deficits."

Every such collection has at least one essay that comes to be quoted frequently because it provides new insights. Hugh Heclo's chapter in the King volume, "Issue Networks and the Executive Establishment," is a case in point. I predict Moe's chapter will capture this honor for the present volume. He undertakes the difficult task of trying to separate the person who is president from the presidency so as to establish the political and administrative context within which the occupant of the White House must operate. He marks a politicization and centralization that "have grown over time not because of who presidents are or what they stand for, but because of the nature of our institutions and the role and location of presidents within them" (p. 269). Moe sees the Reagan administration as particularly successful in directing policy making and politics from the White House. Trust me. This is a stimulating essay for you and your students.

Peterson's review of deficit politics may be appropriate as chapter "13" but I would have placed it much earlier in the book (rearranging chapters is endemic to former editors). It is surely basic to the new direction in American politics. It even dominated much of the 1984 presidential politics, if not necessarily the campaign itself. Peterson provides such a literate and common-sensical analysis of this topic that one can only hope that it is read widely in Washington and among other opinion leaders outside. He acknowledges many of the in-town explanations for the emergence of deficit politics in 1981. But he adds changes in the structure of public opinion and the effects of deficits as interpreted by professional analysts. In other words, this essay, like many in this book, fights resolutely against oversimplifying a set of very complex phenomena.

Taken together, this book and the King volume referred to earlier would make fine texts for a course on post-Watergate America. So much has happened in a decade. And works like these reassure me that political scientists have a great deal to say about "who" has gotten "what, when, how" during that time.

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The Politics of Deregulation. By Martha Derthick and Paul J. Quirk. (Washington, D.C.: The Brookings Institution, 1985. Pp. xii + 265. \$28.95, cloth; \$10.95, paper.)

Conventional wisdom in political science and economics has long held that regulatory agencies are captured by the regulated industries. Examining three recent cases of deregulation, *The Politics of Deregulation* adds substantial weight to the growing evidence against what has been termed the capture theory or the economic theory of regulation. The three cases examined in depth in this book—airlines, trucking, and telecommunications—demand study because they illustrate a rare form of politics where broad general interests are able to defeat more narrow producer interests. In each case, according to the authors, powerful interests benefited from regulation: airlines from the pricing policies of the Civil Aeronautics Board, trucking companies from the legalized cartels of the Interstate Commerce Commission, and AT&T from the lack of competition in telecommunications services and equipment.

Derthick and Quirk argue that these powerful interests were overcome by the advocates of more diffuse interests for several reasons. First, a large body of economic research detailed how consumers were hurt economically by regulation that limited competition. The conservative economic challenge to regulation in this case was consistent with the neopopulist view of Ralph Nader and others that only the industries benefited from economic regulation. Second, leaders in the executive and Congress used this consensus to press for reform of the regulatory system. These political leaders were aided by aggressive bureaucrats, particularly Alfred Kahn who deregulated portions of the airline industry by administrative action. Third, absence of the dire consequences predicted by industry then emboldened Congress which passed more far-reaching legislation in two of the three cases. Derthick and Quirk contrast the results of these three successes with five deregulation "failures," natural-gas pricing, air pollution, Davis-Bacon, milk marketing, and merchant-marine shipping.

The Politics of Deregulation is more than a carefully researched set of case studies; the cases are used to document three important theoretical arguments. First, regulatory policy responds to changes in the political and economic environment. Unlike many past studies

of regulatory policy that ignore macropolitical forces, Derthick and Quirk illustrate how economic forces such as inflation, and political forces such as consumerism, anti-government opinions, and the ideological makeup of political parties changed the results of regulatory policy. They give much credit to the politics of ideas. Second, they directly confront the argument that the behavior of individual members of Congress can best be explained by their need for reelection. They argue convincingly that the only explanation of deregulatory attempts is individual members' conceptions of what good public policy is. Third, they challenge economic notions of interest groups that hold larger groups are more difficult to organize and keep cohesive. Their cases reveal that 16,000 trucking firms were far more cohesive in support of regulation than the handful of major airlines.

Although the book is a strong effort to illustrate the commonalities of deregulation, the reader is left with the impression that each case of deregulation is idiosyncratic. Airline deregulation might easily have failed without Kahn's aggressive leadership, without the inflationary growth in the economy that permitted airlines to prosper under deregulation, and without Stephen Breyer's suggestion to Ted Kennedy to get involved in the policy area. Without airline deregulation as an example of success, trucking deregulation might not have been attempted. The telecommunications case is even more unusual in that competition was established more by courts than by Congress. In one sense the successful deregulations may share more in common with earlier efforts to establish social regulation than the five deregulation failures discussed in the book.

Some minor flaws exist but do not detract from the value of the book. General congressional coalitions for deregulation are illustrated with simple percentage figures from the Senate when the analysis needs a multivariate approach to separate out the influences of ideology, constituency interest, and other factors. In this process voting patterns in the House are ignored. The author's argument that industry benefits when it recognizes the futility of opposition and makes compromises with deregulation advocates. Trucking interests who did so, however, fared no worse than airlines who did not; and dairy interests who rarely compromise fared best.

Perhaps the most serious problem is that the telecommunications case does not fit the pattern of deregulation. Bureaucratic innovations were in trivial areas. The key innovators were the courts who permitted long distance competition via a procedural decision and who presided over the AT&T antitrust suit. One might even argue that the antitrust decision was to AT&T's long-run benefit.

These minor problems, however, do not detract from a solid, theoretically driven group of case studies. The detail of information would benefit students of Congress, interest groups, and public policy as well as those interested in the more narrow area of regulation.

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Capitalism and the American Political Ideal.

By Edward S. Greenberg. (Armonk, NY: M. E. Sharpe, 1985. Pp. 288. \$30.00, cloth; \$12.95, paper.)

Over the last fifteen years the way most political scientists write about business has changed markedly. Through the late 1960s and early 1970s, business was most commonly described as an interest group, competing with other interest groups in a highly fragmented political system. Since the mid-1970s, however, this perspective has virtually disappeared from the discipline. Now nearly every student of American politics who writes about business characterizes its political position as "privileged."

Capitalism and the American Political Ideal echoes what has become this new conventional wisdom. According to this perspective, business has not only successfully resisted each new challenge to its prerogatives, but its interests and values have themselves defined the boundaries within which these challenges have taken place. Greenberg develops this thesis by presenting a concise, but extremely well-written history of business/government relations in America from the Founding Fathers through Ronald Reagan.

Nothing in this book has not been said countless times before, though Greenberg does demonstrate an impressive ability to make this position seem fresh and interesting. Where he

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attempts an original contribution is by purporting to explain, "How . . . a large, expensive, and interventionist government with a vast and variegated range of functions and responsibilities emerged in a society so committed to the ideas of limited government." Greenberg considers four explanations; those offered by reform liberals, free-market conservatives, neo-conservatives and Marxists. He finds the latter most persuasive because it alone recognizes the critical role played by government in maintaining the imperatives of capitalist growth.

Unfortunately Greenberg's definition of Marxism is so vague that he never really tests its explanatory powers. Granted that America is still a capitalist system, dominated by a number of large companies, it does not necessarily follow that each significant expansion of the role of government over the last century has been functional for American business. Perhaps had government been either more or less active—or intervened in different ways—American corporations might be more internationally competitive than they are now. Greenberg's analysis also does not explain why the American government is still less interventionist than any other capitalist state.

The principal strength of this book is its historical narrative. Its primary weakness lies in its treatment of business/government relations over the last two decades. Greenberg writes knowledgeably about the dynamics of reform during the Progressive Era and the New Deal. But, curiously, he completely ignores this century's most recent reform period—the one that began in the mid-1960s and ended in the late-1970s. That decade witnesses a geometric expansion in the scope of government control over corporate decisions in the areas of consumer and environmental protection, personnel policies, and occupational health and safety. In fact, no other capitalist nation has witnessed a more determined—or more successful—effort by trade unions and public interest groups to limit the ability of managers to determine what was produced, under what conditions, where, and by whom.

Greenberg overlooks this recent challenge to management prerogatives, for to do so would require him to question the central assumption of his book, namely that business has always been politically dominant in America. Greenberg also cannot make sense of the resurgence

of corporate power since the late-1970s, since his analysis prevents him from recognizing the extent to which it ever declined. As a result, his concluding discussion of the collapse of corporate liberalism and the triumph of Reaganism is the weakest part of the book.

Greenberg's analysis of contemporary economic developments is equally static. For example, his figures on corporate concentration do not take into account the penetration of virtually all domestic markets by foreign producers over the last 15 years. He attributes inflation to the ability of corporations to control the prices they charge, but then says nothing about the extraordinary price stability of the American economy since 1980. He claims in a footnote that businessmen "have remained impressively silent about traditional regulatory policies beneficial to (their) market domination," thus overlooking the deregulation of markets in telecommunications, banking, airlines and trucking since the late-1970s. And his view that American producers are unwilling to engage in highly competitive practices ignores both the entrepreneurial explosion that the United States has experienced since the early-1970s as well as the unprecedented number of bankruptcies and hostile takeovers since 1980.

In just two decades, political scientists have gone from systematically minimizing the political power of business to grossly exaggerating it. Hopefully over the next decade, we can begin to formulate a more balanced—and accurate—appraisal.

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The Politics of Insurgency: The Farm Worker Movement in the 1960s. By Craig Jenkins. (New York: Columbia University Press, 1985. Pp. xvi + 261. \$30.00.)

Farm Workers, Agribusiness, and the State. By Linda C. Majka and Theo J. Majka. (Philadelphia: Temple University Press, 1982. Pp. xi + 346. \$29.95.)

The case of the United Farm Workers (UFW) challenges common knowledge about interest groups in general and about labor unions in

particular. Mancur Olson's work suggests that the logic of collective inaction can only be overcome through selective benefits, but farm workers' willingness to risk imprisonment, beatings, and death during the period of initial organization does not seem to be explained by their cooperative gas station and funeral insurance. Union membership has been declining, yet the UFW was able to organize workers often considered unorganizable. And within a labor movement that has been characterized as exclusively concerned with job security and wage increases, the UFW has emphasized control issues such as labor contracting and pesticide use.

Why did the UFW succeed at creating a durable farm workers' union when previous efforts had failed? Why did the UFW adopt the approaches that differentiate it from "bread and butter" unions? *The Politics of Insurgency* and *Farm Workers, Agribusiness, and the State* offer similar answers to these questions. Both books emphasize the conjunction of organizational strategies appropriate for farm workers' internal resources with a favorable external political environment. The authors of both sympathize with farm workers and, rejecting Frances Fox Piven and Richard Cloward's arguments against the transformation of insurgent movements into permanent organizations, support the UFW. Majka and Majka provide a longer historical overview, tracing class conflict through the Chinese, Japanese, International Workers of the World (IWW), and Depression periods, as well as the UFW era. Jenkins concentrates on postwar efforts to organize farm workers and provides more detail about the failures of the National Farm Labor Union (NFLU) and the Agricultural Workers Organizing Committee (AWOC).

The authors of both books reject classical theories of social movements for the resource mobilization approach identified with sociologists such as Charles Tilly, Mayer Zald, and William Gamson. The differences are explained more fully by Jenkins, whose first chapter is a good introduction to the social movement literature. The classical model treated social movements as irrational responses to individual discontents. Within the resource mobilization approach, in contrast, "Grievances were seen as collective and derived from structural antagonisms built into

social institution. Grievances were real and significant, but the central factors explaining the emergence of social movements were the increases in organizational capacities and political opportunities, not the intensity of individual discontents" (p. xiii). Class conflict is thus inherent in agribusiness production of specialty crops; its outcome has depended upon farm workers' economic power, ethnic solidarity, and external support.

As both Jenkins and Majka and Majka show, farm workers' economic power has varied with immigration policies. For example, the bracero program of 1942-1964, which imported Mexican workers, weakened organizing efforts by increasing labor supply and by providing a pool of workers who could be used as strikebreakers. Organizing strategies are examined more closely by Jenkins, who explains how the UFW used selective, material incentives to bring workers into local units where ethnic and religious ties could generate solidarity. Students and clergy motivated by purposive incentives served as volunteers for strikes and boycotts. Jenkins traces the shifts and tensions between strategies aimed at building community organizations and strategies aimed at building external support. He shows vividly how Cesar Chavez was able to use nonviolence to turn repression against the growers and their governmental allies.

For Jenkins and for Majka and Majka, the UFW's successful boycotts and its ability to benefit from its own repression demonstrate the importance of the union's external support. While the authors of both books analyze external support in terms of state intervention, Majka and Majka do so more completely. Drawing upon neo-Marxist theories of the state, they portray the state as capable of simultaneous repression and reform, and argue that reform rechannels insurgency as it provides tangible, and perhaps structural, gains for farm workers. Their conception of the state includes such quasi-governmental institutions as the land grant college system and takes into account the division of authority between federal, California, and local governments. Within this framework, Majka and Majka explore the origins and implications for class conflict of the bracero program, the California Agricultural Labor Relations Act, and attempts to cover farm workers under national labor legislation. The termination of the bracero

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program in 1963-64 suggests the specific interconnections between state and social movement they trace. The NFLU collected information about the program's abuses, which led other unions, religious and political organizations, and, significantly, a CBS television documentary, to demand reforms. The Department of Labor responded to this pressure by enforcing rules on bracero contracts more strictly. This enforcement made the program less attractive to growers, so their opposition to efforts by liberal members of Congress to end the program was weaker than it would have been in earlier periods.

Jenkins' discussion of external support offers many of the same conclusions, but he gives less attention to California state government and tends to treat elite responses as more simplistically either pro-grower or pro-worker. For these reasons, he exaggerates the significance of "center-left" electoral success. Liberal Democratic control of the presidency and Congress after the election of 1964 did not produce gains for farm workers comparable to the new civil rights legislation of the War on Poverty. George McGovern, Harrison Williams, and Eugene McCarthy were farm worker allies at the national level, yet within California Governor Pat Brown, also a liberal Democrat, sought to continue the bracero program and ignored the UFW until his 1966 election campaign. Jenkins does, however, place the UFW's success within the context of the generalized politics of turmoil during the 1960s.

The Politics of Insurgency is marred by some factual errors. Truman's Secretary of Labor was Maurice, not James, Tobin, and he had been mayor of Boston, not New York (p. 110). No legal restrictions prevented Ronald Reagan from seeking a third term as governor of California in 1974 (p. 195). And Charles Hardin, the political scientist, is not Clifford Hardin, Nixon's first Secretary of Agriculture (p. 64). These mistakes are trivial in themselves, but they raise questions about the reliability of details less easily checked.

Overall, though, *The Politics of Insurgency* and *Farm Workers, Agribusiness, and the State* effectively combine theory with case study, anger with objective scholarship. Anyone studying farm workers will need to read both; anyone studying interest groups, labor unions, or agriculture policy will learn

from reading both. Either would be an excellent choice for courses on any of these subjects.

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The Party Goes On: The Persistence of the Two-Party System in the United States. By Xandra Kayden and Eddie Mahe, Jr. (New York: Basic Books, Inc., 1985. Pp. 256. \$17.95, cloth.)

This book is important for what it symbolizes and for what it says. It symbolizes transition from an era in which American political scientists were beguiled by the themes of party decline and party demise, to an era of recognition of the durability of the two major parties as important forces in electoral politics and perhaps even in government. The authors, a political scientist noted for her work on American political parties, and a former Deputy Chairman of the Republican National Committee ("the prototype of the modern party" [p. 5]) find that the parties command resources and have built organizational structures that enable them to exert influence over nominations, to operate as strong forces in the electoral process, and to seek to influence public policy. These are the indicators of effective political parties (p. 25).

We are told that the two national party committees resemble each other today much more than they do their predecessors of a decade or two earlier in that both perform functions not earlier attributed to parties (pp. 5, 60-61). The contemporary parties are served by "better educated and more professional staffs" than earlier, and are (or are in process of becoming) masters of new technologies for mobilizing public support (p. 87). "The resources the national party can amass and distribute today cannot be matched by any other actor in the political process..." (p. 93).

The authors exorcise three *betes noires* of contemporary parties: the private, for-hire purveyors of campaign services to candidates in competition with and possible corrosive effect upon parties; political action committees popularly thought to constrict party sources of funds and supplant party influence over nominations; and the federal legislation of the 1970s

which frequently is depicted as harmful to the parties. Campaign consultants may well have been introduced to their crafts while employed as party staff, and the parties may refer clients to them (p. 88). For a variety of reasons to be gleaned by the reader from a somewhat diffuse chapter on interest groups, the political action committees "will never dominate the parties" (p. 126). The two major parties are beneficiaries rather than victims of the Federal Election Campaign Acts.

Effective application of party resources in a flow from the top down (p. 84) enables the national parties to influence candidate selection and to shape campaign issues and government policy (pp. 129, 189). National party programs are also credited with responsibility for changing patterns of party identification (p. 177). The impact of the new order of politics on the state parties seems as yet unclear to the authors, but the new professionalism and associated centralization of control discourage volunteerism and weaken the local parties (pp. 109-10). The reader must accept much of the content of the book on the authors' authority. For the description of the contemporary national parties Kayden's scholarly record and Mahe's on-line experience justify our confidence in the accuracy and balance of their perceptions. But the reader inclines to be more exacting when informed that attributes of national party organizations are casually related to such "consequences" as centralization of party control, influence over candidate selection, and changes in party identification. Such relationships cannot be established *ex cathedra*. So, too when the authors move away from the present and seek to ground findings of change in today's parties on party conditions in an earlier time, or to hazard predictions for the future, the reader frequently would like more supporting evidence or reasoning than is offered.

The Party Goes On reconciles the conventional wisdom on the decline or demise of party with findings of strong and vibrant national parties by invoking the metaphor of death and rebirth. The national party organizations of today represent a "rebirth"—"Phoenix . . . risen from the ashes" (p. 3). This interpretation is singularly inappropriate to the national party organizations, which took on the character of continuously functioning party headquarters as recently as the 1920s. If

national party organizational strength were graphed for each party and the peaks and troughs of their experience as organizations since the 1920s were smoothed, I am convinced the result would be a curve depicting a gradual upward slope, providing little justification for the imagery of rebirth from ashes.

If, as suggested, a continuous line of development characterizes the modern history of the national party organizations, we would expect that different as the parties of today may be from those of the 1950s, the distinguishing features of the parties of the 1980s are adumbrated in the party experience of the 1950s. The authors establish the 1950s as the "base period" for examining change (pp. 60, 70), but they make no effort to present systematic data on the national or state parties of that era. Such data for the base period are available at reasonable expenditure of effort. Three sources come to mind: published studies, dissertations, and archives—most typically the party records in the Truman, Eisenhower, and Kennedy Presidential Libraries. Much of Alexander Heard's *Costs of Democracy* (Chapel Hill: University of North Carolina Press, 1960) could have been exploited toward this purpose, yet it is not referenced. Heard's chapter on "Fund-Raising and Party Cohesion" dealing with transfer of funds among party committees and raising the possibility of conceiving of "financial management as a way to develop party unity and to build a stable organizational structure" (p. 294) is in point. While he found no evidence party leaders so conceived of party finance, his survey of the experience of the 1950s evokes in the reader's mind the agency relationships among party committees reviewed in *Federal Election Commission v. Democratic Senatorial Campaign Committee*, 454 U.S. 27 (1981). The authors' finding of higher levels of education and professionalism for today's party staffs (cited above) included state committee staffs, which suggests the relevance of attempting to draw upon R. H. Ebel's dissertation on *The Role of the Professional Staff in American State Parties* (Michigan State University, 1960—see Table 1 [7], p. 114) for base-line comparison data.

Party records in the presidential libraries indicate many of the party practices of today are reflected in the party experience of the 1940s and 1950s. The Democratic National Committee (DNC) was, by the mid-1940s,

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investing in congressional campaigns (Official File, Box 940, Truman Library), and in the early 1950s the DNC was allocating funds to the Democratic Congressional Campaign Committee (Butler Files, Box 435, Kennedy Library). The DNC may have pioneered direct mail sustaining fundraising under Mitchell's leadership in the early 1950s (Mitchell Papers, Box 126, Truman Library). In summary, the tendrils of today's "modern" parties reach back into the 1950s—a happy choice of decade for comparison for a study which, if bromidic in some chapters and neglectful of opportunities in others, is compelling in its interest and significance.

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Artful Work: The Politics of Social Security Reform. By Paul Light. (New York: Random House, 1985. Pp. ix + 255. \$8.95, paper.)

Case studies have long been accepted as one of the best ways to learn and teach about the policy-making process. This study of the social security reform enacted in 1983 is based on an even broader premise, that "anyone who wants to understand American politics must first understand social security" (p. ix). The cost and number of people directly affected by social security is enough to guarantee its continuing presence on the political agenda. The electoral relevance of social security is sustained by partisan and ideological disagreements on such fundamental matters as the purpose of the program itself. But the importance of understanding social security and the value of Light's case study go beyond this. It is a study of "dedistributive" policy making, of how elected political leaders go about raising taxes and cutting benefits without cutting their own throats. To understand American politics of the 1980s, a politics of resource constraints and budget cuts, we must indeed understand what Congress and the President did about social security in 1983. Paul Light draws on his own experience as both a participant and observer along with extensive interviews with key players to describe the politics of social security reform. His account is one that will

engage the interest of students and teachers alike. It is an excellent case study, one that explains not only who got what, but when, where, and how. The book certainly has the potential for meeting another criterion put forward by its author, that of providing a lesson for political leaders faced with dedistributive policy decisions.

Another standard for judging this book, as Light suggests, is what it teaches us about American politics, about the political context of policy making. The nature of public opinion, the structure of Congress, life cycles in the presidency, and the overriding incentive of reelection are identified as factors that determine the shape of dedistributive policy making. As described by Light, this type of policy making is characterized by: the presence (or creation) or an "inescapable crisis" (p. 138); low staff involvement and limited information on members' opinions as a result of the issue being "politically hot" (p. 15); legislative action early in a session, "at the point furthest from the next campaign" (p. 15); and limited participation. In this case, public participation was limited by a number of devices that permitted policy makers to find "an occasional hiding place" (p. 5), "some shade from the sunshine" (p. 195), or even to "confuse the public with complicated packages at the subterranean levels of government" (p. 84). The number of executive and legislative participants directly engaged in working on a reform package went from the "Gang of Seventeen" involved in the 1982 budget negotiations, to the 15-member National Commission on Social Security Reform, to the ultimately successful "Gang of Nine." "One thing we learned," a White House staff member points out, "was to keep the number of players down to a minimum" (p. 146).

But this type of policy making, as Light points out, is not typical of American politics. Indeed, he characterizes passage of the social security rescue bill as "a legislative miracle" (p. 3), a term he also uses for the 1982 tax increase (p. 26). This is a case study, in other words, of an extraordinary political event. Throughout the book we are reminded that social security reform "was no longer an issue for the normal legislative process" (p. 127), that budget negotiations "had to be outside normal procedures" (p. 142), that the Gang of Seventeen was an "attempt to leave the normal

legislative process in search of a compromise on a very difficult issue" (p. 146), that the conference committee (like all conference committees) would be "outside the normal legislative process" (p. 217), and that "eventually, most of the crucial decisions came outside the normal process" (p. 233).

What can we learn from this case that will help us to understand American politics? Light draws on characteristics associated with "normal" policy making in American politics to explain this atypical case. We see this in Light's conclusion that an agreement was reached "more out of mutual fear than the public interest" (pp. 3, 192), in his committee-jurisdiction explanation of Senator Domenici's call for a "truthful budget" (pp. 149-50), and in the familiar "electoral connection" descriptions of congressional and presidential behavior in chapters two and three. But what about the guiding principle of equity that served to bring together the two sides divided over benefit cuts or tax increases (p. 172), the overlapping interests of all members in passing a bill (pp. 128, 134, 160, 204, 209, 225), and the public-interest reference in the observation of one participant that "we all had to believe that the negotiations were important for something more than immediate political gain" (p. 181)? To understand the 1983 social security reform one must be aware of this agreement on certain governing principles and shared ideas about the public interest. The greatest value of Paul Light's analysis of this legislative "miracle" might be that of leading us to look for similar patterns in other cases of redistributive policy making and in other policy areas. Instead of using a fixed, "normal" framework of American politics to explain the case, we can use case studies such as this to adjust that framework and to better understand American politics.

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The American Dream and the Popular Novel.

By Elizabeth Long. (Boston: Routledge & Kegan Paul, 1985. Pp. 243. \$22.50, cloth.)

Intent on shortening the distance between concepts and experience, Elizabeth Long impels her readers to realize that social scientists who want to comprehend the drift of

American values tend to absorb the metaphors employed by America's renowned social critics. Their metaphors refer to catch-all attitudes such as "other directedness" or "narcissism" rather than to the welter of rhythms tapped and articulated by popular novelists. Focusing on the three decades after World War II, Long has taken note of the accounts of American value systems provided especially by David Riesman, Daniel Bell, and Christopher Lasch. As frozen categories, the pivotal metaphors of their prose tell social scientists what we all regard as success and how submissively we all deal with malaise, alienation, boredom, and anxiety. Yet when one searches, as Long has done, for evidence for this state of affairs by meticulously reading best-sellers of the period, "we all" turns out to be only that precious group of upper middle-class academics whose elitism is betrayed by our nostalgia for a culture that excludes complexities expressed in music, movies, television, and popular novels—novels ranging from historical sagas and spy fiction to the tales spun by Bellow, Heller, Updike, and Mailer. Best-selling novels in particular tell a story, one summarized by Long, which is more contradictory, more equivocal, and above all more telling than the one presented as novel by the most prominent of social critics.

Not that the social critics should be dismissed. They successfully reduce the complex, intricate aspirations and predicaments of Americans to dimensions congenial to duly educated readers. The critics tell their stories in terms of autonomous, discrete categories which signify the changes from, say, an "entrepreneurial ethos" to "the spirit of conformity." They provide useful typologies however much they thereby distort cultural processes, suppress the crosscurrents that are the stuff of social change, and limit the present by schematizing it as the antithesis of a postulated past. True, they may not allow one to make sense, for example, of the popularity of Ronald Reagan or the vacuity of so-called public life. But they assuredly have their uses. With what I take to be irony, Long appreciates how the personal discontents of their authors give energy to critical inquiry: their own class-based malaise inspires partisan, ideological, polemical activity.

The argument for turning to popular novelists so as to resist the tacit reductionism of

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academia is hard to make. No wonder that almost one third of Long's concisely written book is taken up by a defense of her methodology. Her case, I am persuaded, is compelling, doubly so because she details the process of best-seller production (a sociologist now, she had worked for Simon & Schuster as well as Alfred A. Knopf, Inc.). What is more, she appeases skeptics by not advertising that social critics are no less storytellers than popular novelists, each merely following different epistemological conventions to keep readers engaged. Nor does she irritate by proclaiming, as she might, that popular novelists tell us more about ourselves than social scientists manage to accommodate during duty hours. She surely realizes that the account she provides of changes in American values can scarcely become the basis for the austere enterprises of social scientists. After all, political scientists (to select but one subculture) would have to attend to ambiguities and complexities deleted from their training and their textbooks. They would have to open their publications, classrooms, convention panels, and analytical frameworks so as to provide a home for phenomena throbbing at the margin of consciousness.

American public life offers but meager space for discussing and testing the changing circumstances of our existence, for symbolizing and enacting new definitions of success. Noting that "the novelistic landscape of private life and interiority has grown in complexity as success loses its transcendent meaning" (p. 117), Long implies that in this submerged, seemingly private sector political process remains alive. Unlike the social critics who treat the populace as a homogeneous, univocal object manipulated by institutionalized forces, the authors of best-sellers give expression precisely to a political world unseen by the custodians of high culture. In this world, moral sanctions may have faded. The boundaries between reality and fantasy may have eroded. And yet a multitude of Americans—their tenured critics notwithstanding—are ingeniously coping in various identifiable ways. They maneuver in open-ended space misleadingly called private—a space George Kateb in his recent book on Hannah Arendt envisaged as "the unstructured immensity in which people idle, observe, ruminate; imagine, move about, encounter, travel down the open road; in which, in short,

they experience." Elizabeth Long allows for the perception that it is this unaccredited political arena which is screened out by current academic practices.

Which America is more real—that depicted by the social critics or that depicted by popular novelists? The answer depends of course on one's epistemology, one's interest. The America of the popular novelists is richer and more various, certainly more difficult to manage. Suspicious of the austerity imposed by epistemologies, I'd opt for the novelists. Their fictions legitimate the enactment of richer lives, perhaps of new ways of failing. Tolerating more disorder in the midst of the prevailing order, being more generous, they hold out promise for the extension of politics and, with luck, the extension of the scope and methods of political science.

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The Arms Race: Economic and Social Consequences. By Hugh G. Mosley. (Lexington, MA: D. C. Heath and Company, 1985. Pp. xiv + 203. \$22.50, cloth.)

In view of the massive resource commitment that has been made to the defense sector in the Reagan Administration, this book addresses an important and timely subject. Its focus is suggested more accurately by its subtitle—it is concerned with the domestic economic consequences of investment in the defense sector, and is in large measure an attempt to estimate the true economic burden of military spending rather than examine the arms race per se.

The central argument made in this book is that "military Keynesianism," which the author defines loosely as "a diffuse set of ideas and assumptions about the compatibility, and even beneficial effects, of a high level of military spending and economic prosperity" (p. 5), does not produce benefits for the economy, and in fact is harmful to economic growth, employment, and price stability.

The first three chapters of the book are concerned with the historical background of military Keynesianism and with clarification of some conceptual issues regarding the measurement and burden of defense spending. In this

section, the author takes issue with several of the conventions in measuring defense benefits. For example, he disputes the use of GNP accounting in measuring the contribution of military outlays to economic growth, pointing out correctly that the military's contribution to GNP is by definition larger the larger the defense budget is, because GNP measures government output in terms of expenditure inputs. This tautological relationship between GNP and military spending gives a misleading impression of the contribution of defense to economic growth. The author also argues that the size of military spending is grossly underestimated because it excludes the budgetary burden of such items as foreign military assistance, veterans pensions, and retirement payments for the civilian employees of the Department of Defense. In calculating the burden of defense, he differentiates the budgetary burden from the societal resource burden, but concludes that both are approximately 50-70% greater than the figures reported in the annual federal budget or in the National Income and Product Accounts. The interpretations in these chapters are for the most part cogent and persuasive.

Most of the remainder of the book is devoted to a consideration of the effects of military spending on employment, inflation, growth, trade, and other economic indicators. It contains a useful survey of previous research findings to support the arguments that defense spending contributes less to economic growth and employment than equivalent amounts of expenditure for many domestic purposes; that it contributes more to inflation than do other types of spending; and that it does not promote technological innovation to the degree commonly asserted.

The evidence cited in support of the arguments tends to be drawn mostly from the literature critical of defense spending, although there is occasional reference to authors whose findings are contrary to the critical position. In some cases the conclusions seem to overstate evidence, which detracts from the argument. For example, after citing some studies that suggest a negative relationship between the military share of GNP and economic growth, and others, using different measures, suggesting a positive relationship, the author concludes that they show that "military spending... has been used as an instrument of economic

policy," a conclusion which is unsupported by the data. Mosley concludes that military spending inhibits productivity and, therefore, economic growth, relying primarily on evidence of the decline of U.S. productivity growth and high investment in military R&D, which he concedes is circumstantial at best.

The author seems to argue that military spending is inflationary because the mix of DOD purchases—particularly for specialized purposes—comes from sectors with high inflation rates. But he then shows that the inflation rates for procurement and RDT&E are just as often less than the composite inflation rate for DOD, suggesting that it is not primarily the specialized goods which are most inflationary. He also notes that military spending, like other government spending, is not necessarily inflationary if it is financed by increased tax revenues or borrowing rather than money creation. But he nonetheless concludes that "military expenditures have a strong inflationary bias, which is rooted in the political-economic characteristics of military spending" (p. 130).

Despite this tendency to overstate what the data shows, the author has provided a useful and informative survey of the critical literature on the economic impacts of defense spending.

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Images of Voting/Visions of Democracy. By Peter B. Natchez. (New York: Basic Books, Inc., 1985. Pp. xv + 287. \$21.95, cloth.)

Although vastly increasing knowledge of electoral behavior, the early voting studies failed to develop political theory about the role of the electoral process and mass participation in constitutional democracy. While advancing empirical theory with survey-based investigation, the pioneering works slighted normative inquiry. In articulating these failures to realize the promise to advance understanding of both voting behavior and democratic theory, *Images of Voting/Visions of Democracy* calls researchers and theorists to bring together philosophy and empirical analysis in the more discerning examination of politics.

Part of the problem with the early electoral studies, in Natchez's view, was their under-

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emphasizing of the truly political and governmental, while highlighting the sociological and psychological. In demonstrating the importance of social background and psychological identifications for political behavior, those studies, initiated by sociologists and social psychologist, relied on consumer behavior models and stereotypical images of a rational, informed, and activist citizenry. These ideas lacked actual roots in classical democratic theory (tied since Aristotle to the role of the Many), and blamed citizens for failing to attain a putative, individualized ideal. Political research, for Natchez, should elucidate the roles and values of leaders and citizens, and advance the electoral process in democratic society.

The book makes other suggestive contributions. It comprises an initial intellectual history and critique of the voting studies, as well as Natchez's own analysis of voting, political participation, and democracy. Chapter 3 is a fine overview of political socialization and group bases of politics. The introduction to the second part on electoral issues, which might better have extended the initial chapter, cogently identifies the theoretical failings of the predecessor works. Drawing on unpublished correspondence, chapter 7 puts in context V. O. Key's contributions to the issue bases of public opinion, voting, and popular government, while elucidating his involvement with quantitative electoral analysis.

Although provocative, the book is uneven. A major weakness consists in largely identifying the voting studies with *The People's Choice* and *Voting* from Columbia, *The Authoritarian Personality*, and *The Civic Culture*. The latter explore possibilities for (anti) democratic personality and culture, but hardly discuss voting. Though Chapter 6 critiques the Survey Research Center's failure to extend normative political theory, the book devotes little specific attention to the central voting study, *The American Voter*, and its more systemic sequel, *Elections and the Political Order*. This is all the more surprising because those efforts in certain ways fit Natchez's critique—despite touching on electoral theory, they are far more models of extraordinary normal science than of normative inquiry. Classic works forged by a quadrumvirate of scholars, only two of whom had their original training in political science, tended to approach political behavior and par-

ty identification as essentially similar to other social behaviors and attitudes.

Planned originally for two books, the history, critique, and contributions by Natchez intermix, though often without clear cumulation. The final chapter presents the author's conception of the intentional, reasonable voter. (It also discusses Milgram's study of obedience to authority in the context of mass/elite relations in a democracy, but overstates the extent of blindly following orders.) Perforce incomplete and left to able editing, the chapter suggests approaches to questions of rationality in issue and retrospective voting, and how normative concerns might integrate with political investigation.

Natchez came to study government with Key at Harvard, but the senior scholar's untimely death left the younger to pursue indirectly the lessons of intertwining philosophical and quantitative scholarship. Unfortunately, like *The Responsible Electorate*, this book had to be posthumously completed by a colleague. A poignant account by Lois Wasserspring Natchez and an introduction and editing of the final chapter by John C. Blydenburgh describe the course of the author's political studies and clarify the meaning of his work. Had Key and Natchez lived longer, visions of philosophically inspired electoral study might currently be more fully developed. This promising study should inspire political scientists and philosophers today to pursue voting research which extends beyond images of candidates, enriching the meaning and understanding of electoral democracy.

RICHARD SOBEL

Smith College

Political and Economic Migrants in America: Cubans and Mexicans. By Silvia Pedraza-Bailey. (Austin: University of Texas Press, 1985. Pp. viii + 242. \$27.00, cloth.)

Scholars of assimilation have paid little attention to the role of the state. They have focused instead on non-political social processes that may "naturally" lead toward assimilation. This is not surprising inasmuch as political scientists have not contributed sub-

stantially to the literature of assimilation, leaving it mostly to other disciplines.

Sociologist Pedraza-Bailey, a Cuban immigrant, argues convincingly that assimilation cannot be understood in a political vacuum because state policies may either inhibit or facilitate it. This thesis is developed around a comparison of Cuban and Mexican migration to the United States.

Using census data, the author tests the popular belief that migrants from Cuba have undergone more rapid assimilation than those from Mexico. Explanations for her findings center around two concepts: "economic migrants" and "political migrants." This classification is based on three criteria: whether political disaffection or economic concerns comprise migrants' primary motivation for migration, and whether the countries of origin and destination define them as economic or political migrants. According to Pedraza-Bailey, the significance of this distinction is that political migrants assimilate more rapidly because the country of destination extends more assistance to them.

The book concludes that Mexicans have long come to the United States as economic migrants, while Cubans entered as economic migrants until Castro's rise to power and as political migrants since then. Thus the author expected that among migrants who entered the United States during the Castro era, Cubans would have undergone more extensive assimilation because they were political migrants, while the Mexicans were economic migrants. For those arriving in the pre-Castro era, she expected no significant difference between Cubans and Mexicans because both came as economic migrants. Using such indexes of assimilation as job prestige and income, and controlling for differences in status at the time of arrival, both hypotheses were confirmed.

The book argues that the United States has never really tried to assimilate economic migrants, and much of its details the national government's neglect of Mexican migrants. Economic migrants are admitted to supply cheap labor, so assimilation with its accompanying rise in economic status would be self-defeating. In sharp contrast, the United States has made extraordinary efforts to assist political migrants because they are refugees from countries hostile to this country and with whom the U.S. is locked in competition for

world opinion. A very informative chapter is devoted to the numerous programs the United States designed to assist Castro-era Cuban migrants.

I have some reservations about this book. The author does not clearly indicate the methodology for determining whether motivation for migration is primarily political or economic. Nor does she tell us how to categorize migration that is political by some of the three standards but economic by the others (e.g., if the migrants' motivation is primarily economic but they are defined by the United States as political migrants). There seems to be an assumption that the Cuban-Mexican-United States situation is typical, but is it? Do the two categories of political and economic migrants really exhaust all the possibilities? What about religious migrants, for example?

Despite these reservations, this book is on balance a substantial contribution to the literature. The reorientation it calls for is long overdue and the author's statistical analysis is a convincing demonstration that the distinction between economic and political migrants is a step in the right direction. This book has implications for several subfields of Political Science, including international relations, foreign policy, and the politics of minority groups. If political scientists in these areas will build on Pedraza-Bailey's innovative book, our overall understanding of assimilation should increase markedly.

GEORGE C. KISER

Illinois State University

State and Local Government Administration.

Edited by Jack Rabin and Don Dodd.
(New York and Basel: Marcel Dekker, Inc.,
1985. Pp. xiv + 446. \$55.00, cloth.)

Most edited books are carefully designed and integrated into a presentation that builds a central message from among several independent thoughts. This is usually accomplished through the selection of topics and by an organizational format that leads the reader to the primary theme(s). Commonly, editors assure this by providing introductory summaries that highlight their objectives and how each of the contributed articles adds to the achievement of the goal.

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Whereas Rabin and Dodd offer 21 original essays (only one is cited as having partially appeared previously in print), their opportunity to make a significant case on state and local administration is lost in the absence of clear goals. Nowhere do the authors counsel the reader regarding how each contribution relates to the purpose of the text. The collection takes on a meaning that only the reader can define. With that said of the book's basic shortcoming, a focus on the book's worth is in order.

The initial section offers three articles on the organization and management of state and local administration. Of the three, the lead article's focus on reorganization is a very good summary of state and local efforts at restructuring administration (Garnett). The other contributions on the distinction between chief administrative officials and chief executive officers in city government (Reed) and the emerging role of court administrators (Hays) offer solid discussions.

From this short section, the authors turn to eight articles on budgeting and financial management. State expenditure budgeting practices get attention (Chapman), as does municipal budgeting (Moore), before attention is turned to issues of revenue restraint by Florestano ("State Limitations on Local Fiscal Authority"), Gargan ("Fiscal Dependency and Governmental Capacity in American Cities"), and Rebovich ("Fiscal Stress in the American States"). These five are to the point and paint a good overview of budgetary and fiscal issues. Especially noteworthy is Chapman's grasp of the absence of neutrality in the budget process. The articles on information systems (Jackowski) and productivity (Hatry) offer only minimal additions to this section. Placed in more appropriate contexts however, they might be useful for understanding state and local administration.

Policy making in state and local governments is the third thematic focus. The internal process subtheme of policy making offers articles focusing on the significance of personnel staff selection and roles in the governor's office (Wyner), the growing liability exposure issue of government officials (Hildreth and Miller), the budget process as a control device (Skok), and an overview of the functions performed by legislative staffs in policy making (Balutis). The final two articles bring attention to service

delivery: Hagebak on the integration of human services and Morgan on alternatives in approaches to local service. Although disjointed as a unit, this section does give the reader the best article in the book. James Perry's discussion of the governor's leadership as having a critical impact on organizational change is timely and stimulating, and pinpoints the central nerve in the policy-making process.

The concluding section in the book brings attention to the personnel administration segment of state and local government. Included are a look at accountability and merit in performance measurement (Finkle), training as a vital investment in organizational effectiveness although expediently reduced or eliminated in difficult time (Sumek), and a review of issues of public pension funds and their liabilities in state and local governments (Klinger and Nutter). The last two articles (Finkle and Gross) trace the growth of union activity and labor relations in subnational and grassroots governments.

The gap in state government administration literature is partially filled through these essays that summarize much of the relevant research. Local government which has received more extensive treatments is given worthwhile attention, especially on the issue of state-local interrelationships. The information is summarized well, but offers very little original research or thought.

ROBERT T. PERRY

Ball State University

Environmental Politics and Policy. By Walter A. Rosenbaum. (Washington, D.C.: CQ Press (a division of Congressional Quarterly, Inc.), 1985. Pp. ix + 328. \$10.50, paper.)

Thirteen years ago, serious doubts were expressed in this *Review* about the possibility of writing textbooks on environmental politics and policy. There seemed at that time to be not enough learning, too little "accumulated political wisdom," to allow textbook writers to integrate their findings with those of their predecessors (*APSR*, 66: 593-94). It was in this context that Walter Rosenbaum published and later revised *The Politics of Environmental*

Concern (Praeger Publishers, 1973 and 1977). He is still the only American political scientist who has tried to deal with a broad range of environmental issues in the framework of a single analysis. I think those who teach environmental politics on a regular basis have always admired his willingness to tackle a large and difficult subject in one reasonably short (and reasonably priced) volume. However, I think it is also fair to say that the strength of his writing, and the value of his book for teaching, has stemmed more from its summary analyses of the recent political history of selected environmental issues than from its original and skilled deployment of disciplinary insights.

This latest book will, I believe, evoke the same balance of judgments. It is different enough in form and content from the previous work to be properly marketed by its new publisher as a new book, rather than a revised edition of an old one. The substantive focus is changed, for example, by giving air and water pollution control separate chapters instead of combined treatment. Rosenbaum has also emphasized toxic and hazardous wastes, and, in a chapter on the environmental impacts of energy development, has supplemented a much abbreviated discussion of the environmental impacts of surface coal mining with an excellent section on nuclear power issues. What used to be a chapter on the politics of timber, and became a 1977 essay on the new politics of land use, has now become a review of issues affecting the public lands—particularly in the light of James Watt's tenure at the Department of the Interior.

At the same time, there are elements of continuity between this book and its predecessors. This is most obvious in chapter 2, "The Politics of Environmental Policy," where Rosenbaum tries, as he did in a single chapter of his first book, to summarize what modern political science has to say about the "common constraints shaping governmental response to environmental issues throughout the policy cycle" (p. 33). His earlier willingness to speculate about the impact the environmental movement has had and is likely to exert in the future on policy makers, the political system, and the policies they produce is also displayed, this time in a final chapter called "An Agenda for the 1980s" (chap. 9).

What the reader cannot clearly discern in this mixture of old and new, however, is an

explanation of how and why it is that Rosenbaum thinks his selected topics are coherently tied together. In an effort to find an answer, I did what I think many others familiar with Rosenbaum's work would find it sensible to do. I remembered that his book, like his subject, has a history, and I tried to compare the intellectual premises of this new book with those he relied on twelve years ago.

The first thing driven home by this comparison, and perhaps not fully appreciated before, is the enormous influence exerted by Lynton Caldwell (*Environment: A Challenge for Modern Society*, Natural History Press, 1970) on Rosenbaum's early thinking. And the second is the way Rosenbaum has abandoned this earlier, and very radical, approach to his subject in favor of a wholesale and apparently wholehearted incrementalism. Twelve years ago, Rosenbaum was prepared to argue, with Caldwell and others, that the effective resolution of environmental problems required a fundamental transformation of American political and administrative institutions. Now we find him arguing for modest, even pedestrian, structural reforms of the Environmental Protection Agency, for the infusion of institutional risk assessment into the appraisal of new technologies, and for the broader application of incentive devices, rather than command-and-control regulation, to bring environmental pollution within socially acceptable limits (chap. 9).

What happened to environmental politics and policy in the United States in the last decade and a half to warrant this dramatic (but unacknowledged) shift in the analytical orientation of one of its principal chroniclers? Rosenbaum flirts with the idea that people in government and environmental leaders have begun "to learn from experience and mistakes" (p. 312). He offers a primer of the lessons that a decade and a half of policy making, implementation, and evaluation have taught (pp. 6-17). He also identifies several factors that might have caused this policy learning to occur. These include the ideological reappraisal of policy design provoked by the Reagan presidency (*passim*), the special relationship of business to government (pp. 38-39, 117-23, 286-94), and the changing role of science and scientists in defining environmental problems and policy alternatives (chap. 3). His discussion of this last topic is a major positive feature

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of the book, and one of the best short treatments of the subject in print.

Whether and how policies change for the better as a result of learning are questions quickly coming to the center of policy analysis. Rosenbaum has offered some tantalizing thoughts in this book about the impact learning from experience might be having on environmental policy. But we shall remember his bringing us up to date on the events that have prompted speculation about environmental policy learning—more than his ability to explain how political change can sometimes make policies different and sometimes both different and better.

GEOFFREY WANDESFORDE-SMITH

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Davis*

Secrets of State: The State Department and the Struggle Over U.S. Foreign Policy. By Barry Rubin. (New York: Oxford University Press, 1985, Pp. ix + 335. \$25.00.)

Secrets of State deals primarily with the role of the State Department in the formulation and execution of American foreign policy. Starting with a cursory review of the making of U.S. foreign policy from the republic's early days until 1933, it proceeds to devote almost equal attention to the policy-making processes in the administrations of Roosevelt, Truman, Eisenhower, Kennedy-Johnson, Nixon-Ford, Carter, and Reagan. One of its main themes is the growing weakness of the State Department *vis-a-vis* the National Security Council (NSC) in the policy-making process. A second theme emphasizes the increasing personal and bureaucratic infighting in the government. In this regard, Rubin feels that the Carter and Reagan administrations, for all of their many differences, are similar in terms of their high personnel turnover, lack of direction at the top, clashing personalities, and serious differences among State, Defense, and the NSC. As a result of these continuing problems, he finds American foreign policy to be increasingly controversial, inconsistent, difficult to sell at home and abroad, and ineffective.

Rubin is not totally critical of American foreign policy. High praise is given to a few individuals (e.g., Dean Acheson), and the

period from 1947–1952 is found to be one of creativity and unusual effectiveness. Nevertheless, despite the author's attempts to balance his criticism with merited credit, relatively little is considered worthy of praise.

In a study that finds so much to criticize, it is surprising that almost no proposals are offered for reform. The author cogently argues that most proposals for institutional reform would not be useful because America's difficulties reside far less in its institutional arrangements than in the too-frequent lack of sufficient enlightenment among the top six hundred foreign policy decision makers and in their general inability to work together. But because Rubin also acknowledges that providing an optimum blend of genuinely qualified persons probably cannot be arranged by elaborate reform, he is left with almost no concrete suggestions for improvement. And so despite its scattered praise for a handful of State Department officials, the dominant thrust of the study coupled with its lack of recommendations gives the impression that not only does America have a badly flawed foreign policy apparatus but that it probably cannot be significantly improved by human design.

Many will not fully share this gloomy assessment and for good reason. Nowhere in Rubin's book is the American foreign policy-making process carefully evaluated either against what was feasible in any given situation or against the quality of operation of the foreign ministries of other major states. Indeed, the chief flaw of the book is that it contains no clear, explicit standards by which the performance of the U.S. State Department can be systematically evaluated with reasonable objectivity. For all we know, the U.S. State Department might be doing a fine job compared for example, with the performance of the French or Soviet foreign ministries. Based on the material in *Secrets of State*, the State Department cannot be compared with anything. So, because its criticism is not based on any explicit, realistic criteria of excellence, there is not reason to take it at face value.

Rubin's book has a number of virtues; it is interesting and well written. It also can serve almost as a compendium of the problems attributed to the State Department for the last nearly two centuries. Regrettably, it is no more than this. Is there a need for yet another book devoted principally to criticizing the State

Department? This reader has not been persuaded that such a need exists.

JOSEPH M. SCOLNICK, JR.

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Liberalism and American Constitutional Law.
By Rogers M. Smith. (Cambridge: Harvard University Press, 1985. Pp. 328. \$22.50.)

The dual aim of this ambitious, lucidly written study in constitutional law is to "clarify liberalism's theoretical precepts and difficulties and to identify the notions that are actually forces in current affairs" (p. 6). Smith's analysis begins in general agreement with Louis Hartz' thesis about the "corpse of philosophy" upon which law has flourished in the American liberal tradition. He recognizes many other early sources of American ideals beyond Locke, to be sure, but argues that all of these served primarily to support and to supplement an essentially Lockean liberal body of ideas at the center of our political tradition. Part 1 of the study is devoted to a brief and rather schematic outline of these core ideas, focusing upon the priorities of peace, intellectual progress, economic growth, and personal liberty. But Smith goes beyond Hartz in contending further that this philosophical corpse of "early liberalism" has been a restless one fraught with important difficulties from the beginning. He cites in particular the troubling aspects of consent-based obligation, egoistic individualism, and sufficient moral purpose which have long plagued liberal theory. Moreover, he recognizes three alternative strands of critical thought—communitarian, romantic, and egalitarian—which have emerged to challenge, supplement, and ultimately modify that liberal framework throughout our history.

Part 2 then examines four constitutional policy areas—due process, free speech, apportionment, and economic welfare—to illustrate the ways in which law has developed from liberal origins to accommodate this variety of critical challenges. These chapters provide mostly cursory summations of the central cases and controversies in each area, and hence constitute in some ways the least original part of the book. However, both the author's command of the material and his interpretive

judgements are sound. Moreover, he successfully demonstrates his point that most doctrinal innovations have amounted to little more than "a superficial patchwork of often contradictory solutions that have temporarily cloaked but never resolved the continuing dilemmas of the liberalism expressed in American law" (p. 4). Although more sensitive to quasi-democratic, egalitarian, and utilitarian values, constitutional law in these areas thus has also been rendered more ambiguous, incoherent, and lacking in sustained purpose as well. Smith's discussion of the Burger Court's moderate "democratic relativism" in this regard is especially convincing, if again not particularly novel.

It is in the final part of the book that the argument becomes the most ambitious, original, important, and predictably problematic. Beginning from a critique of the three primary "alternative theories" urged by others to solve the dilemmas of liberalism, Smith advances the argument that "we should reformulate early liberalism's substantive purposes so as to be politically desirable and philosophically credible today" (p. 170). Specifically, he points to the core liberal value of "rational liberty" as a guidepost for redefining and legitimating the many now confused doctrines of modern egalitarian position of Rawls and Dworkin, but differs somewhat in its important distinction between two notions of egalitarian self-worth. Rather than aiming for the goal of equal "self-esteem," which implies excellence in achievement, Smith advocates the promotion of "universal, equal respect," which implies only a minimal standard of personal moral attainment and reflective self-determination. This principle of "rational liberty," he suggests, offers the unique advantage of providing a practical solution to the problems of liberalism without abandoning the liberal tradition itself. The author demonstrates the logic of this claim with regard to both the larger theoretical dilemmas of liberal ethics and the specific areas of modern constitutional law criticized earlier.

These arguments are beautifully developed, but are still likely to be troublesome for many critics and defenders of liberalism alike. For example, the author's admittedly traditional quasi-interpretivist approach to constitutional policy making is vulnerable to criticism from several influential contemporary jurisprudential perspectives. In particular, many scholars

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might rightly question the value of such a traditional analysis of law cast primarily in terms of moral philosophy largely independent of discussion about the long-developing context of unequal social and political power in capitalist America. Moreover, the author's attempted severing of the "rational liberty" principle from its property-based, individualistic, and inegalitarian implications in traditional Lockean liberal theory involves a host of related problems. Above all, it is simply not clear what is left of liberalism or gained for doctrinal clarity, given that the author expects the standards of "rational self direction" to be defined differently by each community. Smith may be right that the new ethic promises a potentially moderate route to legitimating progressive advances in existing doctrines. But it seems more probable that the "rational liberty" principle would continue instead to justify mostly inconsistent yet conventional policies not unlike those of the Burger Court that he criticizes. Indeed, the author admits that his own application of the new principle to contemporary constitutional policies produces few changes, and even those are quite debatable according to varying conceptions of "liberty." All in all, many readers thus are likely to remain unconvinced that liberalism possesses the resources to heal itself, at least without drawing heavily from other contending traditions of political commitment as well.

Despite these and other reservations, however, this is a very challenging and important book. Impressive in scholarship and ambitious in scope, Smith's volume represents a sophisticated attempt to develop from our limited tradition a purposive theory for reorienting contemporary constitutional debates which deserves a broad and serious scholarly reading.

MICHAEL W. MCCANN

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Constitutional Inequality: The Political Fortunes of the Equal Rights Amendment. By Gilbert Y. Steiner. (Washington D.C.: The Brookings Institution, 1985. Pp. 110. (\$22.95 cloth; \$8.95 paper.)

This slim volume seeks to determine the reasons for the failure of the Equal Rights Amendment (ERA) to become a part of the

U.S. Constitution. Steiner reviews the relatively long history of the amendment, first introduced in 1923, and opposition to it, largely from organized labor. The vicissitudes of the amendment in Congress, including the well-known hostility of Chairman Emmanuel Celler, Chair of the House Judiciary Committee, and the key roles played by two Congresswomen, Edith Green and Marth Griffiths, are highlighted.

The major portion of the book, however, examines why the amendment finally faltered, although its easy passage in the Congress in 1972 appeared to assure early success. Steiner points to several problems: the difficulties inherent in the amendment process for issues dealing with substantive change, the relatively lukewarm support of successive presidents, and the possibility that legislative or judicial policy making might provide viable alternatives to the ERA.

Ultimately, Steiner concludes that the course of ERA's ratification was complicated by a newly emerged opposition to abortion (in the wake of *Roe v. Wade* in 1973). Additional issues raised by the Vietnam War and the subsequent Soviet invasion of Afghanistan related to the ERA's impact on American women. Steiner also questions the decision to extend the ratification period limit for an additional three years in 1979, as of doubtful legality, and addresses a similar theme with regard to suspect procedural strategies in Congress which unsuccessfully attempted to renew the ERA after its failure in 1982. Nonetheless, he concludes that the three-year extension, at least, was irrelevant to the final outcome.

Indeed, the nature of the opposition that emerged, remains the most interesting puzzle of the ERA experience, particularly in light of the fact that within a year of congressional passage of the amendment, 30 states had ratified, often by large majorities. By 1975, 34 of the 35 states that were going to ratify the amendment had already done so. Only one state ratified thereafter, and it proved impossible to gain support in even one of the three more states needed to pass the amendment during the 39 month extension period. While disagreements over priorities and lack of unity between the National Organization for Women (NOW) and other groups existed, the reason for ERA's demise clearly lies in the emergence of a determined opposition, which

managed to escalate the level of conflict and transform the issue from one of "equity" to one involving a transformation in lifestyles and values. The rise of the New Right and the ascendancy of conservatism in the Reagan era—utilizing sophisticated interest group and campaign tactics involving PACs, direct mail, and the media—changed the nature of the debate over the ERA and made it politically costly for legislators to support the amendment. It is not simply enough for Steiner to mention Phyllis Schlafly and antiabortion politics. Surely an analysis of the failure of the ERA ought to examine the sources, significance and future of organized opposition to feminist politics as manifested in the ERA ratification conflict. Regrettably, this book sheds little light on such issues; in order to deal with them we need to refer to works such as Janet Boles' on the ERA and Kristin Luker's on abortion attitudes and politics.

As it stands, Steiner's book remains an interesting factual account but one which does not illuminate the political dynamics that animated this controversy. Particularly in the absence of reference to any primary sources, the author's final conclusion that the ERA struggle might be forgotten if only the Supreme Court would declare sex a "suspect classification" seems unrealistic both in terms of current judicial politics and the politics of the womens' movement.

JOYCE GELB

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Public Policymaking in the American States.

By Jack Treadway. (New York: Praeger Publishers, 1985. Pp. 193. \$35.95.)

Richard Dawson and James Robinson's seminal article on public policy in the American states was the first systematic comparative state policy analysis by political scientists. It also framed the question that would dominate twenty years of research—the relative importance of political and socioeconomic variables in the development of public policy in the American states. Subsequent studies took diverse approaches to this central concern, developing a variety of theoretical conceptualizations and applying diverse analytic techniques.

In this volume, Treadway attempts to organize and sort out developments in this field. He provides a reasonably thorough, comprehensive review of this body of work as it has developed in the writings of political scientists. *Public Policymaking in the American States* is a literature review that is organized to discuss four major relationships: (1) the influence of the socioeconomic environment on the political system; (2) the influence of the socioeconomic environment on policy outputs; (3) the influence of the political system on policy outputs; and (4) the relationship between politics and the environment in the determination of public policy outputs. The book is very similar in purpose and format to Brett Hawkins' 1971 treatment of *Politics and Urban Policies* (Indianapolis: Bobbs-Merrill). It contains no original empirical research.

Treadway draws upon more than 120 studies that appeared from 1963 to 1984. While he does not discuss the surge and decline of productivity in the comparative study of public policy in the American states, his review contains the data for such an analysis. The literature started slowly; single articles were published in 1963, 1964, and 1965. Then it began to take off—five pieces were published in 1966, ten in 1969. Such analyses peaked in 1973, when 15 items were published. Sixty-four percent of the published items covered by Treadway appeared in the years from 1968 to 1976. Since 1976, publishing activity has been light, averaging less than four items per year. In addition, relatively few of the articles in recent years have appeared in leading journals of political science. The *APSR* published none from 1980 to 1984. This pattern reflects the absence of major empirical findings or theoretical developments in recent years.

Treadway provides a careful account of the literature, presenting the major findings and discussing disputes over the appropriate conceptualization and specification of the systems model that guides so much of the research. He discusses a number of studies that diverge from the dominant framework, and he identifies contradictory empirical findings among the various studies.

Treadway is most impressed with studies emphasizing the force of incrementalism and national policy in state policy processes. He also emphasizes (1) studies which suggest that the determinants of policy depend upon the

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policy or policies analyzed and (2) studies that indicate that the determinants of policy vary over time or that they vary depending upon whether one examines policy across systems or over time. He believes that available findings provide the basis for a contingency model of state policy systems. He concludes that "the exact mixture of politics and the environment . . . (that determines policy) depends on what policy area is being examined and how the environment, the political system, and policy outputs are being measured" (p. 179).

The strength of this volume is its fairly comprehensive coverage of the political science literature on comparative state policy analysis and its attempt to organize and make sense of the diverse findings in that literature. Treadway has organized the material well. He provides concise descriptions of the various studies and their findings. He writes clearly. The book makes accessible a large and scattered body of literature.

The book is limited in what it provides. It offers little in the way of new theory. Its criticisms of various studies are the standard criticisms that appear in the literature. It offers no new empirical evidence. In addition, Treadway it is not particularly sensitive to the validity or reliability of individual studies. While noting some criticisms and shortcomings, he reports the findings of various studies as if they were of equal value. Part of the problem is that he does not establish a set of standards by which to assess contributions to the literature. Thus, it is hard to tell how much weight to attach to various studies in formulating a set of hypotheses or a model for further exploration.

Despite his relatively comprehensive coverage of the literature, Treadway misses a number of important contributions. Like most political scientists working in this area, he virtually ignores the work of economists, except for the early studies by Sacks, Harris, Bahl, and Fischer. Nor does he draw upon the contributions of sociologists such as the fine study by Kristen Gronbjerg, *Mass Society and the Extension of Welfare: 1960-1970* (Chicago: University of Chicago Press 1977).

EDWARD T. JENNINGS, JR.

University of Missouri
Columbia

The Breakdown of Democratic Party Organization, 1940-1980. By Alan Ware. (New York: Oxford University Press, 1985. Pp. xii + 275. \$29.95)

Consciously in the tradition of James Q. Wilson's *The Amateur Democrat* (Chicago: University of Chicago Press, 1962), this book is a case study of Democratic parties in three cities. The cases are well-chosen, ranging from a traditional "machine" city (New York) to an urban area with "non-partisan" traditions (East Bay, California, where Oakland and Berkeley are located) and in between, a city with a history of moderately strong parties (Denver). Employing archival research, interviews and surveys of party leaders and activists, the author dates and describes the disintegration of the local Democratic parties. Well written and researched, this book marks an important contribution to the literature on party organizations.

The "decline of party" thesis is perhaps the most widely agreed to proposition in American political science, but as Ware points out, there is little agreement on when and how party decline occurred. Ware finds the process to have been varied, uneven, but thorough at the local level. Of the three most widely-cited causes of party decline—socioeconomic change, modern campaign technologies, and party reform—he finds the first to be the most important and the third, the least. Indeed, decline was precipitous and catastrophic only when all three factors operated at once, in the early 1960s. Ware likens the process to a person with heart and bronchial ailments who is involved in a traffic accident: urban decline damaged the heart of the Democratic parties, allowing modern campaign technology to cut short their breath, and the "accident" of institutional reform completed their demise.

In Ware's view, the necessary condition for party decline was the inability of local party leaders to recruit activists, the "heart" of party strength at mid-century. Changes in urban society and government reduced dramatically the numbers of "professional" activists available to the parties, though initially the same trends produced "amateurs" were gone as well, so that local parties could not muster enough manpower for effective campaigns. Though the dearth of activists had many causes, Ware suggests that the absence of

solidary incentives was critical. The decline of "regular" and "reform" clubs occurred simultaneously, as potential activists found alternative social outlets.

The same forces that sapped party manpower also weakened the allies of local Democratic parties—labor and black organizations, which might have helped replenish the party labor pool. Instead, these organizations withdrew from local campaign activity, just as a number of conflicts endemic to party politics appeared with great intensity; factionalism, issue extremism, and reform efforts. Such "accidents" further depleted the pool of activists. When calm was restored, the party organizations had been largely replaced by those who could campaign effectively, namely individual candidates. While the reforms did little to restore party strength, Ware sees them as inevitable given the collapse of local parties.

If the inability to recruit activists was a necessary condition for party decline, modern campaign technology was a sufficient one. Strong local parties might have slowed the use of television and campaign consultants, but the growing impotence of local parties hastened their employment. And the increasing use of modern technology exhausted the parties further. As with party reform, Ware claims that new campaign techniques were inevitable once local decline was far advanced. Candidates now have many means of campaigning, which they employ according to their particular circumstances, making some elections more competitive (usually state-wide and national contests), but other much less so (mostly local races). Thus, highly atomized, "party-less" politics has appeared in formerly Democratic strongholds.

What needs to be explained then, is the persistence of party politics in the absence of viable organizations. One reason is the development of alternatives to local parties. Ware reports a number of examples among Democratic candidates: organizations based on the perquisites of incumbent legislators, single-issue concerns, and popular officials (who act as "warlords" locally). Another reason is competition from the Republicans, who have developed strong national party organizations. Adept at the use of modern campaign technology and dedicated to grass roots party-building, the Republican organizations may represent a new support for party politics.

Given the quality of this study, one hopes Ware's next book is "The Development of Republican Party Organization, 1940-1980."

JOHN C. GREEN

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Administrative Rulemaking: Politics and Processes. By William F. West. (Westport, CT: Greenwood Press, 1985. Pp. 217. \$35.00, cloth.)

This book depicts the temper of the political times. As American society reflects the dilemmas of government regulation vs. deregulation and as the American Political Science Association focuses upon organization of power as the theme of the 1986 annual meeting, both politicians and political scientists express more interest in formal rules, structures, and procedures.

West has authored a book which, in brief fashion, gives succinct and appropriate detail to the expansion of the delegated authority of American public institutions. The author analyzes administrative rulemaking for implementing statutes and evaluates requirements utilized to guide and delimit rulemaking decisions.

He critiques the role of structural mechanisms in the policy-making process and emphasizes political determinants and effects of procedural choice. This text is both timely and valuable for the study of political, bureaucracy in its examination of pervasive rulemaking throughout the federal bureaucracy, of tension resulting from the growth of administrative policy making, and of broad delegations of discretionary authority sanctioned by the courts.

The author maintains that the constitutionality of delegated authority has never been resolved by the courts. He summarizes that acceptance of delegated legislative authority in almost any form by the courts is a response to policy needs instead of legal considerations. West concludes that rulemaking is widely accepted as a means of implementing authority.

He describes pressures to reform the administrative process, delineates rulemaking requirements of the Administrative Procedure Act (APA), and spells out exemptions from the APA. In explaining formal and informal

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rulemaking procedures in the APA, West elaborates on structuring rulemaking discretion.

The author analyzes legislative policy making and evaluates institutional rulemaking with a case study of the Magnuson-Moss Act of 1974 and its impacts upon the Federal Trade Commission (FTC). West points out that the Magnuson-Moss legislation encouraged reliance on rulemaking and that such rulemaking has profound effects on the FTC's relationship with its political environment. Noting that rulemaking was adopted by the FTC in early 1960s and emphasized in the 1970s in order to police unfairness and deception and to enhance consumer protection, West concludes that the FTC's experience confirms the potential superiority of rulemaking as an effective implementation procedure. According to the author, rulemaking emphasizes force the FTC to confront complex and controversial issues.

In writing this definitive volume on politics, processes, and administrative policy making, West emphasizes that two basic perspectives—the causes and effects of formal structures and the competing interests of group politics—should guide political scientists' conceptual analyses of administrative institutions.

West sees the rulemaking process as involving the tension between rationality, embodied in formal structures, and responsiveness to group demands. Within this conceptualization West offers a valuable contribution to the literature of political bureaucracy by analyzing the dramatic expansion of delegated legislative authority of recent years and the proliferation of requirements structuring the exercise of bureaucratic discretion.

JOHN E. ROUSE, JR.

Ball State University

The Politics and Development of the Federal Income Tax. By John F. Witte. (Madison: The University of Wisconsin Press, 1985. Pp. xxii + 437. \$25.00.)

Political scientists have generally paid much less attention to tax policy than to the spending side of the budget. This imbalance has been significantly narrowed by John Witte's masterful study of the federal income tax. The virtues of the book are stylistic, methodological, and

substantive. In the course of learning a great deal about taxes, readers also get a model lesson in how to gather and use data. Witte is a lucid, careful writer whose findings are consistently grounded in the evidence at hand.

Witte is guided by the principle that the method of inquiry must be appropriate for the issue being studied. The book, accordingly, offers a medley of approaches that are not typically combined in the same study. Historical research (which accounts for about half of the book) is employed to show how tax legislation has evolved over the past century; survey data are used to compare public opinion and the incidence of tax burdens and benefits; roll call analysis is brought into play to explain recent tax activity in Congress; statistical measures are devised to analyze the type and distribution of tax expenditures; and economic and normative analyses provide a basis for assessing competing theories of taxation. Witte is adept in using—and explaining—each of these approaches. This reader came away feeling that the right approach was selected for each area of inquiry.

Witte retrieves historical research from the discard pile of political science to explain why the tax code is so complex and fails to redistribute income from wealthier to lower-income classes. The story is told in a lively manner, as each period's increment of legislation brings tax policy closer to its current predicament. Witte convincingly argues that the nominally progressive tax structure was more an accident of wartime needs to raise revenues than an instrument of economic redistribution. Progressive rates meant higher middle-class tax burdens and fueled pressure for relief. In peacetime, tax legislation has typically been tax reduction, sometimes through adjustments in the rates, but often through the extension of tax expenditures. All sectors and classes have benefitted from this behavior, though not always to the same degree.

Increased complexity, it turns out, has often been a byproduct of efforts to make the tax code more equitable rather than of blatant catering to narrow interests. Congress has had an active role in shaping tax policy; it has frequently disregarded or altered presidential recommendations concerning the amount and mix of taxes. Sometimes, Congress has favored capital formation; other times, it has sought to

ease the burden on low-income taxpayers. The lack of policy consistently is mirrored by a lack of theoretical cohesion. Witte dissects several leading theories of taxation, demonstrating that none is a fully-adequate guide to policy.

The book takes a close look at tax expenditures, and Witte's findings have a bearing on recent efforts to reform the tax system. Tax expenditures have grown because tax burdens have increased. Over the years, as Congress has raised tax rates, it has faced pressure from middle-income payers pushed into higher brackets, as well as from various interests seeking financial subsidies. Many big preferences date back to the formative years of income taxation, though some recent ones have been narrower than those of long standing. Tax expenditures are hard to take away because they have characteristics similar to open-ended entitlements: the benefits are conferred on a broad swath of the population and their value grows automatically without new legislative action. Wealthy persons receive more tax breaks than do those who make less money, but only because they pay more taxes.

Despite widespread general disenchantment with the tax system, public opinion data do not stray widely from actual tax policy. Although Witte is careful not to push his findings too far, he suggests that effective tax rates are pretty close to what taxpayers think they should be.

The evolution of the tax system validates the incrementalist claim that major policy developments can cumulate from numerous small adjustments. "Never has there been," Witte exults, "more convincing evidence for Braybrooke's and Lindblom's contention that incrementalism can lead to massive change..." (p. 129). In this reviewer's judgment, descriptive incrementalism is not an issue worth fighting over, nor is it a matter that can be settled empirically. Just about all social behavior is an incremental extension of the past and present into the future. Taxation is

not significantly more incremental than other, less measurable, public policies. Because the size of increments is indeterminate, anything but the most revolutionary change can be said to conform to this model. Witte regards the truly massive tax increases imposed in World War II as incremental; so, too, have been Congress' numerous tinkering with the tax laws in the postwar era.

It is as a norm for assessing policy processes or outcomes that incrementalism is an important issue. Witte's conclusions, coming in the final chapter, after a book full with "the system works" comments, are surprisingly adverse. He suggests that incrementalism "may produce unpleasant policy outcomes when extended over a long time frame" (p. 367); "the income tax as a fundamental and ostensibly equitable means of raising revenues has been slowly but continuously eroded" (p. 369); and that "the politics of income taxation follow a self-destructive path." (p. 377). Witte's solution is to insulate tax policy from democratic politics by giving the executive greater influence, strengthening internal congressional controls, such as closed rules and markups, and having more stable policy that would not be altered to satisfy immediate pressures on Congress. "Lasting tax reform," Witte argues in the book's final words, "will not come without lasting tax reform" (p. 386).

This may be a valid conclusion but it does not flow directly from the study. Having examined the development of tax policy with painstaking care, Witte failed to consider whether different political arrangements over the past century significantly affected the outcome. The linkage of institution and policy is always difficult to forge; it is a measure of John Witte's accomplishment that this work can be put to a test that can be applied to few recent studies in public policy.

ALLEN SCHICK

University of Maryland

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COMPARATIVE POLITICS

The Nordic Parliaments: A Comparative Analysis. By David Arter. (New York: St. Martin's Press, 1984. Pp. x + 421. \$30.00.)

In this welcome volume, David Arter undertakes to describe, analyze, and compare the roles of the Nordic parliaments in the policy process. In seeking to accomplish these purposes, he examines in considerable detail not only the Danish *Folketing*, the Icelandic *Althing*, the Norwegian *Storting*, the Swedish *Riksdag*, and the Finnish *Eduskunta* but also the "home rule" legislatures (the *Landsting* of Greenland, the *Lagting* of the Faeroe Islands, the *Landsting* of the Aoland Islands) and the Nordic Council, the regional consultative assembly. This is an ambitious project, especially since along with intra-Scandinavian comparisons the author touches upon relevant features of the United States Congress and the parliaments of Britain, France, and West Germany.

The result is a compendium of information, most useful in providing in one book an almost encyclopedic description of legislative structures and procedures in Northern Europe. After an introductory survey of the institutions, their members, and, in an abbreviated discussion, the party systems, Arter considers the parliaments under three headings: policy making (the influence of commissions of inquiry, the role of civil servants, the impact of cabinets and prime ministers), policy adoption (plenary sessions, agendas, presiding officers, standing and "upper tier" committees, the adoption of measures), and policy implementation (parliamentary questions, motions of no confidence, legislative oversight through investigating and review committees, advisory committees on foreign relations, ombudsmen). Frequent examples illuminate these topics, though some may not be entirely enlightening to those who lack familiarity with Scandinavian politics. Arter is not unconcerned with broader questions—the "decline of parliaments" thesis, for example—and reaches some general conclusions from the Nordic experiences. He finds a lack of direct involvement by

parliamentarians in policy formulation, except in Sweden. Because most measures adopted are government proposals, the Nordic parliaments, with the possible exception of Iceland, clearly belong to the family of deliberating, rather than legislating, assemblies. Their influence lies more in the scrutiny and control of the executive, but their impact on policy implementation is weak when compared to that of Congress or even the House of Commons. Political changes in the 1970s and 1980s—realignments in party systems, the regular incidence of minority cabinets (especially nonsocialist ones), and challenges to the consensus by organized groups raising new issues—have created conditions for a modest revival of Nordic parliamentary power.

Valuable as the book is, it suffers from a number of defects. In his attempt to include almost everything, Arter pays a price in style and clarity; the reader sometimes feels that he is plodding through a dense forest. The treatment of the political and party contexts within which the parliaments operate is brief and cursory. As is almost inevitable in such an extensive survey, some important studies are overlooked; *Folketingsmedlemmer på Arbejde* by Erik Damgaard and colleagues, for instance. There is no reference to the many useful publications of the various parliaments, such as Denmark's *Folketingets Handbog* and the annual *Folketingsarbejde*. Particular to be regretted are the inadequacy of the index and the complete absence of a bibliography. In the skimpy index, nearly three-fourths of the entries are names of individuals; there are no entries for such basic topics as "committees" or "electoral systems," despite their extensive coverage in the book. If one wants to make a quick check on aspects of the Norwegian parliament, there is no entry for "Norway;" one would have to track down a number of separate references to the *Lagting*, the *Oldesting*, the *Storting*, assistant ministers, and so on. This holds true for the other parliaments as well. The lack of bibliography is frustrating and a hindrance to anyone wanting to look

beyond this study or to pursue a topic in greater depth. Wading through the footnotes is hardly an adequate substitute.

Despite these criticisms—meeting some of them would have required Arter to write a longer or a different book—*Nordic Parliaments* is a treasury of information on the legislatures of Northern Europe. As such, it is a valued addition to the small but growing body of literature in English in the region's political systems.

KENNETH E. MILLER

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Politics and Public Policy in Kenya and Tanzania. Edited by Joel D. Barkan. (New York: Praeger Publishers, 1984. rev. ed. pp. xviii + 375. \$34.95, cloth; \$13.95, paper.)

This book, an update of a work published in 1978, consists of a set of comparative studies of political institutions and public policies in Kenya and Tanzania. Development, political parties, legislators, bureaucracies, the link between ideology and objective conditions, class formation, rural self-help, rural development policy, urban policy, education policy, and foreign relations are compared. It remains the most comprehensive comparative study of these countries available. Yet, the objectives of the book are more than the presentation of a collection of insightful studies.

In the preface to the first edition, reprinted in the volume under review, the editor suggested that the book "is intended to be a discussion in which the participants attempt to speak, and argue with themselves and with each other, within the rubric of a single set of questions..." (p. viii). The four questions he set out were (1) How successful has each country been in achieving its developmental goals? (2) Which policies have been most effective? (3) What internal and external factors have affected the efforts of policy makers? and (4) How useful are Kenyan and Tanzanian experiences likely to be as a guide for other Third World societies?

Although several authors consider some of these questions, they do not determine either

the organization or the content of individual studies. As a result, the book does not have the kind of coherence the editor sought. More significantly, there is virtually no interaction or discussion among contributors. The reader is left with a wide variety of conflicting interpretations and questions. For example: who is more correct in his description of class structure in the two countries, Holmquist or Leonard? Is Stren's attempt to interpret urban policy without serious consideration of class conflict appropriate? Is policy such as self-help just a "sideshow" only "significant for what it reveals about the overall structure of power..." (p. 193), as Holmquist suggests? Should the party in Tanzania be considered an entity separate from the bureaucracy as Okumu implies, or grouped with it as Leonard suggests? If class struggle within each country is as important to policy decisions as several writers suggest, should not Gordon's review of foreign policy make more of a connection between the two? Such questions indicate continuing disagreement among scholars over both the best units of interpretation and the proper specification of those units in the East African context—fundamental concerns whose resolution is of great importance for the advance of social science research. Yet, the absence of direct interaction among the authors has left many questions unanswered. The goal of the editor in achieving a high degree of coherence and interaction has not been fully realized—a criticism as true of the first edition as of this revised one.

Indeed, the revised edition differs only marginally from the first. Leonard's article on class formation was added, while Okumu's on foreign relations was dropped and replaced by Gordon's work on the same subject. Okumu was dropped as co-editor, and Holmquist revised his article on political parties. The extent of revisions within existing articles varies, with no fundamental changes in the arguments presented. None of the authors changed his principal analytical variables or findings, suggesting that interaction among authors outside the pages of this book has produced no convincing arguments or data on matters of disagreement. The updated material does lend credence to observations of trends previously made, such as the increasing difficulties Tanzania has faced in its attempts to break radically from its colonial heritage.

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Despite the problems mentioned above, Barkan's edited work brings together the insightful views of 10 fine scholars who have spent many years studying Kenya and Tanzania. The footnoted material at the end of each chapter and Holmquist's bibliography at the end of the book present the reader with an excellent set of sources for further reading. Also, the differing views of the authors about politics and policy in Kenya and Tanzania challenge us to seek a resolution of the contradictions.

DEAN E. MCHENRY, JR.

Claremont Graduate School

The Making of Foreign Policy in China: Structure and Process. By A. Doak Barnett. (Boulder: Westview Press with the Foreign Policy Institute of the School of Advanced International Studies, The Johns Hopkins University, 1985. Pp. xiii + 160. \$18.50, cloth; \$10.95, paper.)

Analysts of Chinese foreign policy have traditionally relied on communist ideology, official media statements, and the views of selective leading cadres (e.g., Mao Zedong, Zhou Enlai, Deng Xiaoping) to explain the twists and turns in Peking's conduct of external relations. Because of the relative inaccessibility of Peking's policy processes, they have not, until recently, generally focused on the institutional context in which Chinese foreign policy is formulated and decided. *The Making of Foreign Policy in China* goes a long way to fill this lacuna. Combining his long-standing expertise in Chinese politics and his extensive contact with members of the Chinese foreign policy community, A. Doak Barnett offers us an extremely useful picture of Peking's foreign policy structure and process. He sheds light on important questions such as where in the party and governmental bureaucracies major foreign policy issues are dealt with, who the key decision makers are, and how different parts of the Chinese foreign policy machinery are related. This "mapping" has been informed by interviews with a variety of Chinese officials who are directly and actively involved in the formulation and implementation of foreign policy. They included Premier Zhao Ziyang, vice-ministers and desk heads in the Ministry of

Foreign Affairs and the Ministry of Foreign Economic Relations and Trade, and leading members of the diplomatic corps, research institutes, press organs, and major cultural exchange organizations.

The recent changes in the Chinese foreign policy apparatus, as reported by Barnett, have been in the direction of greater institutionalization and professionalization. The older leaders have generally assumed the "second line," with "first line" responsibilities being taken over by a younger generation headed by Premier Zhao Ziyang and Party General-Secretary Hu Yaobang. Many of the new leaders have specialized knowledge and administrative experience in their respective fields. Concomitantly, the Politburo and its Standing Committee have withdrawn from dealing with day-to-day policy making. Instead, the Party Secretariat and the "inner cabinet" of the State Council, working in cooperation, have become the key centers for policy making. Only the most important and perhaps controversial issues are referred to the highest echelons of the party.

Barnett's study shows that despite recent reforms, the Chinese foreign policy apparatus still appears to be relatively weak in several areas. Institutionalized coordination among the diplomatic-political, economic, and military organizations involved in foreign affairs—especially at the lower working levels—seems to be insufficient. In sharp contrast to the U.S., the defense establishment is poorly represented in the key bodies for making and formulating foreign policies. Also, despite recent trends toward greater specialization and professionalization, Peking still faces a shortage of expert personnel. This constraint affects the quality and quantity of information the Chinese leaders can draw on. Although policy-related research is being emphasized, there continues to be heavy reliance on "raw" or "unevaluated" data. *Reference Materials*, a compilation of news articles in foreign newspapers, appears to constitute the main source of current information about international developments for many Chinese officials. There is nothing comparable to the large U.S. intelligence community in China. The Ministry of State Security is relatively small, and seems to play a more indefinite role in the councils dealing with foreign affairs.

On the other hand, various mass organizations (e.g., the Chinese People's Association

for Friendship with Foreign Countries) and the news agencies (e.g., the New China News Agency) play a much more active and authoritative role in the foreign policy process than their American counterparts. People-to-people diplomacy has been a traditional part of Peking's foreign policy, and Peking's press representatives have sometimes served as de facto diplomats in areas where China has no formal ties. In addition, with the adoption of its "open door" policy, foreign economic issues have gained greater importance in Peking. The recently established (in 1982) Ministry of Foreign Economic Relations and Trade has consolidated in one organization functions (e.g., foreign trade, investment, aid) that are dispersed among many agencies in the U.S.

In summary, Barnett's study makes a very valuable contribution to our knowledge about the institutional context of Chinese foreign policy. It clarifies many questions that heretofore have been assigned to the policy-making "black box" in Peking. It should be read by all students of Chinese foreign policy, and by those interested in comparative foreign policy as well.

STEVE CHAN

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Language Policy and National Unity. Edited by William R. Beer and James E. Jacob. (Totowa, NJ: Rowman and Allanheld, 1985. Pp. x + 244. \$34.50.)

This volume of essays contributes to the slowly growing body of literature called *the politics of language*, *politico-linguistics* or even *glottopolitics*. The editors have included essays from four continents. Mature and younger scholars write about the United States, Canada, Belgium, Finland, Spain, France, Algeria, Malaysia, China, India, and Bangladesh in order to provide empirical data on the rise of language issues and policy responses. The editors have written a careful introduction to make sense out of the data and a conclusion in order to build theory in this developing area of comparative politics.

Beer and Jacob's preference for unity over disunity is strong and practically unquestioned by the authors. They wish to understand how

government can preserve unity in multilingual societies through policy when language becomes an issue. All the essayists agree with this assumption that big is better except for Jyotirindra Das Gupta, a pioneer in scholarship on the politics of language. Das Gupta challenges the notion of unity as a good per se by examining the case of Bangladesh and Assam. The unity of Pakistan meant domination, he believes, while independence for Bangladesh meant a step forward in human development. At odds with the assumptions of many political scientists, but compatible with the assumptions of many sociologists, he concludes, "There is nothing intrinsically destructive about language politics, including language demands which affirm autonomy" (pp. 213-14).

Equally provocative for other reasons is the chapter on the United States by Edward Sagarin and Robert J. Kelly, who insist that multilingualism is and will remain an important feature of the American polity. They do not believe the ethnic revival will ever end. They support the notion that America has no official language, which is a bit like saying that Great Britain has no constitution. It seems clear to me that precedent and practice make English the official language of these 50 states.

Milton Esman, veteran scholar in the analysis of ethnic and national movements, writes clearly about bilingual policies of the Canadian federal government. He believes provincial unilingualism is an inexorable trend, but recent court cases have established bilingualism in Quebec and Manitoba, or at least more of it than provincial authorities wished. The wealth of detail, the analysis, and the conclusion are useful for others interested in Canada. Although bilingual foreign policies at the federal level "have not insured the unity of Canada, that unity would have been at much greater risk without official bilingualism" (p. 54).

Matthias Hartig's short summary of issues in Belgium covers the main points well. Erik Allardt, well known in the U.S. for his studies of ethnicity, analyzes the place of Swedish in Finland and the policies designed to protect this minority. The focus of policy is the commune, where services must be provided in the language of the residents. A commune must provide services in both Swedish and Finnish if the minority equals at least 8% of the total, or 3,000 persons (p. 93). Attitudes of both groups

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support mutual respect, a more important contribution to unity than is policy, it seems.

James Rial's analysis of Spain naturally focuses on the Basques and the Catalans. James E. Jacob and David C. Gordon, who know the history of French language policy very well, provide many historical details about language policy, and conclude that the government is now willing to allow a greater place in schools for regional languages, because they have declined in importance and are no longer perceived as a threat to national unity. Gordon's separate essay on Algeria and Lebanon before 1975 points to the continuing importance of French outside Europe and the dilemma of Algeria in asserting its authentic Arab-Islamic culture while building a modern secular state that uses French.

Diane K. Mauzy explains how the Malay, fearful of being overwhelmed by the Chinese and Indians, use language policy to stamp Malaysia with their identity and to insure jobs for themselves. She concludes that the Malay language is now safe, but that ethnic divisions are a threat to peace and unity.

Pao-chien Tseng offers a discussion of linguistic complexity in China. Although Chinese speakers are 93.3% of the population and share a common written language, they speak 10 different dialects that are not mutually comprehensible. Since 1911 all Chinese governments have promoted the Beijing variety as the Common Spoken Language, or *Putong Hua*, and in 1960 the policy makers decided that beginning in the third grade students would use *Putong Hua*.

In his conclusion William Beer, building on his own previous work on ethnicity in France, contributes to a theory of linguistic mobilization. Why do language differences become a focus for group organizing and demands? Decline of the nation-state, "rapid development of previously backward regions" (p. 223), and a feel of "dislocation" and relative deprivation among educated elements are the principal factors, he writes. One might have wished for a conclusion about policy, but Beer's ideas are welcome. And so are the bibliographies at the end of each chapter.

The editors make clear that they wish their book to suggest studies by others. In what areas can political scientists appropriately work to enlarge an appreciation of the politics of language?—language planning as an exam-

ple of the policy process and public administration; interest group behavior; political parties, which sometimes have been organized expressly to promote language demands; elite behavior, particularly literary elites; the effects of culture; and many other subjects. Some political scientists have already made important contributions in these areas, but more are needed. The Beer and Jacob volume is required reading for those who enter this fascinating area of scholarship.

BRIAN WEINSTEIN

Howard University

The Indian Foreign Policy Bureaucracy. By Jeffrey Benner. (Boulder: Westview Press, 1985. replica ed. Pp. xiii + 305. \$19.50, paper.)

India Under Pressure: Prospects for Political Stability. By Robert L. Hardgrave, Jr. (Boulder: Westview Press, 1984. replica ed. Pp. xiii + 214. \$18.50, paper.)

In the last 10 years the study of Indian politics has shifted increasingly and usefully toward an analysis of India's political economy. Francine Frankel's *India's Political Economy*, Ron Herring's *Land to the Tiller*, Pranab Bardhan's, *The Political Economy of Development in India*, and the Desai, Rudolph, and Rudra volume, *Agrarian Power and Agricultural Productivity in South Asia* offer a sampling of the change in subject and approach. The two volumes under review here take us back to an earlier era when the focus was on the more strictly institutional: the organization of bureaucracy, the capacity for coalition building, party strategy, etc.

This more institutional approach has its strengths. It can provide a full description of political actors whose identities are too easily lost in more abstract treatments of economy, society, and "the state." However, the approach can also risk slippage into an excessively descriptive genre of analysis that treats governmental bodies as entities whose importance is not fully demonstrated.

Jeffrey Benner's description of the Indian foreign policy bureaucracy demonstrates both the advantages and problems of a largely institutional approach. As a predoctoral publication, it shows remarkable skill. Benner has

the stamina and capability to ferret out through extensive interview, documentary, and secondary analysis a clear and comprehensive portrayal of the organizational origins of Indian foreign policy. For scholars or diplomats who want a good, sound, but not pedantic description of both pre- and post-Independence foreign policy bureaucracies, this book is an important tool. Benner's description of the British-Indian bureaucracy—its structure, its responsibilities, the shifting locus of decision making—is brief but well-researched. The story is always engaging. Drawing on data collected elsewhere, Brenner reports, for instance, that the slow rate of Indianization meant that of 36 initial recruits to the future Indian Foreign Service at Independence, only 4 had international experience. His well-chosen comparisons with foreign policy establishments elsewhere are also revealing: in 1981, 2.5% of the American Foreign Service came from minority backgrounds. By contrast, 11.2% of the highest cadre of the Indian Foreign Service, Class I, came from Scheduled Caste and Scheduled Tribe backgrounds. Benner's description of the service's procedural hierarchy, reform efforts, research and intelligence, and his occasional depiction of particular foreign ministers and secretaries provide an excellent close account of a bureaucracy about which little has been written.

The main weakness of this book is acknowledged explicitly by Benner in the preface. Although it began as an account of how bureaucracy shapes policy (and Benner promises a subsequent study that will deliver on the promise), the present study does not explore this causal linkage. It sets, instead, a good descriptive foundation for a subsequent study of the role bureaucracy may play in steering policy formation.

Hardgrave's analysis of past trends and prospects for political stability in India provides the best of the genre of well-elaborated "political risk analysis." The study, supported under contract by the U.S. Department of State, is balanced, informed, careful, and supremely economical in its presentation of the multiplicity of forces that bear on the continued stability of India's democratic institutions. Hardgrave looks at economic pressures, regionalism, communalism, ministerial instability, and foreign policy. Every important section of the political landscape is sketched.

Although each political problem (the Punjab, for example) receives no more than a few pages of discussion, Hardgrave zeroes in on the critical, defining issues. For example, he argues that the Punjab is not a problem of religions in conflict, but is rather, primarily, a political matter whose outcome will depend heavily on the future political role of the regional party, the Akali Dal.

For anyone seeking a quick, expert guide to the history and future of particular political issues and events, this book is a fine reference tool. Complicated changes in class stratification are summarized, the unfailingly complex issues in Assam are outlined, and key developments in state politics are enumerated. The book's greatest strength lies in its superb "briefing" of the reader. It is not, however, the useful research tool it might have been, due to the absence of a bibliography and the existence of only the most limited footnotes (due no doubt to the required format of a State Department report). The reader who may want to follow up on Hardgrave's consistently interesting argumentation about particular issues will feel frustrated by the lack of guidance to references. Because, too, of its "briefing" format, the reader does not come away with a clear theoretical perspective on the determinants of political stability. The pieces are there but not the theoretical specification of how they fit into a whole.

Despite some of these limitations, both Hardgrave's and Benner's analyses offer rich and careful accounts of their chosen subjects. Both books demonstrate that much can be offered by a comprehensive account of the structure and operation of governmental institutions.

MARY FAINSOD KATZENSTEIN

Cornell University

Political Parties in Western Democracies. By Klaus von Beyme. (New York: St. Martin's Press, 1985. Pp. 444. \$39.95.)

As Beyme states at the outset, political parties are one of the central themes of political science; parties were one of the first concerns of our discipline, remain a common topic of research interest, and are relevant to nearly all other aspects of our field. His goal in this book

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is to survey these multiple aspects of parties in a broad comparative perspective. Such a book has been needed for several years as the older classics have aged.

Beyme's book is structured around the comparison of party types, grouped by their ideological tendance: Liberals, Radicals, Social Democrats, Christian Democrats, Communists, etc. This is an innovative approach; it highlights the historical evolution of party forms, and emphasizes comparison of party groups rather than separate descriptions of national party systems. An early chapter defines these ideological groupings, and these types are then used effectively to frame the discussion in chapters on party organization and the structure of party systems.

Another chapter deals with electoral politics, and the changing bases of voters' electoral decisions. This material includes a discussion of the importance of social cleavages and the role of party attachments in guiding citizen behavior. To complete his survey, Beyme hurriedly covers several topics toward the end of the volume. A quick review of coalition formation literature introduces the topic, but does not progress much further. Similarly, a section on policy output is intended to determine whether parties do matter, but the presentation leaves the question unanswered.

Probably the most interesting chapter is Beyme's conclusion on the future of Western party systems. Running counter to the conventional wisdom, he argues that the present crisis of party legitimacy occurs because parties are expanding their roles and performing more functions in the modern welfare state. While Beyme's argument needs further development, it provides a thoughtful counter to standard predictions of party decline.

I think scholars and students will share one criticism of this work: it is simultaneously too broad and not broad enough. On the one hand, Beyme tries to cover almost the entire field of party research, which leaves too little time for many topics, especially when a large sample of Western European parties are included. On the other hand, in an attempt to avoid overgeneralizing, he often errs in the opposite direction. The presentation frequently focuses on the deviant cases while avoiding discussion of general tendencies. Competing causal theories are presented without an attempt to identify primary and secondary

forces. This hesitancy to generalize is reflected in Beyme's frequent criticisms of empirical research, which is interestingly labeled "analytic" research in the book. This study could benefit from more analysis, empirical or otherwise.

This book was originally written as a West German text, and St. Martin's has done a good translation. However, the origins of the book might pose two minor problems for adoption in American university courses. First, the book presumes substantial prior knowledge of the political systems in Western Europe. It would be difficult, for example, for a student to abstract a good understanding of the British party system solely from the material presented here. Second, the literature citations often overlook major research publication in English, in preference for German-language alternatives.

This will not become a classic study of political parties. It was not Beyme's intent, and he studiously avoids the synthesis of party research that would make this a more theoretical volume. He cites Weber to justify the lack of theorizing given the present state of our knowledge! Instead, this is a rich descriptive study of what we do know about political parties. The tabular material, appendices on electoral results in two dozen democracies, reference materials, and Beyme's descriptions of this evidence makes this a basic reference for the comparative study of political parties.

RUSSELL J. DALTON

Florida State University

The Politics of Rapid Urbanization: Government and Growth in Modern Turkey. By Michael N. Danielson and Rusen Keles. (New York and London: Holmes and Meier, 1985. Pp. xviii + 286. \$34.95, cloth; \$19.50, paper.)

The conceptual framework of this study of urban growth and politics in Turkey is adapted from Danielson and Doig, *New York: The Politics of Urban Regional Development* (1982). From this perspective, governments are not regarded as mere handmaids to private economic interests, but are seen as capable of playing an independent role in channeling and

influencing patterns of urban development. In fact, it is argued in both books that the role of government may vary along a continuum from a very passive stance, which merely ratifies decisions made in the private sector, to a highly active role in which government initiates development. It is also suggested that government agencies vary in terms of both areal and functional scope in their urban activities, and that their ability to concentrate resources, which varies from agency to agency, from case to case, and from country to country, can be a critical factor in urban development.

Danielson and Keles have concentrated their resources in this book almost exclusively on a descriptive account of the role of government in Turkish urban growth, with only minimal comparisons with other societies. This is unfortunate, for it is difficult to imagine two urban systems more different than those of New York and Turkey. Thus, if the patterns of governmental involvement in urban affairs are indeed similar, then comparison on the basis of "most different systems" ought to provide some useful insights. The absence of more explicit comparisons of this sort is the more lamentable in view of the fact that the co-authors are eminently qualified to carry off such an effort. Danielson undoubtedly needs no introduction to the American political science community. Keles is one of the most talented Turkish political scientists currently working on urban problems, and has gained considerable international recognition as well.

Danielson and Keles set themselves a two-fold task. First, they seek to assess the effect of urbanization on government, with attention to such phenomena as government structures, finance, intergovernmental relations, bureaucracy, participation, parties, elected officials, interest groups, and public policy. Second, they are concerned with the impact of government on patterns of urbanization, involving such factors as migration, settlement patterns, distribution of jobs, housing, and the location of such facilities as transportation. They undertake this task by outlining first the context of rapid urbanization in Turkey during the recent past, then briefly sketching the major types of cities that exist in the country, and finally assessing the relationships between cities and central government on the one hand, and political interests on the other. With the stage thus set, they then examine the interrela-

tionship between the forces of urbanization and the political system by looking in some detail at three policy areas: urban services, housing, and planning.

Danielson and Keles point out repeatedly that the Turkish political system is highly centralized. Thus, urban leaders are hamstrung. There is very little they are able to do without the agreement or authorization of central government ministries or agencies. They have few resources and less authority. On the other hand, central government agencies are not much better equipped to deal with the enormous problems of rapid urbanization. They, too, lack trained personnel and adequate financial resources. Moreover, they are plagued with overlapping jurisdictions and lack of coordination. In short, Turkish governmental structures involved with urban problems are characterized by centralization of authority and fragmentation of function. The result is that attempts to deal with the scourge of uncontrolled urban growth have been largely ineffective.

Perhaps the most dramatic—certainly the most visible—symbols of urbanism in Turkey, as in most Third World countries, are the mushrooming squatter settlements that ring every city of significant size. Danielson and Keles recount the numerous efforts of Turkish governments, especially nonelected regimes less subject to the pressures of democratic politics, to bring these ugly eyesores of modernism under control. All such attempts fell victim to the chronic ills of Turkish government and politics: inadequate resources, untrained personnel, complexity of government, and pressure from private interests, including, of course, corruption. Indeed, despite the claim set forth by their conceptual approach (that government makes a difference in urban development), a reading of this book leaves one wondering what difference government really does make.

The authors make a number of recommendations, some of which are all too familiar. They suggest that unless future governments are willing to allocate at least 5% of annual budgets for housing, they will be no more successful in bringing the hydra-headed urban monster to heel than were their predecessors. They urge the adoption of clearer planning objectives, and suggest that ways be found "to use the inherent advantages of a centralized

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political system to focus resources in ways that encourage local initiative, leadership, and adaptation" (p. 232). They argue for greater autonomy, resources, and authority for cities, so that they may more effectively assert control over rapacious private interests and imperious central government agencies.

In short, this book is a carefully phrased, well-documented account of the failure of yet another government to control urban growth and development. Unfortunately, the common theme which seems to link the Turkish urban experience to the American (less so, perhaps to the European) is the timeless question the journalistic wag has put in the mouths of the putative builders (past as well as contemporary) of another sprawling metropolis, Chicago: "Where's mine?"

FRANK TACHAU

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Introduction to British Politics: Analysing a Capitalist Democracy. By John Dearlove and Peter Saunders. (Cambridge: Polity Press, 1984; New York: Basil Blackwell, 1985. Pp. 458. \$45.00, cloth; \$14.95, paper.)

State and Society in Contemporary Britain: A Critical Introduction. Edited by Gregor McLennan, David Held, and Stuart Hall. (Cambridge: Polity Press, 1984; New York: Basil Blackwell, 1985. Pp. ix + 384. \$34.95, cloth; \$9.95, paper.)

The word *introduction* appears in the title of both of these books, and both are intended to be texts—McLennan et al. is an outgrowth of an Open University course in Britain. Good luck to anyone trying to use either for instructional purposes in the United States. Dearlove and Saunders do include such topics as parties, interest groups, and the judiciary—subjects almost totally absent from McLennan et al.—but their coverage hardly is comprehensive. These are not texts, but interpretations of British politics and society.

Both books share an almost Germanic obsession with "the state" and seek to reveal how oppressive British society is. Typical of their idiosyncrasies is Dearlove and Saunders' discussion of the civil service and the judiciary under the melodramatic title "The Power of

the Secret State." Although both books are infused with Marxism, they are better described as left-wing radical. Dearlove and Saunders grant that "to be a good political scientist you must be keenly aware of the marxist tradition, and to be a good Marxist you must know 'bourgeois' social science" (p. 6). Know it, yes, but the actual use of it in these books too often is more a matter of scholarly trappings than a method of obtaining knowledge.

Dearlove and Saunders present alternative perspectives that have been offered to explain British politics, assess their logical consistency, and ascertain the extent to which they fit the facts. Somewhat similarly, McLennan et al.'s collection of 10 original essays is intended "to connect a review of relevant empirical material to central theoretical questions about the state and thus to the main traditions of political theory" (p. 2).

The alternative approaches frequently are strawmen, and much of the discussion is fairly predictable. Hall's chapter in McLennan et al. defines representation as "all the ways in which classes or other social forces in society win power in or influence over the state, so as to make it conform more closely to their interests" (p. 7). He concedes that as the franchise was expanded "the popular forces *did* get wider representation in the state: but at the price of remaining a subordinate, rather than the leading or hegemonic element" (p. 44). While the book is not riddled with jargon, the approved phraseology is fairly pervasive.

The heart of Dearlove and Saunders is a discussion of class, gender, and racial inequalities in Britain designed to show that the elite enjoys "the exclusive right to use, control, dispose of and benefit from crucial resources" (p. 181). Much of the evidence in this chapter actually supports the concept of pluralism. "Power, it seems, *is* to some extent, dispersed, fragmented and cross-cutting" (p. 205). But Dearlove and Saunders can't accept this, and press on to show that "systems of domination" do prevail. Eventually they concede that this concept is virtually incapable of empirical investigation. So after nearly 50 pages they conclude that "the sociological analysis of power will always involve controversial and personal judgements about what power is and how it is to be analysed" (p. 217). This hardly is a profound or useful insight; but it all has been worthwhile because "the cosy world of tradi-

tional political science" has been left "a long way back."

Four essays in McLennan et al. trace the growth of state intervention in economic affairs in Britain from the 1880s to the present. They conclude that Britain continues to be a capitalistic system—hardly a surprise. Three other essays consider the state's role in provision of health care, family life, and the organization of science. Another explains how public opinion was manipulated during the interwar period through control of the BBC. McLennan's contribution seems to promise a bit more, since it focuses on parties and the decline of class voting, but his comments prove to be superficial, and he ignores much of the relevant literature.

The final chapter by Held, although excessively long, does provocatively challenge the standard view of Britain as a system with high legitimacy derived from consensus on basic values. In contrast to Dearlove and Saunders, who see legitimation as "the direct or indirect control over people's ways of thinking and acting other than through the use of physical force" (p. 338), Held recognizes that "legitimacy implies that people follow rules and laws because they think them right, correct, justified—worthy" (p. 302). Dearlove and Saunders maintain that the "soft" means of manipulating legitimacy—such as the Church, the press, and the educational system—can be effective only temporarily. Held argues that the British system long has rested more on instrumental or conditional acceptance than on true legitimacy. The basic goal of Thatcherism has been to reduce peoples' expectations of receiving benefits from the government and to use symbolic gratification—pride in being British for its own sake—to generate legitimacy. Since the success of this strategy is uncertain, Held, like Dearlove and Saunders, sees a strengthening of the more coercive means of obtaining compliance in order to protect capitalism.

The fundamental problem with these books is that their writers seem ignorant of Macridis's generation-old distinction between grand and middle-range theory. Dearlove and Saunders justify their approach by asserting that "a political theory which focuses on 'politics' alone will always be limited in its analytical capacity and trapped within the system it needs to explore" (p. 434). On the contrary, grand theory of the type infusing these books

is inherently abstract and incapable of validation. Only use of Macridis's more limited middle-range theory offers hope of generating reliable knowledge. Failing to understand that, these books provide little more than a point of view.

What is most interesting about them is what they reveal about the condition of both British politics and undergraduate instruction. Traditionally the Labour party sought equality of status and protection for the working class. Now Labour is increasingly dominated by middle-class intellectuals seeking to transform Britain. If this movement has an archetypal figure, it is the sociology lecturer at one of the polytechnics. Three of the five authors and editors of these books are in departments of sociology. While none is poly faculty, Dearlove and Saunders are from one of the newer universities, Sussex, whose reputation for left-wing enthusiasms even exceeds that of many polys. And McLennan, Held, and Hall, plus four of the other contributors to their volume, are associated with the Open University, which often is seen as sharing the orientation of the polys. If one is interested in such a perspective, these two books are as good a place as any to get it. Despite the pervasive commitment to a particular ideological framework, there are elements of scholarship as well.

JORGEN RASMUSSEN

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Islam in Revolution: Fundamentalism in the Arab World. By R. Hrair Dekmejian. (Syracuse University Press, 1985. Pp. xi + 249. \$26.00, cloth; \$12.95, paper.)

The Islamic resurgence has been a visible part of global affairs for a number of years. It is now possible to go beyond tentative speculations about the resurgence and base interpretations on a growing body of detailed studies and primary source material. R. Hrair Dekmejian's study of fundamentalism in the Arab world is an important contribution to this "new generation" of analyses of the current Islamic situation.

In this book, Dekmejian's basic interpretation of the Islamic revival remains the same as it was in his 1980 article on this subject (*Middle East Journal* 34(1980): 1-12): the resurgence is

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a product of a multifaceted crisis situation in the Islamic world. In general terms, "Islamic fundamentalism is perceived as a cyclical phenomenon which occurs as a response to acute and pervasive social crisis" (p. 6).

Dekmejian, however, goes beyond the confines of his original interpretation in a number of ways. More attention is given to the historical roots of Islamic fundamentalism, and greater emphasis is placed on motivations that are more than sociopolitical or economic in origin. He concludes that "the Western and Marxist practice of approaching the fundamentalist phenomenon from a socioeconomic perspective can provide at best partial answers" (p. 178). The dimensions of spiritual crisis and moral authority are recognized as important.

Islam in Revolution begins with five chapters of general analysis. The cyclical pattern of the rise and fall of fundamentalist sentiments throughout Islamic history, parallel to periodic social crises, is described. Then a broad portrait of contemporary Islamic fundamentalism in the Arab world is painted. In this, reasons for the appeal of fundamentalism, the major groups involved in revivalist movements, the ideological content of those movements, and taxonomy of fundamentalist organizations are all clearly presented, often in schematic and list form. This latter aspect reflects both a strength and a weakness of this presentation. These lists and charts are very helpful as summaries, but they can give the unwary reader a sense of comprehensiveness and concreteness that is unwarranted.

The second part of the book presents case studies of Islamic resurgence in Egypt, Syria, Iraq, Saudi Arabia, and the Arab/Persian Gulf area. Dekmejian utilizes recent specialized studies of Islamic developments in these areas. However, he goes beyond these in presenting descriptions of the ideologies and programs of movements whose writings are not easily accessible. For example, for the group that killed Sadat, and the ideas of Juhayman al-Utaybi, who was a key leader in the movement which took over the Great Mosque in Mecca in 1979.

Part 3 of the book is a discussion of "consequences and prospects" for Islamic fundamentalism. In this, Dekmejian considers the implications of the Islamic resurgence for United States policy. In his view, what can be expected is "the persistence of protracted conflict

between U.S. interests and Islamist objectives. Indeed, theoretically there can be no ground for mutual accommodation" (p. 173). However, he suggests that a peaceful settlement of the Arab-Israeli conflict and significant progress in social reforms could lessen the tension by "amelioration of the crisis milieu" (p. 174) within which fundamentalism thrives.

The book closes with three helpful appendices. The first is a table providing information about the 91 Islamist organizations on which the study was based. Appendix 2 is an extended glossary of Islamic terms, and appendix 3 is a statistical analysis of the Islamist groups, presenting a general profile of the category, based on the 91 groups studied.

Dekmejian's conceptual framework is clearly laid out for the reader. Because of this, it is surprising that Dekmejian has chosen as a basic unit for analysis a concept that does not seem to be appropriate for his data. He has rather artificially confined his discussion to "the Arab world" in this book, in contrast to his 1980 article, in which he also gives some attention to the important Islamic experiences in Iran, Turkey, and Pakistan.

The underlying analysis for the book depends significantly upon the Iranian case. Dekmejian's discussions of Iraq and the Gulf clearly show that it is not possible to exclude Iran from consideration. The Iranian experience is an important part of each of the case studies. It might have been better to have included Iran in the basic unit of analysis, since it plays such an important role in Dekmejian's presentation. One wonders, for example, if the statistical profile presented in appendix 3, on which important aspects of the study are based, would be altered by the inclusion of Iranian data.

Dekmejian's book is a sophisticated handbook for the political analyst who needs a good summary picture of Islamic fundamentalism in the Arab world. However, it is more than that. It has a breadth of analysis that should be helpful to specialists in modern Middle Eastern and Islamic studies as well.

JOHN O. VOLL

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The Rise and Fall of Chilean Christian Democracy. By Michael Fleet. (Princeton: Princeton University Press, 1985. Pp. 264. \$35.00, cloth; \$14.50, paper.)

Michael Fleet's study of the Chilean Christian Democratic Party helps fill a noticeable void in the literature on Chilean politics and society. The PDC has played an undeniably central role in the Chilean political arena for the past several decades; yet there are few studies that offer a careful examination of the party's historical political fortunes. Fleet sets his analysis apart from Marxist interpretations of the role of the PDC (especially since 1970) that characterize the party as a mere tool of the bourgeoisie. In addition, he critiques pluralist analyses that stress primarily ideological/institutional considerations to the exclusion of class analysis. Rather, Fleet asserts that a "modified class analysis" that highlights the "interdependence of class, cultural, ideological, and political factors" (p. 42) can best explain the rise and fall of Christian Democracy in the Chilean context. In chapters 2 through 5 Fleet examines the actions of the PDC during the pre-1964 period, the 1964-70 Frei government, the 1970-73 Allende government, and the 1973-83 period under military dictatorship. Drawing on his analysis of the Chilean case, Fleet's final chapter highlights certain common characteristics of Christian Democracy in Latin America and Europe.

This study has several significant strengths. The author's use of Eduardo Hamuy's survey data lends weight to his critique of crude Marxist or pluralist interpretations of the PDC's role in the breakdown of democracy in Chile and the institutionalization of a military dictatorship. Fleet shows that elite decision making has reflected the profound internal contradictions between the class and ideological cleavages evident in the party since its early days as the Falange Nacional. His multi-dimensional analysis makes an important contribution to our understanding not only of the rise and fall of the PDC, but also the dynamics of the Chilean political arena over the past several decades.

Fleet's analysis, however, ignores several critical actors that have influenced the fortunes of the PDC—most noticeably, the Catholic Church. Until 1958, the majority of ostensibly "nonpolitical" Church elites continued to legiti-

mate the Conservative Party's claim to be the exclusive representative of the Catholic vote in Chile. The hierarchy's dramatic change of alliance to the PDC between 1958 and 1964 evidenced by the open recruitment for Frei in Catholic labor organizations was a critical factor in the rapid increase in party followers. In fact, Fleet's exclusion of the Church from his analysis is a serious omission throughout his study, especially with regard to the Church's central role in base-level mobilization under the Pinochet regime. Particularly since Frei's death, many within the hierarchy have taken a far more critical stance toward the party leadership. Fleet, with his emphasis on class and ideological cleavages, does not address the impact that the changing relationship with the Church has had on party recruitment on legitimation.

On the other hand, Fleet's emphasis on examining the PDC in terms of the broader Chilean context de-emphasizes several important developments within the party. The author fails to offer new insights concerning the changes in internal party alignments that were critical in the party's historic shifts from support to opposition, both to the Allende government and to the military regime. This study could have benefitted from a closer analysis of the internal party debates, congresses and elections that resulted in significant shifts in PDC strategy in the past two decades. The chapter on the post-1973 period also leaves the reader with nagging questions about the effects of the authoritarian experience on internal party structures and alignments. How has repression affected the party leadership and their strategies for institutional maintenance? How has the opposition to Pinochet affected doctrinal disputes within the party? What is the nature of the PDC's relationship with the newly emerging organizations at the base?

In spite of these problems, Fleet succeeds in his purpose of offering a more subtle and balanced account of the historic role of the PDC in Chile. This study is a welcome addition to the literature, in that it helps refine the historical debates concerning the PDC's contribution to the breakdown of Chilean democracy and the surprising staying power of the Pinochet regime.

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Comparative Resource Allocation: Politics, Performance and Policy Priorities. Edited by Alexander J. Growth and Larry L. Wade. (Beverly Hills, CA: Sage Publications, 1984. Pp. 247. \$28.00, cloth; \$14.00, paper.)

The Welfare State and Its Aftermath. Edited by S. N. Eisenstadt and Ora Ahimer. (Totawa, NJ: Barnes and Noble, 1985. Pp. x + 329. \$27.50.)

Reviewing an edited book is invariably difficult; reviewing two is all the more difficult. The two books to be discussed here contain a total of 32 articles, with at least as many purposes and themes, and it is virtually impossible to develop a common basis for description and evaluation. Unfortunately, neither set of editors appears to have undertaken such an integrative effort for their single volume.

Comparative Resource Allocation, as the name implies, purports to be a collection of studies about the allocation of resources between the public and the private sector, and secondly among programs within the public sector. Unfortunately, the majority of the essays contained in the volume are not comparative (Rose and Karran, Rothchild, and Montgomery stand out as contrary examples here) and the editors have done little or nothing to attempt to provide a comparative framework that could attempt to integrate the diverse topics and approaches of the authors of the individual essays. The essays cover countries ranging from western industrialized countries through the Soviet Union to Third World countries. The editors' introductory essay, rather than providing a means of integrating the allocational issues raised in the individual articles or a framework to which the authors might have reacted, attempts to demonstrate that regimes can be classified by their pattern of resource allocation. The variables selected to predict the location of the 91 countries appear somewhat unusual as ways of assessing the role of the public sector (Why passenger kilometers per capita, or industrial employment per 1000 population?), and the approach seems to conflate those variables with the more political characteristics of regimes inherent in concepts such as polyarchy. At the other end of a quantification spectrum, several of the essays were almost totally descriptive. The remaining essays (especially Rose and Karran, Rothchild, and Most) showed some attention

to descriptive detail, but also with some interest in addressing important political questions, and are useful individual efforts that have not been brought together into a real book.

The Welfare State and Its Aftermath is a more (though far from totally) integrated set of essays all having something to do with the welfare state in industrialized political systems, and the problems that currently appear to beset these political and economic arrangements. While the title of the book, as well as the first essay, tends to beg the question about the waning viability of these arrangements, the book does address an important question, and generally does so in an informed and interesting manner. One must comment, however, on the extreme unevenness of the essays in length and scholarly content. It is certainly interesting to have the comments of practicing politicians directly involved with making policy for the welfare state, but an "article" of two pages with a few tables of data appears to add little to our understanding of the complex issues involved. The book is the product of a seminar, and it would appear that at least some of the essays went directly from the tape recorder to print.

There are, however, several other things that commend this book. One is the large number of data tables presenting interesting and useful information. Too often publishers refuse to print such copious amounts of data. A second point to be commended is the emphasis on the normative and intellectual elements of the "crisis" in welfare states, as well as on the economic problems of financing growing expenditures. Financing the welfare state is easy—it only requires money. The more difficult task appears to be to understand the intellectual and moral basis of the system and the attacks on those values. In regard to the last comment, it is interesting to note the extent to which commentators of all ideological stripes represented in the book have at least some doubts about the moral basis of the claims of the welfare state on the money and interests of citizens.

Another important positive point of the *Welfare State* is its analysis of programs and prospects in politics systems, which are not usually given much space when the welfare state is the topic of discussion. There are interesting discussions of Japan and Switzerland, as well as a briefer discussion of Austria. These

are in addition to an excellent discussion of more commonly analyzed Scandinavian countries, and several interesting discussions of the Anglo-American systems. There is also an extended set of articles about welfare state programs in Israel that demonstrates the difficulties of attempting to be a welfare state and a warfare state simultaneously.

Perhaps the most interesting point in this volume is the importance of the public bureaucracy and public employment in the welfare state. Martin Rein discusses this as a dependent variable and points to the importance of the employment generated—directly or indirectly—by welfare state programs and the special role of those programs in providing employment opportunities for women. Several articles point to the negative reactions generated in clients by the “bureaucracy” surrounding welfare state programs. However, Johansen and Kolberg point to the importance of government employees in maintaining the welfare state in the face of attacks. While some or all of that defense is obviously self-protection rationalized in the interest of the client, it may have the effect of preserving benefits for the very clients who complain of being mistreated by public employees.

Both these books address important topics, and each contains interesting and informative essays. What both lack is greater integration and a common framework for analysis.

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Ethnic Groups in Conflict. By Donald L. Horowitz. (Berkeley: University of California Press, 1985. Pp. xiv + 607. \$25.00.)

It is only underscoring the obvious to note that the early optimistic expectations of both scholars and politicians that ethnic differences would dramatically decline in the face of national homogenization or class consciousness were at least premature. Rather, the world has experienced a wide range of ethnic conflicts which have frequently resulted in violent confrontations. As the author asserts, “A bloody phenomenon cannot be explained by a bloodless theory” (p. 140). While Professor Horowitz does not quite provide us with an over-

arching theory to explain ethnic conflict, he does give us a very rich volume in terms of scholarship and analysis. The questions he seeks to answer and the conceptual frameworks he provides are stimulating and reinforced by extensive research. While the specialist on particular areas may quibble with specific interpretations of the facts or differ with regard to some of the author’s assumptions, they are presented with care and a realization of the complexity of his subject matter.

The units of analysis are relatively large ethnic groups from severely divided societies in Asia, Africa, and the Caribbean that are separated by vertical cleavage and defined by ascriptive differences. The first section seeks to define the meaning of ethnicity and the scope of the problem, assessing the wide range of strife and convulsions attendant thereto, in addition to the causes and characteristics of ethnic conflicts. His analysis is comprehensive in its review of possible factors responsible for the multifaceted nature of the problem, and posits a series of interesting hypotheses and conclusions. For example, he finds ethnicity is frequently more compelling than class, and not merely a mask for social class conflict; questions assessments of modernization as an explanation for severe ethnic conflict; and argues that the theory of group entitlement can be a strong forecasting tool.

The second major section of the volume analyzes the relationship of ethnicity to party systems, emphasizing the importance of ethnic parties and showing under which conditions different patterns emerge. These party systems are composed of (1) ethnic parties, (2) ethnic parties and coalitions of ethnic parties, (3) ethnic parties and multi-ethnic parties, and (4) multi-ethnic single parties. The author provides lengthy case studies to exemplify his categories, including Guyana and Trinidad, Sri Lanka, Nigeria, Uganda, and Malaysia, as well as shorter examples primarily from Afro-Asia.

The third section covers an area about which Professor Horowitz has already written extensively, the military. As he points out, the military is both a source and object of ethnic conflict, and “ethnic coupmaking” can have profound effects upon the composition of both military regimes and the armed forces themselves. Here he begins to turn to ways of ameliorating ethnic conflict, in this case considering strategies to prevent ethnically oriented coups.

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These include homogenizing the armed forces, balancing within the officer corps or balancing by creating new units, employing foreign forces, and kinship control.

The final major part is in instructive analysis of possible means of reducing ethnic conflict in the modern nation state. The author critically examines the employment of European models such as consociational democracy, finding little relevant for the Third World. He also gives relatively short shrift to such solutions as comprehensive agreements, partition, and international integration. Treated more seriously and extensively are federalism and devolution, electoral systems (although there are few Afro-Asian states where competitive voting is very meaningful), and preferential policies. In each case these possibilities are examined with caution, giving proper consideration to possible costs and benefits. Professor Horowitz terms the problem of ethnic conflict intractable, but not absolutely so. Even that assessment may be overly optimistic, however, as evidenced by the author's own analysis.

This is an important and comprehensive contribution to our understanding of ethnically based conflict. The wide range of emphasis, particularly those made in passing, may confuse the uninitiated, and the book could use either a bibliography or an index of authors. However, these are minor criticisms of a work that is stimulating, well researched—the extensive footnotes are worth reading in themselves—and thoughtful.

FRED R. VON-DER MEHDEN

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Government and Politics in Islam. By Tareq Y. Ismael and Jacqueline S. Ismael (New York: St. Martin's Pres, 1985. Pp. vii + 177. \$27.50.)

The title of this book is bound to be misleading. It will disappoint those readers expecting to find an overview of Islamic political institutions or analyses of the contribution of Islamic ideas to contemporary government. One finds nothing resembling a comparative study of legal systems, the adaption of representative structures, or Islamic prescriptions for modern social and economic problems. The book is not in fact concerned with all Islamic states, but

only with the Arab countries—most notably Egypt—and with Iran. A more accurate title would have been "A Brief Examination of Popular Islamic Thought and Political Activism in Selected Cases." Viewed in these terms, the volume provides an often useful survey of what Islamic political theorists have written through the centuries on such themes as political succession, legitimacy, and accountability. The study also examines the reformist tradition in Islam: essentially what several major thinkers have said about such topics as modernization, westernization, and socialism.

Description of the Islamic Brotherhood and other militant fundamentalist groups in Egypt comprise much of the second half of the book. The Iranian case is also explored in synopses of the ideas on government of Ayatollah Khomeini, Ali Shariati, who sought to fuse Islamic principles with socialism, and former president Bani-Sadr. A final chapter offers capsule accounts of the current status of Islamic activism in a number of Arab countries. Official regime responses to the challenges presented are, however, of far less interest to the authors, and no attention is given to the defensive reactions of establishment *ulema*. The present Egyptian government's policies in coping with religious militants are ignored, as is the control of the Shi'ite opposition in Iraq. Also curiously absent are any mention of the Islamization program of Pakistan's Zia ul-Haq and stranger still, attention to the ideas of Abdul Aala Madoodi, which have contributed significantly to activist thought in the Arab world as well as in Pakistan.

If Ismael and Ismael eschew writing a more comprehensive text, they have nonetheless opted for a textbooklike approach in another sense: they venture few independent judgments. Theorists are sometimes compared, but only rarely are their ideas dealt with critically or their impact assessed. In the same vein, a thin treatment of contemporary international Islamic organizations in the final chapter concludes without a word about their effectiveness or role in Islamic revitalization. The authors append newspaper interviews with three Arab heads of state and an address by Anwar Sadat that are intended to illustrate religious symbolism at work. Yet with these materials analytically detached from earlier discussions, they seem to be mostly filler. Ismael and Ismael

have succeeded in describing accurately the intellectual foundations of Islamic political activism, and have highlighted the activities of some important militant groups. Yet even within their limited conception of the task, the book is incomplete and unprovocative.

MARVIN G. WEINBAUM

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Portugal's Political Development: A Comparative Approach. Walter C. Opello, Jr. (Boulder, CO.: Westview Press, 1985. Pp. xii + 235. \$21.00.)

It is unfortunate when younger scholars feel they must "make it" in the profession over the backs of their predecessors by misrepresenting and "straw-manning" their positions, presumably thereby making it easier to knock them over or blow them away.

In this book the author refers to a particular group of scholars as "corporatists," as though that represented their own political and personal preferences. This is false and misleading. Quite a number of scholars in the profession have used a *corporatist approach* without being themselves, corporatists; in fact, most of us who have written on these themes have been in our own writings quite critical of various corporatist regimes. Such guilt by association cannot be allowed to stand.

The author is also strongly critical of what he calls "culturalist" interpretations. It is never made clear what precisely, he means by that, but again there is a lot of misrepresentation and straw-manning. No serious scholar in recent times has sought to elevate political culture to the status of a single and all-inclusive explanation. Rather, those who analyze the political culture variable tend to see it as complementary to other variables. At times political culture may itself shape or even cause certain structural outcomes; at other times it is determined by them. Such causative webs tend to be complex, and their interrelations may vary over time and circumstances. They are not usually the simplistic schemes the author of this book suggests.

Curiously, despite this earlier criticism, the author himself concludes by emphasizing the importance of cultural variables. He talks at length about the creation of an "official cul-

ture" in Portuguese history—precisely what earlier scholars emphasized. His research findings emphasize strongly the continuity of Portuguese political culture despite the upheavals and structural changes occasioned by the Portuguese Revolution of 1974. His concluding position, after repeatedly waxing both negative and positive on the subject, seems remarkably akin to the more complex statement of multicausality of Gabriel Almond, Sidney Vera, and other scholars who have written in sophisticated ways about political culture.

Actually, once all this methodological and ideological baggage and heavy posturing are stripped away, this is an interesting and quite good book. It seeks to examine Portuguese political development in the broader and comparative context of Western Europe. It uses an approach borrowed from *Crises and Sequences in Political Development*, by Leonard Binder et al., and does so provocatively and imaginatively. The author says that his purpose is to contribute to the "maintenance and betterment of democratic government itself" (p. 3)—a laudable goal, but one that sometimes biases his approach and leads to his neglecting some not-very-democratic features of Portuguese political life.

This life nonetheless goes on to make a significant contribution. The author usefully surveys 10 centuries of Portuguese history and provides a quite sophisticated analysis of contemporary Portuguese politics. The book contains an excellent analysis of the Portuguese administrative state system, which is imaginatively combined with a survey of the administrative elite. It provides interesting data on local government structures and institutions.

In his conclusion Professor Opello suggests both how little the Portuguese administrative-state machinery has changed since the Revolution and, concomitantly, how weak are the Portuguese attachments to their new liberal and democratic institutions. In his stress on the persistence of administrative structures and an elite-dominated, top-down decision-making process, the author repeatedly points to the elements of continuity prevailing both before and after the Revolution. These are valuable research findings, but they are precisely those of the earlier authors whom Opello criticizes. Hence, while this is a useful study of a country to which political scientists have paid insuffi-

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cient attention, it is marred by the animus and misrepresentations with which its author began.

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The New Mediterranean Democracies: Regime Transition in Spain, Greece and Portugal. Edited by Geoffrey Pridham. (London: Frank Cass & Co., Ltd., 1984. Pp. viii + 193. \$29.50.)

Successful transitions from an authoritarian political system to a liberal democracy require the prudent hurdling of numerous obstacles. While potential barriers are often specific to the country in question, comparative analysis helps to cull behavioral patterns characteristic of these transitions. This collection of essays, which originally appeared as a special issue of *West European Politics* (vol. 7, no. 2, 1984), considers three European cases that almost simultaneously experienced a democratic transition. It gives specifics on each case, and presents comparative insight into the processes of this type of political change.

The volume contains an introductory essay by Pridham and three sections of three essays each. Part 1's historical chapters describe Spain's, Greece's and Portugal's political antecedents and their paths to democratic rule. Part 2 focuses on these three countries' party systems, the changes they have undergone in response to their often volatile context, and the role different parties played in the reforms. Part 3 is the book's strongest analytical section. It comparatively considers selected topics pertinent to the transitions, such as socialism, the international context, and government performance.

The comparative perspective offered by Pridham's introductory chapter and the essays in part 3 by Salvador Giner, Alfred Tovas, and Guiseppe Di Palma make this book a strong improvement over similar works on Southern Europe—e.g., Bete Kohler's *Political Forces in Spain, Greece and Portugal* (London: Butterworth, 1982). Pridham's introduction sets the tone with an excellent discussion of a model, inspired by Dankwart Rustow, that

questions theories of democratic transitions. This essay also frames later discussion of the three cases in a strong analytical context of broader questions of political change, the art of comparison in political analysis, and models of political systems. Giner's chapter in part 3 on socialism in Southern Europe, with its struggles, adaptations, and differences in these countries, adds to the strong comparative orientation of *The New Mediterranean Democracies*. The same applies to Tovas's comparative discussion of international influences on these transitions such as economic difficulties, the EEC, and NATO, and to Di Palma's essay, which, like Pridham's, seeks a theoretical perspective though which to evaluate these and other cases. Di Palma analyzes the linkage between government performance and political legitimacy.

The case study chapters cover less original ground. In part 1, Kenneth Medhurst presents a concise overview of the major events and forces in Spain's nascent democracy, referring with varying emphasis to the parties, the Church, the monarchy, the military, and Spain's regional diversity. He emphasizes the flexibility of Spain's 1978 constitution as a framework within which difficult decisions could be resolved or temporarily postponed. P. Nikiforos Diamandouros's chapter, on the other hand, is not as optimistic. With more convoluted prose, Diamandouros emphasized Karamanlis's role in the Greek transition, the political *carte blanche* handed to Karamanlis after the discrediting of Greece's authoritarian rulers following the Turkish invasion of Cyprus, and the importance of Karamanlis's and Papandreou's charismatic and politically dominant personality. While more analytically interpretive than historically descriptive, Diamandouros's belief that the new Greek system is more presidential than parliamentary does not seem to be confirmed by Prime Minister Papandreou's recent successful showdown with President Karamanlis. Such questioning contrasts with Thomas C. Brumeau's insightful description of Portugal as a semipresidential system, closer to the French Fifth Republic than the Westminster model of parliamentary democracy. Brumeau cautiously yet optimistically suggests that despite widespread participation, Portugal's democratic system has not fully developed its structural arrangements.

In part 2, Mario Caciagli, Christos Lyrintzis,

and J. R. Lewis and A. M. Williams discuss the party systems of Spain, Greece, and Portugal, respectively. Utilizing Sartori's model, Caciagli emphasizes the late start of the Spanish parties during the transition and their relative lack of historical continuity. He suggests that the parties' policy convergence and ambiguity makes them consistent with West European party systems. Such behavior, however, has come at the price of little support for the parties themselves, and led internal crises in several parties. Lyrantzis, in comparison, believes the Greek party system is a "combination of new elements and old practices" (p. 116). He questions whether Greece's transition has taken it beyond clientalism to the politics of mass parties. He suggests other factors, such as Greek foreign dependence, class cleavages, and growth in the state bureaucracy, have also influenced Greek party behavior. Lyrantzis adds that the Socialists' newness has prevented the structural permanence of a European mass party, that the communists are prisoners of an "ideological ghetto," and that the rightist New Democracy failed to develop a mass party structure because, prior to its 1981 defeat, it relied on "bureaucratic clientalism." Lewis and Williams, for their part, argue that the Portuguese political parties are socially and territorially based: conservative in the rural north and leftist in the south, with its urban centers and large rural landholdings. They suggest that, as in Greece, personality plays a strong role in Portuguese party politics. Coalitions among parties, given changing relationships between themselves and the Portuguese presidency, however, are the most important developments to watch.

This book's 10 chapters are well integrated. Pridham has done a commendable job of avoiding many failures of essay collections. Both the sections on specific cases—while under topical umbrellas of historical processes and party systems—and the broader comparative analysis of his own introduction and the third section's three essays are well chosen. Some inevitable unevenness in prose nevertheless appears, and several chapters repetitively analyze certain events. These weaknesses, however, do not seriously tarnish the otherwise excellent writing, compiling, and editing that went into this book.

In sum, *The New Mediterranean Democracies* is an important contribution to our sub-

stantive understanding of the recent transitions to democracy in Southern Europe. It will be useful to both scholars and advanced students interested in contemporary Spain, Greece, and Portugal, and to area specialists of Southern and Western European politics, especially since Europe's southern nations are increasingly standing shoulder-to-shoulder in international affairs with their northern counterparts. This book also adds to the growing comparative literature on transitions to democracy as a particular type of political development—a concern equally important beyond the Mediterranean.

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Decentralization: The Territorial Dimension of the State. By Brian C. Smith. (Winchester, MA: Allen & Unwin, Inc., 1985. Pp. 227. \$25.00, cloth; \$12.00, paper.)

Brian Smith's topic has been labeled "one of the great, resonant themes of contemporary politics" by Nelson Polsby. Throughout the developed and developing world, decentralists argue for deliberate efforts to counteract the centralizing tendencies in modern political systems, tendencies that undermine political democracy and impede economic development. Many isolated case studies of decentralist policies exist—for example, in collections edited by L. J. Sharpe (*Decentralist Trends in Western Democracies*; Sage, 1979) and Peter Merkl (*New Local Centers in Centralized States*; University Press of America, 1985)—but there is little comparative theory.

This book's jacket promises "the first book to provide an analytical framework for the comparative study of decentralization." What Smith offers instead is a catalogue of most of the observations about, and rationales for, decentralization existing in the literature on intergovernmental relations. He defines decentralization as the "delegation of power to lower levels in a territorial hierarchy" (p. 1), and concentrates therefore on the institutional relationships between higher and lower levels of government.

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A well-published authority on this topic, Smith displays an encyclopedic knowledge of the literature and assembles a bibliography that is surely one of the most comprehensive of its kind. At some points in the text he demonstrates impressive skill in summarizing large bodies of material, as in his chapter on "Financing Decentralized Government," which contains an excellent survey of recent trends in local government taxing and spending in Western Europe. Another strong section is the early chapter on "Decentralization in Theory," in which Smith surveys the theoretical views on political decentralization held by three ideological schools: liberal demographic theorists, public choice economists, and Marxists. He criticizes the "romantic idealization" of decentralization in liberal democratic theory, and also finds public choice theory inadequate as a defense of decentralization. Surprisingly, he takes a completely uncritical stance toward Marxist interpretations of intergovernmental relations, never openly acknowledging the Marxists' central dilemma: that although they decry centralized political power as the handmaiden of monopoly capital, they are nevertheless suspicious that attempts to decentralize power may produce conservative outcomes, because economic elites often dominate lower-level units of government and because governmental fragmentation inhibits the redistribution of resources.

Despite Smith's wide-ranging knowledge of the subject, the book suffers from the common affliction of such surveys—it offers little more than a hurried and uncritical run through a vast literature. Indeed, many of the chapters read like annotated lists: of the forces compelling decentralization in the modern state (chapter 3); of the criteria commonly used to draw subnational boundaries (chapter 4); of the factors contributing to centralist trends in modern states and the indicators commonly used to measure degrees of decentralization in governmental systems (chapter 5); of the types of local government institutions used in different systems (chapter 7); of the reasons why central administrative bodies set up field offices and the types of field administration commonly used (chapter 8); and so forth.

Fundamentally, this book is an exercise in classification; Smith's primary contribution lies in devising the categories and sorting the information. Once sorted, however, the infor-

mation is not used to draw conclusions, to construct hypotheses, or even to speculate about relationships between categories. In fact, Smith explicitly disavows any theoretical ambitions in his preface.

Smith's determination not to take a theoretical stance prevents him from fulfilling the tantalizing promise that appears early in the book. From the preface onward, Smith reiterates his conviction that a nation's choice of decentralized institutions is essentially a political rather than a technical one. Yet he provides no insight into the relationships between political and institutional variables. He only notes vaguely that the relative degrees of centralization and decentralization in societies reflect "the power of different groups to promote and defend their interests" (p. 59); that "conflicts within local communities will include different reactions to central interventions, depending on whose interests the centre is perceived as promoting" (p. 98); and that choices of local government institutions "are made in political circumstances—that is, when interests are in conflict" (p. 141). The reader gains little understanding of which groups are likely to favor which intergovernmental forms under different sets of circumstances.

The one intriguing exception is Smith's brief discussion of the political interests that have shaped Nigeria's institutional choices. He observes that, in general, the emerging bourgeoisie in developing countries depends on national political power, and therefore opposes decentralization (p. 194), and then describes the Nigerian case, where a realignment of state and local relations was crafted expressly to contain the power of traditional political elements in the countryside, and to assure the new entrepreneurial class better access to the investment capital sources that are controlled by state governments (p. 195). This brief section on Nigerian politics, one of the best in the book, shows what Smith might have achieved had he used his broad knowledge to advance the theoretical debate instead of merely to codify the existing literature.

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Shop Floor Bargaining and the State: Historical and Comparative Perspectives. Edited by Steven Tolliday and Jonathan Zeitlin. (New York: Cambridge University Press, 1985. Pp. vii + 261. \$34.50.)

Shop Floor Bargaining and the State is a collection of seven essays on the complex and contradictory relationship between states and industrial relations. Jonathan Zeitlin's introductory chapter critically reviews theories of industrial relations and theories of the state. Skeptical of the possibility of adequate general theory, he suggests some tentative understandings that he believes should underlie empirical research. The other essays, informed by these understandings and located firmly within more specific historiographical and sociological debates, are empirical studies of public policy and industrial relations in particular historical periods.

The overall argument of the book is that there is an ambiguous relationship between the state and the shop floor. Zeitlin argues against the prevailing political and theoretical consensus that there is an inherent deep and pervasive conflict between the state and labor movements has been a central feature of modern politics, so has the enactment of public policy that enhances the autonomy of workers' organizations and undermines managerial prerogatives. Zeitlin suggests that liberal and Marxist theories of the state and of industrial relations are prevented, by their reductionist and determinist assumptions, from accounting for the contradictory relationship between the state and shop floor bargaining.

Liberals and Marxists alike, Zeitlin suggests, have analyzed the state as a passive respondent to pressures and imperatives generated within civil society. Drawing critically upon the realist tradition, he suggests that states pursue goals that cannot simply be derived from the play of interest groups, the needs of the economy, or the class structure, and that the autonomy and coherence of states vary greatly over time and space. States are distinctive, often internally contradictory structures with specific histories. In addition, he argues that liberals and Marxists have concurred that official trade union organization and formal collective bargaining procedures inherently tend to institutionalize conflict and contain industrial disorder, and that collective bargaining is deter-

mined by market constraints. Zeitlin suggests that, in fact, shopfloor bargaining cannot be understood in so determined and fixed a manner, without reference to social actors' own definitions of their situations, the limits of managerial strategies for controlling work, or the complexities and variability of market competition. The book, then, is an argument for more historically informed understanding of the contingent encounters between public policy and shop floor bargaining.

The case studies present extensive, carefully collected, and well-interpreted evidence in support of the book's key arguments about the state and the shop floor: that particular states have acted to restrain management and enhance workers' bargaining power, as well as to constrain labor; that different sections of the state have different, sometimes conflicting orientations towards labor; and that state policies toward labor do not necessarily produce intended or unchanging results. Alastair Reid discusses the relationship between the British state and industrial relations in shipbuilding during the First World War, Noel Whiteside discusses British public policy and port labor reform between 1911 and 1950, . . . Steven Tolliday discusses government, employers and shop floor organization in the British motor industry between 1939 and 1969. Howell Harris discusses the roles of politicians, bureaucrats, employers and unions in the shaping of federal labor relations policy in the United States in the 1930s and 1940s, and Bryn Jones compares the impact of the state on labor control in the British and American aerospace industries. Giovanni Contini surveys changing public policy and shop floor bargaining in postwar Italy.

The book, however, does not make explicit why certain historical cases have been selected and what their analytic significance is. Many of the studies, for example, focus upon "strategic" industries—shipbuilding, docks, motors, aerospace—and upon wartime history. Realist theories which assert the independence of the state from civil society and stress its security functions, are likely to be strongest in these particular cases. However, the book also shows that the exigencies and legacies of war are present in more "normal" historical periods. Jones' interesting essay on aerospace shows how British and American states have shaped product markets and industrial struc-

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ture, as well as labor relations directly, in a war industry, and many of the essays show how institutions and forms of regulation initially established in wartime are maintained. Similarly, the cases that illustrate division and conflict within states are studies of what are generally regarded as uncoordinated or "weak" states, Britain and the United States, and Harris himself regards the New Deal as a period of exceptional policy incoherence even for the American state.

The case studies also seem unrelated to one another, though the Jones essay offers some reflections on the legalistic industrial relations framework whose emergence Harris so interestingly describes. Only the Jones essay is itself comparative, in the sense of systematically addressing similarities and differences and trying to generate an interpretation or explanation based upon comparison. It is also located within current debates over the labor process rather than in historiographical currents.

While the principles of selection and assessment of the case studies are not entirely clear, the essays offer a valuable set of perspectives on the theory of the state, the labor process, and particular episodes of public policy and labor history. Individually the essays are models of clarity and careful scholarship.

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The Three Worlds: Culture and World Development. By Peter Worsley. (Chicago: University of Chicago Press, 1984. Pp. xiv + 409. \$28.00, cloth; \$12.95, paper.)

This extended essay by a senior British sociologist is a contribution to development theory or, as the author puts it, "meta-theory." In spatial terms the coverage is comprehensive, notably in the final chapter devoted to a critique of the current convention of dividing the world into three parts. The author recognizes that, for a long time, the prime focus of loyalty will be the state. Worsley is, nevertheless, a determined adherent of holism, not only in terms of the ultimate unity of human society, but in his search for a unified theoretical approach. Convinced of the "relative autonomy of subcultures," he seeks all the same to avoid

the earlier sociological relegation of society to "at best, a mosaic or aggregate of subcultures" (p. 53). Chronologically, Worsley's range is not quite so wide; essentially concerned with modern development, he concentrates on the last century and a half.

Because it touches on hundreds of contemporary and historical topics, the book is not easy to assess in a brief review. Even the conceptual framework, which unrolls in a leisurely and occasionally contradictory fashion, requires close scrutiny. For example, while Worsley sometimes draws on anthropological usages, he ultimately resorts to a residual definition of culture as "that which lies outside the sphere of political economy" (p. 60). Perhaps the author could have avoided such a loose definition by using recent explorations like Peter L. Berger and Thomas Luckmann's *The Social Construction of Reality*, or, still better, by resorting to examinations of specific culture contexts that especially interest Worsley, such as Roger Eastide's *Les Religions Africaines au Brésil* [African religions in Brazil]. Apparently the residual definition is intended to be an instrument to combat "economistic" brands of Marxism and neo-Marxism (Worsley cites Immanuel Wallerstein's center-periphery theory among others) and to reject the deterministic dichotomy of base and superstructure. "Only by an act of supremely economistic intellectual violence can a mode of production be thus wrested from its social and cultural matrix, and, by fiat, given the status of a causally prior social sub-system" is Worsley's (p. 105) trenchant criticism of Marx's own neglect of the religious factor. Instead of such reductionism, the author is convinced that one must begin by ascribing equal importance to cultural and material factors.

Nevertheless, Worsley—who readily admits his Marxist antecedents—continues to draw heavily on Marxist categories. He stresses that two of his five chapters are devoted to conventional "underclass" categories—peasants and workers. His preferred interactionist approach tends to locate exploiters and exploited in every social situation, and even to sympathize with the notion that "people fight the class struggle every day of their lives" (p. 180). More significant is the way the author treats ideas and movements, if not "culture" itself, as reflections of group interests: "Ideas, that is, become significant social forces...because

they legitimize, ideologically, the privileged position of those who profess those beliefs" (p. 240). Believing, as I do, that the concepts of false consciousness and ideology are the most enduring elements of Marx's legacy, I should not wish to reject applying this interesting interpretation to some situations. At one point, moreover, Worsley does briefly indicate the problematic nature of "ideology." In his frequent practical applications, however, he fails to stipulate the kinds of privileges and privileged groups that may turn ideas into ideology. This lacuna, plus his use of terms like "incipient class consciousness," leads one to suspect that Worsley has slipped back into assuming the Marxist interpretation that makes cultural factors epiphenomenal.

The tendency is most apparent in chapter 4, Worsley's most extended discussion of a non-Marxist category, ethnicity. Although recognizing that cultural attitudes that produce ethnic identities are historically prior to class alignments or even interest motivations, he argues that

cultural traits are not absolutes or simply intellectual categories, but are invoked to provide identities which legitimize claims to rights. They are strategies or weapons in competitions over scarce social goods. . . . Ethnic identities, tribal or national, may be retained or shed, old ones invested with new meanings, new ones may emerge and become readily adopted; the crucial consideration being whether they are useful. (pp. 249-50)

Pursuing this explanation, which he recognizes to be Marxist, Worsley scarcely explores the alternative—that long-term ethnic attachments, although not truly "primordial," retain their strength precisely because the human condition makes them irreplaceable sources of psychic security rather than mere "nationalist mystifications."

For the author's theoretical aspirations, a more serious problem is how to develop a prescription for attaining the "infinitely better society" he believes it is possible to create. Like many Marxists before him, he documents, by innumerable illustrations, the extent of contemporary misery. After demolishing, ostensibly, most of Marx's theoretical structure, Worsley remains convinced that Marx's "normative and conative" elements remain his real contribution. In particular, Marxism has inspired "the dedication to working out better

ways of living together in the future of getting rid of institutions and habits of thought developed over the centuries" (p. 40). Without Marx's "motor" of history, it is rather hard to see how those he inspires can know what to retain and reject from the world's cultural heritage. At one point, Worsley turns to Herbert Marcuse's fashionable emphasis on exploitative consumption. Elsewhere the author worries about environmental deterioration, drawing the conclusion (equally fashionable among some British socialists) that "the problem, moreover, is not one of mobilizing people in order to create wealth, but the equitable distribution of the wealth that already exists" (p. 344). None of this seems very likely to appeal either to orthodox Marxists or to pragmatic leaders in developing countries. To what extent does wasteful consumption really reflect long-established cultural patterns as contrasted with values imposed from outside, and how much does population growth arising from the inertia of family patterns require the creation of new national wealth? In this context, too, close analysis of specific cultural traits in lieu of treating the category as residual would have been rewarding.

In the final analysis, it seems to me that Worsley's notable contribution is his flexible critique and openness to new ideas from numerous schools of thought, rather than theory-building. He cogently warns against "what anthropologists will recognize as avoidance and incest: reading nothing but the growing volume of specifically Marxist literature" (p. 40). *Mutatis mutandis*, this is a warning the non-Marxist theorist can equally take to heart. Worsley's own reading is most illuminating for recent Ibero-American social criticism. One might wish he had used the riches of the *Annales* school, particularly Fernand Braudel's last great production, *Civilisation Matérielle, Economie et Capitale* [Material civilization, economy and capital], which seems a natural counterpoint to Worsley's essay. Nor am I convinced that one can really grasp the impact of Marxism on culture and historical interpretation without access (unfortunately only via German and the East European languages) to recent Hungarian, Polish, and Czech works such as Ferenc Tökei's four volumes. As Braudel dryly comments, "the only thing lacking in this first part of the twentieth century is a masterpiece of Marxist history which might

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have served as a model and a rallying point. We are still waiting for it"—and it seems the wait will be long. All the same, Worsley's openness, combined with a fearless critical spirit, has provided a powerful stimulus for

getting the meta-theory of development off to a fresh start.

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INTERNATIONAL RELATIONS

Liberal Protectionism: The International Politics of Organized Textile Trade. By Vinod K. Aggarwal. (Berkeley: University of California Press, 1985. Pp. xii + 302. \$27.50.

Aggarwal's *Liberal Protectionism* documents the unraveling of the liberal trading system in international textile trade. This is an important book, in that it offers both a theoretical and empirical analysis of the international textile regime from the perspective of international politics. While domestic political factors play a role in this analysis, they are combined with systemic-level explanations as well as with learning on the part of individual negotiators to explain changes in the international regime governing textile trade. *Liberal Protectionism* first develops a theoretical framework for understanding regime transformation, and then tests the theory using the creation, maintenance, and destruction of the international textile regime to determine its validity.

Aggarwal asks three key questions in this study: Why are regimes developed? What accounts for the strength, nature, and scope of regimes? And finally, what effect do regimes have on national behaviors and transactions? (p. 4). The structure of the international state system provides the context for Aggarwal's theory of regimes. He argues that the incentives for states to create regimes rather than impose unilateral controls or negotiate restrictive bilateral agreements include "nesting"—that is, the need to prevent larger institutional arrangements from being undermined—the need on the part of a state to control the behavior of other states through a system of

rules, and the need to reduce transaction and information costs in negotiations.

Aggarwal argues that the strength of a regime is accurately explained by the theory of hegemonic stability, which predicts strong regimes when a single power is dominant (p. 4). The nature of a regime in this context refers to the degree of "openness" supported by the regime, which varies between liberalism and protection to the range of issues and products regulated by the regime. This is an important variable in textile trade, because the regime began with the regulation of cotton textiles and then was expanded to include wool and man-made fibers. The behavior of countries refers to the bilateral negotiations restricting textile trade that occurred in the wake of changes in the multilateral textile regime. Transactions are the trade introduced by the restrictive agreements.

Aggarwal adds to the theoretical debate on international regimes through developing a distinction between regimes and meta-regimes. International regimes are multilateral systems of rules and procedures designed to regulate national actions. These rules and procedures are embedded in "meta-regimes," which are the principles and norms that inform the development of regimes. This framework makes it possible to analyze changes in the rules and procedures of a regime while the norms and principles remain constant, and allows for the synthesis of cognitive and structural explanations of regime change. Aggarwal argues that while structural explanations can best explain changes in the regime (the rules and procedures that regulate national actions), cognitive theories can best explain changes in the meta-regime (principles and norms). Thus,

hegemonic stability theory and transactions cost explains the creation of regimes, while the development of consensual knowledge among states permits the creation of meta-regimes.

Aggarwal then carefully traces the creation, maintenance and destruction of the regime in textile and apparel trade. He begins his account with the negotiation of the first postwar voluntary restraint agreement with Japan. The remainder of the empirical analysis focuses primarily on the negotiation of the Short and Long Term Arrangement on Textiles, the Multi-Fiber Arrangement (MFA), and finally the destruction of the Multi-Fiber Agreement due to the inclusion of the mutually agreed-upon departures clause. This clause allowed textile importers to negotiate bilateral agreements with exporters, which were more restrictive than the provisions of the MFA.

The strength of the book lies in its careful empirical analysis of the interests and negotiating positions of the major actors. Aggarwal also provides a rich portrayal of the interrelationships among the major actors: the U.S., the European Economic Community, Japan, newly industrialized countries, and the less competitive industrializing countries. Lost, however, in this empirical discussion is the theoretical framework. The shift from liberal protectionism to protectionism is explained primarily in terms of the decline in U.S. hegemony as a consumer of imported textile and apparel products.

The real test of this theory is whether it can predict the creation of regimes in other industrial sectors to restrict international trade. To date, we see none. The international trading system is under strong attack, but it has not yet crumbled. A sharper focus on meta-regimes and the interests supporting this consensus may help explain the prospects for future liberal or protectionist collaboration on international trade.

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World Politics and International Law. By Francis A. Boyle. (Durham: Duke University Press, 1985. Pp. xii + 366. \$32.50, cloth; \$14.75, paper.)

Professor Boyle has successfully produced an erudite analysis that develops "a philosophy of international relations on the basis of international law and organizations that is designed to preserve and extend world peace, while at the same time to protect the vital national security interests of states" (p. 57). Throughout his study Boyle posits a fundamental theoretical and empirical conflict between "international legal positivism" and "international political science." The former he finds too often irrelevant to what should and has to be done, while he believes the latter to be not only immoral in its stress on Machiavelian power politics, but often disastrously in error.

His solution is what he calls a "functionalist analysis." By this he means a set of theoretical propositions based upon empirical analysis of the various roles or functions played by international law and organizations in times of crisis. He identifies five of these roles, along with their attendant, theoretical propositions, as (1) definition, (2) decision, (3) adjudication, (4) resolution, and (5) redefinition (pp. 157-59). "A functionalist analysis," Boyle explains, "examines the dynamic interaction between law and politics throughout the course of the crisis, its successes and failures at each stage, and, more importantly, the reasons responsible for the entire process" (p. 156).

To develop his thesis, Boyle divides his analysis into three major parts and a very short, rather unnecessary conclusion. In the first part he discusses the schism between international legal studies and international political science in some detail. Despite his criticisms of "legal positivism," he clearly comes down on its side here when he states that "around the turn of the twentieth century, the mainstream attitude of American international lawyers... was acutely realistic and relatively sophisticated in its comprehensive of the dynamic interrelationship between power and law in international affairs" (p. 22). Interesting, Boyle also points out that although Hans J. Morgenthau is clearly the father of modern international political science—which at various times in this study is scathingly equated with Machia-

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vellianism and power politics—towards the end of his life he abandoned this school for a renewed stress on international law and organization.

In part 2 Boyle uses the 1976 Entebbe raid by Israeli commandos to develop the "functional" theoretical propositions mentioned above. Among numerous other useful findings, he argues well that the U.N. Security Council debates on the Entebbe raid led to "a general consensus...that the taking of hostages violates a fundamental principle of international law and international politics" (p. 136), and generated the will to draft a convention establishing this rule. Since the confines of this review prevent further analysis, let it suffice to say that Boyle admirably demonstrates "why and how considerations of the international law and organizations are of critical relevance to real world crisis management decision makers" (p. 74).

In part 3 Boyle uses the Iranian hostages crisis and a number of other crises faced by the Carter and Reagan administrations to offer some very strong criticisms of the policies that were pursued and his own prescriptions for what should have been done. Although his analysis here is both lucid and at times penetratingly accurate, it is, in my opinion, the least satisfactory part of this generally admirable book.

Boyle's problem here is twofold. In some places his facts and/or conclusions are simply incorrect, and at times his criticisms of the Carter and Reagan administrations tend toward abrasive, *ad hominem* name-calling. It is simply incorrect for Boyle to describe Carter as a president who "willfully provokes the [hostages] crisis in the first place" (p. 184), or contend that "a fairly compelling argument could be made that the seizure and detention of the American diplomats was a legitimate exercise of Iran's right of individual self-defense under article 51 of the U.N. Charter" (p. 189). In addition, I find it incongruous to write about "the Reagan administration's illicit intervention into El Salvador's civil war" (pp. 284-85), but then declare that "Cuban troops are in Angola at the lawful request of the legitimate government of Angola" (p. 288).

With regard to name-calling, Boyle refers to the Carter administration's policy on the MX missile as "absurb" and "harebrained" (p. 210), and its top lawyers in the Iranian hostages case

as "either grossly incompetent or else completely corrupted by the expectation of significant economic gain" (p. 221). Kissinger, Brzezinski, Haig, and Kirkpatrick are called "practitioners of Machiavellian power politics" (p. 295), which elsewhere Boyle informs us "amounts to nothing more sophisticated than a flimsy philosophical justification and transparent rationalization for pursuing a foreign policy that basically consists of waging a permanent and universal war of aggression" (p. 173).

On a more general, substantive level, Boyle's entire analysis also suffers from his understandable aversion to the employment of force and war in international politics. In arguing as he does, however, he manifests a lack of appreciation for the fact—documented admirably by Adda B. Bozeman elsewhere—that force and war remain viable attributes of cultural norms throughout most of the world. In addition, it should be noted that on a general, procedural level Boyle's use of a total of 955 footnotes in his study is, while in part commendable scholarship, also somewhat tediously pedantic.

Despite these criticisms, it must be reiterated in closing that Boyle has produced a rich and provocative study that should prove to be a very important addition to the literature.

MICHAEL M. GUNTER

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Superpower Games: Applying Game Theory to Superpower Conflict. By Steven J. Brams. (New Haven: Yale University Press, 1985. Pp. xvi + 176. \$6.95, paper.)

In *Superpower Games*, Steven Brams examines some of the critical strategic problems facing the two superpowers: the credibility problem inherent in the deterrence strategy, the inability of the superpowers to cooperate and control the arms race, and verification problems associated with arms control. As the title suggests, Brams approaches these problems from a game theory perspective. Those who have tired of the standard two-person noncooperative game approach to modeling behavior in international politics will be refreshed by the innovative adaptations and

new formal concepts Brams introduces. These provide more descriptively accurate and more theoretically meaningful game theory models of superpower conflicts. The analysis is theoretical and rigorous, which does present some problems for readers unsophisticated in mathematics although a helpful glossary of terms is provided. Brams's emphasis on the insights drawn from the models and prescriptive implications for reducing conflict, rather than on the details of the game theory models themselves, should be appealing to a broad readership.

Brams's game theory analysis of three types of strategic games among superpowers reflects the analytic and prescriptive strength of this approach. The innovative formulations capture the underlying logic of the conflict, allow the derivation of the best or most appropriate strategies available to the players, and provide a rigorous way to analyze the possible strategies that the participants can employ to revolve or minimize the conflict. For example, a major problem with the strategy of deterrence is credibility. Brams tackles this problem first by persuasively arguing that it should be cast as a Prisoners' Dilemma rather than a Chicken game. By introducing the notion of *probabilistic threats*—responding to a major provocation by the opponent by cooperating with a probability P , and not cooperating with a probability, $(1 - P)$ —and examining the game in extensive form, he shows that probabilistic strategies are optimal in an expected payoff sense, and more credible and effective than the standard mutual assured destruction strategy.

Brams also formulates the superpower arms race in a Prisoners' Dilemma format. By introducing *detection probabilities*—the likelihood that each participant can detect the strategy of his opponent correctly—and a sequential aspect to the game with the notion of a leader and a follower, Brams derives the conditions under which it can be rational (expected payoff-maximizing) for the players to choose conditional cooperation and avoid the non-cooperative arming equilibrium.

Brams casts the arms control verification problem in the form of the truth game, a two-person, nonconstant-sum game of imperfect information played between a signaler and a detector, where the signaler must choose between telling the truth and lying, and the

detector must decide whether to believe him. Brams derives some interesting optimal strategies for both players. He shows that optimal strategies are mixed for both players: the signaler should mix telling the truth and lying, and the detector, rather than always believing his detection equipment and responding in a tit-for-tat fashion, should mix believing and not believing, even if his detection equipment is highly reliable.

Based on the assumption that "game theory captures a substantial part of the logical basis and empirical reality of superpower conflict" (p. 145), Brams attempts to show how the various game theory formulations can be used to describe and explain superpower conflict. Readers will find themselves questioning whether the specific formulation makes sense or is more appropriate than other possible formulations for particular substantive examples. It is always easy to criticize whether the particular formulation of a problem—the specific game, the payoff structure, the alternatives available to the players, the assumptions about the player's knowledge of the payoffs, etc.—is substantively meaningful or "correct." I leave such criticisms, which are in part a matter of taste, to others.

Two other problems are more bothersome. First, Brams develops a model of deterrence in chapter 1, but then proposes and uses two entirely different models when he examines two case studies of deterrence, the 1962 Cuban missile crisis and the 1973 crisis brought on by the Yom Kippur War. There is little effort made to justify one model over another, or to compare each of the three models across the two substantive cases. This is unfortunate, since it makes the various game formulations and the conclusions derived appear quite arbitrary. Second, deterrence, the arms race, and verification issues in arms control are all substantively connected problems. Yet, each is analyzed with distinctly different models, and it is unfortunate that neither the theoretical or substantive implications derived from each model are linked. For example, how one plays or should play the arms race game will both affect and be affected by how one plays the verification game. The lack of linkage across models and strategic problems creates a choppy and somewhat disjointed overall presentation.

Nonetheless, readers will find that Steven

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Brams has presented an innovative set of game theory formulations of three important strategic problems facing the superpowers. The models provide an extremely useful template to assess the logical basis of superpower conflict. The substantive conclusions drawn from the analyses regarding how the superpowers should act in the various forms of conflict and how they might reduce or resolve conflict are often counterintuitive and certainly controversial, and should spur others to refine or create alternative models and explanations of superpower conflict.

STEPHEN J. MAJESKI

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Deterrence in the 1980s: Crisis and Dilemma.

Edited by R. B. Byers. (New York: St. Martin's Press, 1985. Pp. 235. \$29.95.)

Nuclear Deterrence—Does it Deter?

By Honore (Marc) Catudal. Foreword by Martin J. Hillenbrand. (West Berlin: Berlin Verlag Arno Spitz, 1985. Pp. 528. No price.)

Each of these books examines contemporary notions of deterrence with skill and refinement. The collection edited by R. B. Byers probes these notions as they concern nuclear and conventional weapons technologies, while Honore Catudal's volume is limited intentionally to the nature and evolution of nuclear deterrence. Both books are commendable and worthwhile additions to the proliferating genre of strategic studies. The only problem is the genre itself: it explores the moves, mechanics, and manifestations of global insecurity, but ignores the origins of prevailing competition and conflict.

This is not a trivial problem. Whatever we might do to improve deterrence, it can never amount to more than fine-tuning our preparations for Armageddon. To survive into the future, all nations will have to embrace an altogether different pattern of thinking. Spurred on by recurrent visions of a vast chaos from which there could be neither escape nor sanctuary, they will have to reject the banal syntax of Realpolitik and its associated forms of "deadly logic." In the final analysis, such rejection will require prior transformations of

self. Before it becomes possible to preserve our planet, individuals everywhere will have to look our approaching disappearance in the face.

In all likelihood, however, we will continue to cultivate thoughtlessness (in the sense offered by Hannah Arendt), preferring the illusion of collective immortality to the prospect of perishing. Desperate to demonstrate our "power" as nations because of our impotence as persons, we will continue to surrender to the most vacant intuitions of our leaders. Not surprisingly (in light of that degrading recitation of failures we call history) we will remain in stupor, content to tinker with the very weapons and policies that assure oblivion. Throughout this process of disintegration, cadres of accomplished political scientists will side with hallucination, assiduously suppressing "idealism" wherever it might appear.

In the opening chapter of his anthology, "Deterrence Under Attack: Crisis and Dilemma," R. B. Byers presents the standard "realist" position: "Given the cruel dilemmas posed by nuclear weapons, deterrence is the only viable security option for the future—what remains in doubt is what form it should take and what military capabilities are required to support it (p. 10). This is merely argument by assertion. Even if deterrence were the only security option for the future, its "viability" would surely not be self-evident. Indeed, because nuclear war would be a unique event, any statement ascribing viability to nuclear deterrence is necessarily fallacious. At the same time, this chapter's particular assessments of current nuclear deterrence, especially the discussion of NATO Euromissile deployments, are lucid and helpful.

Janice Gross Stein's chapter, "Deterrence in the 1980s: A Political and Contextual Analysis," examines the "apolitical and acontextual" attributes of deterrence, and thus contributes to a better understanding of the concept. Recognizing the dangers of interpreting deterrence exclusively in military terms, her essay points importantly to new political visions for security.

George Quester's chapter, "The Strategy of Deterrence: Is the Concept Credible?" offers a clear and insightful look at assumptions and trends, but its concluding observation that we may decide to stay with "deterrence by punishment" is less than productive.

The remaining chapters cover the entire range of deterrence issues and will make useful reading for specialists and students alike. In view of current popular concern over Star Wars, the chapter by George Rathjens on "Space Based Defense" will be very timely.

The issues raised in this book suggest it would surely be a mistake of enormous proportion to confine our attention to problems of deterrence. Yet evaluated within its own narrow sphere of strategic studies, *Deterrence in the 1980s*—is both comprehensive and generally well reasoned.

Honore Catudal's book, *Nuclear Deterrence—Does It Deter?*, displays an impressive range of political and historical erudition, and will prove informative to students taking courses on contemporary international relations and American foreign policy. Seeking answers to questions about the basic assumptions of nuclear deterrence and the prospects for rationality in a nuclear world, the author concludes that "there is a growing body of evidence that suggests that nuclear deterrence does not deter—at least not as U.S. policy-makers think it does" (p. 485). Although this conclusion is hardly original—indeed, it appears in parts of the volume edited by Byers—it hints strongly at the need to go beyond deterrence to some other more enduring pattern of safety. What we need now are not more and more indictment of deterrence, but rather some carefully articulated attempts to define such a pattern.

LOUIS RENE BERES

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Black Box: KAL 007 and the Superpowers: By Alexander Dallin. (Berkeley: University of California Press, 1985. Pp. xii + 130. \$14.95.)

Before dawn on September 1, 1983, a Soviet Air Defense Command pilot shot down a Korean Air Lines 747 commercial jet bound for Seoul on a flight from New York. All 269 people on board perished. In its flight across the Pacific, KAL flight 007 deviated significantly

from the well-established route. It crossed in and out of Soviet territory for nearly two hours.

Alexander Dallin carefully sifts through the limited available evidence and competing claims to address two questions: Why did the Korean jet liner depart from the established route? and Why did the Soviets shoot it down? As Dallin observes, "there is no smoking gun that would clinch the argument" (p. 55), but in addressing the first question he succeeds in differentiating the plausibility of various explanations. He makes a strong case for dismissing mechanical failures, crew incapacitation, or hijacking. The case for willful, nonpolitical action by the crew (e.g., to save fuel or beat another KAL flight to Seoul) seems by the author's analysis to be scarcely more plausible. Dallin finds somewhat more possible an explanation based on innocent—but extremely careless—errors by the crew in setting the navigation equipment. Subsequently simulations have reproduced the plane's flight path by inputting the wrong coordinates into one of the three inertial navigation systems and failing to cross check the setting with the other two, and by neglecting to make any inflight radar checks.

The other explanations that Dallin finds difficult to dismiss assume the aircraft participated in an intelligence mission. One version involves "tickling the PVO" (forcing the Soviet air defenses to high status alert for purposes of observation); another concerns aiding with the monitoring of a new Soviet ICBM test scheduled for that night. Dallin reviews the objections raised to such intelligence missions and the possible responses to those counter arguments. He reminds us here, as elsewhere, that logic may be a poor guide for reconstructing human motives, and that the requirement of good judgement may not be an adequate guide for actual decision making.

For each hypothesis, arguments for and against a possible explanation are thoughtfully examined. The serious reader will find it necessary to refer continuously to the extensive endnotes, some of which would have seemed to warrant treatment in the text (e.g., discussion of KAL's apparent change in course just before reaching Sakhalin Island). Occasionally, the author dismisses matters abruptly without reproducing his reasoning (e.g., that the Soviets may have led the aircraft off course, or

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that Marshall Ogarkov's dismissal as Chief of the General Staff had any connection with the incident). To Dallin they may have appeared obvious or minor points, but their neglect grates against the book's usual thoroughness.

No rival hypotheses are examined in addressing the possible reasons for the Soviet destruction of the Korean airliner. Instead, Dallin enumerates the variety of factors that disposed the Soviet military authorities to shoot it down. His survey of contributing factors is extensive—from the ambiguities in international law to Soviet perceptions of the pervasiveness of American intelligence; from Soviet military vulnerability in the Far East to Soviet political culture (e.g., suspicion of outsiders, fear of ridicule) and bureaucratic style (e.g., refusal to give others the benefit of a doubt, strict adherence to the rules). At the outset of this section we are warned that "in retrospect the course of history all too often seems inevitable....we must guard against making the case so overdetermined that in our analysis we deprive the actors of any reasonable choice" (p. 70). Yet the presentation gives exactly that impression. All the Soviet participants followed "a rigid mentality of 'going by the book'—following the rules in a defensive, almost mindless fashion" (p. 84). As described, the Soviet actors' choices seemed predetermined, if not overdetermined. How adequate is such an interpretative approach to Soviet decision (or nondecision) making? More importantly, if this military action seemed reflexive and automatic, are there other conditions that might have resulted in real acts of choice? The author leaves himself open to the charge that he has not adequately helped the reader locate this episode in a somewhat bounded set of Soviet military behavior.

Be that as it may, Dallin offers much to ponder. Consider his disturbing main conclusions. The weight of the available evidence makes an intelligence mission by the airliner on behalf of one of the superpowers one of the explanations most difficult to dismiss. The other superpower destroyed the aircraft in almost robot fashion without knowing or caring whether it was a commercial flight. What report could be more alarming at a time when the crisis stability of both sides' strategic deterrents, may be declining?

As an unsolved detective story or as a grim revelation of current behaviors of the super-

powers in unexpected, urgent situations, Dallin's essay provides fascinating reading.

CHARLES F. HERMANN

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The Soviet Union and Cuba: Interests and Influence. By W. Raymond Duncan. (New York: Praeger Publishers, 1985. Pp. xv + 220. \$12.95, paper.)

Because the Cuban Revolution has always had a unique ability to inspire or infuriate, books about it have been prone to polemics as authors have promoted their preconceived partisan views. The result has too often been simplistic treatments of complex phenomena, an excellent example being the *Surrogate Thesis*, which reduces Havana's international role to that of a pliant spear carrier for Soviet expansionism. Fortunately, W. Raymond Duncan avoids such pitfalls as he examines the subtle nuances of the Cuban/Soviet relationship, stressing the interface (both complementary and contradictory) between their foreign policy interests, especially each country's ability to influence the other's international behavior. Consequently, he has produced one of the best contemporary macrosurveys of Cuban foreign policy. The book's analysis is thorough and balanced; its topical coverage, while not complete, is quite satisfactory; and it is extremely well written.

Duncan's work draws heavily on William LeoGrande's "convergence analysis" of Cuban/Soviet cooperation in foreign affairs and on David Ronfeldt's "superclient" concept. Incorporating these two elements into his "reciprocal influence" model, Duncan concludes that both Havana and Moscow have demonstrated an ability to exert leverage on the other's international policies, and even to draw one another into joint ventures abroad. According to this scenario, "the Soviets and Cubans derive mutual benefits from the relationship and influence-seeking is likely to be utilized in an effort to balance the benefits with the costs if and when either Cuba or the Soviet Union believes that they have become unbalanced" (p. 17). Appreciating the dynamics of this process is, says Duncan, the key to understanding much of Havana's foreign policy behavior.

Factually there is nothing particularly new in this book; its basic information is readily available elsewhere. However, in contrast to many other policy studies, which are often conceptually disorganized, Duncan's volume merits high praise for its coherence. By consistently employing reciprocal influence as his unifying analytical thread, Duncan has created a finely-honed study that is especially effective when dealing with the ongoing Cuban/Soviet controversy over armed struggle in Latin America, Havana's dramatic involvement in Africa, and the Cuban/Russian responses to increased radicalism in the Caribbean Basin. There are, however, some weaknesses that must be noted.

Duncan focuses perhaps too heavily on Cuba's attempts to use its superclient status to influence Moscow's foreign policy positions, thereby enhancing the probability that the Fidelistas will be able to count on Russian cooperation as they pursue their international interests. Admittedly Havana does this, but since Duncan himself stresses the crucial role nationalism plays in the Cuban Revolution, it is somewhat perplexing that he does not devote much attention to the equally plausible proposition that Havana's behavior is often geared toward redefining its relationship with the USSR in order to acquire more political space in which to operate independently. For example, Cuba's efforts to exercise leadership in Third World affairs, which dominated its international agenda during the 1970s, can be seen as indicative of a desire to develop new centers of political/economic support which could, by supplementing its Kremlin connection, give Havana more options and more freedom of action overseas.

Cuba's campaign in the Western Hemisphere to normalize and/or expand its relations with an ideologically wide variety of governments—which recently has involved trying to utilize the debt issue as a catalyst for greater Latin unity—is also consistent with the hypothesis that Havana is seeking to maximize its international flexibility by nurturing new networks of solidarity. Cuba does have, as Duncan heavily emphasizes, a long tradition of commitment to armed struggle and revolutionary politics in Latin America, but the normalization dimension of its activities there has been equally important. By somewhat neglecting the latter, Duncan does not convey the full complexity of Havana's hemispheric policies.

Undoubtedly the book's main shortcoming is its failure to probe deeply into the impact of the war in Afghanistan. The entire thrust of Duncan's reciprocal influence approach demands that this episode be spotlighted, for Moscow's intervention would seem to represent a classical illustration of the limits of Cuban influence on the Kremlin. Indeed, the Russians apparently did not even bother to inform Havana beforehand that they had decided unilaterally to pursue a course in Afghanistan that eventually would seriously tarnish the Fidelistas' prestige abroad and wreak havoc with their Third World leadership aspirations, which in turn may have undermined Cuba's superclient stature and thereby reduced its leverage over future Soviet policies. Afghanistan is a prime example of Moscow creating strong incentives for Havana to diversify its international relations and thus acquire a broader expanse of independent political space.

Despite a few analytical lapses, Duncan has written an outstanding volume. This is a book into which readers can sink their scholarly teeth and come away satisfied that they have been provided not only with an unusually sophisticated perspective on Havana's foreign policy and the often unique nature of the Cuban/Soviet relationship, but also a fascinating case study of superpower vulnerability to manipulation by astute smaller countries that are unwilling to accept docilely their supposedly inevitable subordinate status.

H. MICHAEL ERISMAN

Mercyhurst College

In Defence of Canada: Growing Up Allied. By James Eayrs. (Toronto: University of Toronto Press, 1980. Pp. xi + 431. \$27.50 (Canadian), cloth; \$17.50 (Canadian), paper.)

This is the fourth volume in James Eayrs's history of Canadian diplomatic and defence policies since the First World War. In this study, which covers the period from 1945 to 1955, Eayrs examines Canada's role in the creation of the North Atlantic Treaty Organization (NATO). While Canada's involvement has been largely overlooked in most histories of

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the Cold War, Canada played an influential part in laying the alliance's philosophical and organizational foundations. Eayrs also documents how Canadian foreign and defence policies were transformed by the process of alliance building.

In deciding to participate in creating the alliance, Canada made a decisive break from the policies of the past. Under Prime Minister Mackenzie King, Canada scrupulously had avoided foreign involvements, and its first instinct was cautious isolationism. The conscription crises of both world wars had brought bitter internal divisions along linguistic lines, and avoiding any recurrence was the highest domestic priority. Canada's principal association in world politics was with the Commonwealth. This link to British power was still seen by many Canadians as the country's primary international identity.

According to Eayrs, Canada's disposition changed in response to the specific circumstances of the postwar period. First, in 1948 Canada and the United States had negotiated an agreement on free trade. On further reflection, however, Prime Minister King decided that "complete reciprocity" could be a dangerous political move, and he sought a means to reverse course. The alliance was one method "to get off the hook of reciprocity" (p. 64), as King could now argue that a North Atlantic free trade zone was conceivable. Second, Canadian officials believed that the alliance could enhance Canada's bargaining position with the United States. In a multilateral setting Canada would be subject to less pressure and could, by acting with others, establish "a coalition of constraint" (p. 67) to contain what were often seen as headstrong American actions.

The third impetus towards alliance was the strong anti-Communism that developed during this period, a sentiment that was as much apparent in isolationist Quebec as in English-speaking Canada. Indeed, a French-Canadian Prime Minister, Louis St. Laurent, who took over from King in November 1948, was to be an ardent crusader in favour of new international commitments.

Canada's major contribution in the creation of the alliance was article 2 of the treaty. The Canadian belief was that adherence to democracy and Christian virtues should be promoted as a collective heritage, because an alliance based only on military interests could not en-

dure. The balance of forces would inevitably shift. While this view was not shared by the Americans and others, in principle at least Canada got its way. Article 2, which speaks of free institutions and the development of "a better understanding of the principles upon which these institutions are founded" (p. 128), became the philosophical premise on which the alliance was based at the time of its founding.

Among Eayrs's main arguments, however, is that this Canadian conception was not to prevail. Those who viewed NATO as being exclusively a military pact were to triumph. The United States, in particular, anxious to bring together the strongest combination of military forces, sought early membership for the Federal Republic of Germany and then for Portugal, Greece, and Turkey. Although Canada acquiesced, there were feelings of disquiet that NATO was not longer to consist only of North Atlantic countries that were democratic and Christian. Eayrs also stresses that in the years following the alliance's founding, Canada did little to ensure that NATO institutions would reflect something other than military concerns. The author contends that the government was immobilized by conflicting points of view, and lacked the conviction necessary to follow through in a meaningful way.

Based largely on original sources, Eayrs has written a first-rate history. For scholars interested in images of the Cold War, constraints on middle-power actions, and the fashioning of the alliance, there are rich insights and documentation. The world views of the policy makers associated with what has often been described as the "golden age of Canadian diplomacy"—Lester Pearson, Louis St. Laurent, Brooke Claxton, Escott Reid, and Hume Wrong, among others—are skillfully portrayed.

Yet despite these strengths, some may find the book disappointing. Although nominally a political scientist, Eayrs makes no attempt to analyze how decisions were made, or to use analytical tools. Bureaucratic politics, the power relations between states, and techniques of diplomacy are never focused upon as distinct concepts. Indeed, there is not even a concluding chapter to summarize the positions of the main actors or the overall pattern of events. Presumably the author will attempt to do this in another volume in this series.

For those studying Canadian foreign policy

or the founding of the alliance, the volume is of considerable value. Eayrs has, in this fourth volume, added to the weight of our knowledge.

DAVID TARAS

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Arms and the African: Military Influences on Africa's International Relations. Edited by William J. Foltz and Henry S. Bienen. (New Haven: Yale University Press, 1985. Pp. xv + 193. \$22.50.)

This is a useful, well-edited, and above all eminently sensible collection of essays on what the editors call "the militarization of Africa's international relations." Put together under the auspices of the Council on Foreign Relations, the book understandably stresses the phenomenon's consequences and implication for U.S. foreign policy. While each of the seven essays in the collection examines a different aspect of the problem, all document what is perhaps the book's most important theme: that as military factors have become increasingly important in African international politics, so have those factors "helped turn seemingly parochial African disputes into occasions for superpower competition" (p. 171), and, it should be added, intervention. The proposition is hardly news, but it is here given new emphasis and validation by the skill with which the several authors muster the evidence and argue their cases.

Louis George Sarris, who examines Soviet policy and arms activities in Africa, argues that the Soviet Union, despite the costs and burdens on Soviet resources, had made net political gains on the continent and will continue to seek opportunities to provide arms and other military assistance "so long as their actions continue to focus on guidelines generally acceptable to most sub-Saharan countries" (p. 53). Citing David Newsom in 1979, he reminds the Reagan administration that "Africa's search for international alignments is driven by premises that further Africa's own central priorities" (p. 57), and that in this respect (quoting Reagan's chief Africa advisor, Chester Crocker) they are not pro-Soviet, "pro-American, or pro-Western. They are pro-African" (p. 57).

George E. Moose reviews French military policy in Africa, finds the French to be singularly adroit in finding acceptable ways to further their privileged position in their ex-colonies, and concludes that despite Western criticisms of their actions, "to the extent that French activities are perceived to serve broader Western interests, they can be expected to encourage, and where appropriate, lend active support to French efforts" (p. 97). Walter L. Barrows focuses on Africa's changing and increasing military capabilities, and warns that Africa's international life may well become increasingly militarized, and hence more dangerous, as a consequence. Robert S. Jaster's analysis of South Africa's defense strategy is perhaps the most interesting piece in the book. He argues that the regime uses and intends increasingly to use its formidable military capability both internally to suppress resistance and externally to cow its neighbors and attack ANC and other guerrillas targeting the Republic. Thus, the Republic's leaders "will hope to postpone Armageddon." They will be under no illusion, however, that this strategy will solve the problem of trying to exist as an apartheid state in a world that condemns apartheid as morally unacceptable" (p. 152). Henry Bienen, who considers African militaries as foreign policy actors, reinforces Sarris's conclusions by finding that they do not "align themselves in any clear-cut fashion in either inter-African or global politics" (p. 170).

Finally, William Foltz, in his concluding essay, offers some useful propositions to help guide American policy making on the question of arming Africans. First, he suggests Africa, as well as the United States, is better served by economic than military assistance. Second, he argues that excessive "arming of African security forces can produce rather than reduce disruption" (p. 190). Third, increasing levels of African armaments may well threaten the security of economic installations in which the United States has an interest; and fourth, "military competition provides an easy way for the Soviet Union, which cannot compete effectively in the economic realm, to augment its political and physical presence in Africa" (p. 191). Thus, it is in American interests to limit further arms provision to Africa, to be careful about becoming militarily involved in situations that could evolve beyond their local or regional scope, to help avoid creating rigid

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splits between moderate and radical states by providing weapons in large quantities to the so-called moderates, and, by exercising restraint in arms assistance, to refuse to play the Soviet game of attempting to polarize African affairs along an East-West dimension. It all makes very good sense.

The book is worth serious consideration and study, especially by the makers of our foreign policy.

VICTOR T. LE VINE

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The Warsaw Pact: Alliance in Transition?

Edited by David Holloway and Jane M. O. Sharp. (Ithaca: Cornell University Press, 1984. Pp. 290. \$29.95.)

In late April 1985, shortly after *The Warsaw Pact: Alliance in Transition?* was published, the Warsaw Pact was quietly renewed for at least another 20 years. Most journalistic commentaries concerning that event failed to recognize the central point made by this volume; namely, that the Warsaw Pact is no longer a mere transmission belt for orders from Moscow, and that, in David Holloway's words, the pact is now a "complex mechanism for coordinating the military and foreign policy activities of its members" (p. 37). This book is important; if for no other reason than the prominence given to such an argument.

David Holloway and Jane Sharp have taken a disparate group of papers and molded them into something far more coherent than seemed possible in May 1982 at the Cornell conference for which these papers were originally written. Holloway's introduction sets the stage nicely for the nine contributions, alerting the reader to main points, agreements and disagreements. He and Sharp have organized the book around three general foci: the pact's international evolution and "intra-pact relations," the divergence of East European interests from Soviet policies, and finally, "future" prospects for the Pact.

The theme of this book is worthy of attention. We do, indeed, need to "measure the extent to which the [Warsaw Treaty Organization] WTO has developed into a genuine alli-

ance over the past three decades" (book jacket). Despite this need, and the success of Holloway and Sharp in bringing together scholars known for their study of the Warsaw Pact and Eastern Europe, the "set of rarely explored questions" to which the editors allude never emerges from these contributions. In none of these chapters does one find an innovative effort to measure any phenomenon or any behavior; in none of these chapters is there any substantial effort made to develop conceptually the idea of a "genuine alliance" into which the editors (and I) think the WTO has evolved. This is not so much an indictment as it is an expression of disappointment, since the volume could have been a forum for less standard fare.

Part 2, "Intra-Bloc Relations," begins with a historical overview by Malcolm MacKintosh, reviewing the institutional development of the Warsaw Pact. Condoleezza Rice discusses "Defense Burden-Sharing," and describes many details about East Europeans as arms producers and suppliers. She does not, however, come to grips with the concept of "burden sharing" as applied to the Warsaw Pact; what is "defense burden," how do we measure it over time, and how can we gauge burden relative to capacity or world standards? Christopher Jones' chapter posits the arguments he has made elsewhere—that national formations in the Red Army during World War II guide how the Soviets now deal with East European Warsaw Pact members, and that the "primary mission... of the WTO is to deny national control over national armed forces" (p. 109). Stephen Larrabee's paper on "Soviet Crisis Management in Eastern Europe" concludes part 2 of the book. The idea of "crisis management"—which is intriguing because of all it implies about Soviet behavior within the alliance—deserves much more thorough consideration. What are the components of "crisis" in the Soviet "bloc," and how can we measure those elements? If some crises are more severe than others, as Larrabee suggests, how can we distinguish empirically between and among these cases (Hungary 1956, Czechoslovakia 1968, Poland 1980-81)?

In part 3, Edwina Moreton describes a wide range of issues in which East European and Soviet interests are divided. Jane Sharp emphasizes the East European arms control positions vis-a-vis the Soviets. The descriptive

elements in both chapters are valuable, but neither Moreton nor Sharp offers more than the most general explanatory comments. For example, Sharp notes that "East European attitudes toward CSCE and MBFR vary according to the orthodoxy of their regimes" (p. 191). This hypothesis deserves attention in her study. If one were to identify criteria of "orthodoxy," would they vary over time with "distance" from the USSR in arms control positions—or other elements of international behavior?

J. F. Brown, Paul Marer, and Jonathan Dean, all seasoned observers of Eastern Europe, contribute three chapters in part 4, "Future Prospects." While all three are wary of their predictive task, these analysts do not offer the reader the insights one might have hoped for. Brown, for example, identifies three approaches the Soviets might take regarding Eastern Europe. These are, however, vaguely identified as decolonization, repression, and "muddling through" (the latter, not surprisingly, being the most likely prospect). Paul Marer, writing about "Intrabloc Economic Relations and Prospects," argues that "increased instability in the domestic economies of [Eastern Europe] and in Soviet-East European relations" is likely, and that "economic prospects . . . are not conducive to the Warsaw Pact moving toward greater alliance cohesion" (p. 234). Marer makes it clear that the Soviet Union, by reducing its implicit subsidization of East European economies, will have less freedom to "enforce alliance cohesion." Unfortunately, "alliance cohesion" remains a vague concept, not considered by Marer because it was not his task to do so, but not operationalized elsewhere in the volume either. Jonathan Dean concludes with a final chapter in which he offers a highly speculative assessment of future Warsaw Pact developments. In Dean's view, communist parties will remain dominant, but "individual aspects of more pluralistic politics will be built into East European political systems" (p. 261).

Throughout these contributions, one is struck by the questions raised but left unanswered. Certainly, the Warsaw Pact is an alliance in transition, but what are the principal components of its transformation? How has the WTO's cohesion changed over time? Has the reliability of its forces been enhanced or reduced? Has the distribution of defense

burden changed across countries and over time? And, most importantly, what explains these changes? Although *The Warsaw Pact: Alliance in Transition?* raises such issues in passing, the reader finds no head-on confrontation with conceptual and measurement issues inherent to the study of alliances. The "unexplored questions" that one is told this book considers are never brought into clear focus.

DANIEL NELSON

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Beyond China's Independent Foreign Policy: Challenge for the U.S. and Its Asian Allies. Edited by James C. Hsiung. (New York: Praeger Publishers, Special Studies, 1985. Pp. vii + 215. \$32.95.)

The major catch-phrase of the early days of Sino-U.S. normalization was the "China card"—the presumption that a "united front" between the U.S. and the People's Republic of China would strengthen the U.S. position against the USSR. The initial relation did result in some mutual consultation, and the significant reduction of barriers between the two countries. From the Soviet perspective, the international situation was becoming more critical, especially with the anti-hegemony treaty between strongly pro-U.S. Japan and anti-Soviet China. Following the crises in Afghanistan, Cambodia, and Vietnam in the late 70s, the various international actors sorted out the available options. Even if the Gorbachev leadership does not embark on major changes in foreign policy, the funerals of Brezhnev, Andropov, and Chernenko provided opportunities of quasi-summit diplomacy.

Professor Hsiung's volume addresses not only the Sino-Soviet relationship and its continuing evolution, but Beijing's interests and behavior in Asia. He sees China's use of the term "independent" foreign policy to mean Chinese aims to improve relations with the USSR and continue good relations with the U.S., but not to establish an alliance; and to seek common interests with the Third World. The contributors examine the new independent policy of China and its operation and implications for the USSR, the U.S., Japan, Southeast

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Asia, and Taiwan. The volume omits the south Asian subcontinent and the Korean peninsula.

The central thrust of the study is the strategic triangle of the U.S., the People's Republic, and the USSR, and its dynamics. Peter Berton notes that Beijing's "breakout from the Soviet-Communist alliance system has given it the opportunity to play an independent role in the global strategic balance of power, a feat that neither Western Europe nor Japan has been willing or able to perform" (p. 7). He sees a bipolar relationship with tripolar tendencies, which may be complicated by a more independent role of Japan in the future.

In another chapter, Berton studies Sino-Soviet relations and sees Moscow as attempting to lure Beijing away from the West with economic incentives. Soviet inflexibility on the three crucial issues of Afghanistan, border forces, and Vietnam will make full normalization difficult, according to Berton. Soviet policy is to hinder or prevent China from becoming a major power, and thus it is not likely that major breakthroughs will occur.

The editor looks at the strategic triangle from the perspective of game theory (especially the Stryker-Psathas game), measuring costs and gains, and concluding that "triadic harmony" is in the best interests of all. This condition is unlikely to materialize because of mutual distrust, the very novelty of the ideal, and the necessity that short-term interests be submerged under long-term interests. The approach reminds us that there is nothing permanent in any of the coalition patterns.

Gavin Boyd's essay examines the domestic and international linkages in Chinese foreign policy, and cautiously concludes that Soviet expansion of influence will probably produce strains in Chinese foreign policy, that frustrated party and military figures may regard renewed revolutionary ideals as imperative, and that the regime requires greater capacity to cope with issues in foreign policy. Steve Chan provides a provocative examination of U.S. perceptions, or "credos," about Sino-Soviet relations. A similar examination of Chinese perceptions would have been welcome in this volume.

James Hsiung concludes the volume with an examination of the consequences of China "playing the pivot" in the triadic relationship. A major consequence is that U.S. policy must undergo a realistic reappraisal in light of the

new independent policy. This development also means that the role of Japan and other allies will become more valuable.

The book is a significant analysis of a major development in Asian relations and their implications for American foreign policy, and most timely in examining future developments in Chinese diplomacy.

ROBERT E. BEDESKI

Carleton University

The Export of Hazard. Transnational Corporation and Environmental Control Issues. Edited by Jane H. Ives (Boston: Routledge and Kegan Paul, 1985. Pp. xviii + 229, \$19.95.)

This book is the updated product of a 1979 conference on U.S. hazard exports to the development world. It seeks "to provide the global community with substantive information... on the issues concerning occupational safety and health, environmental protection, international relations, and regulatory issues" (Ives, p. 1). It largely focuses on the first item, whose regulatory and international dimensions are discussed. Environmental protection is barely considered.

Hazard export refers to the establishment of "hazardous industrial processes in one country to serve markets in another country where those processes would not be allowed to function" (Castleman, p. 62). The 16 authors who contributed 14 papers, each ranging from 5 to 30 pages, hold a broad perspective that includes consumer protection and environmental protection. They have written descriptive and reflective pieces on the dimensions of the problem and on our moral and political responsibility for dealing with it. U.S. and international regulatory actions are examined by King, Waldo, and Ruttenberg. Ives and Castleman effectively document the extent of the hazard. Several authors raise important ethical and policy questions (e.g., Waldo, Noble), and remedies are suggested (e.g., Ashford, Janasoff). Michaels et al. contribute an interesting review of the obstacles to the implementation of Western standards in the developing world. Waldo also identifies many key issues that deserved more extensive treatment in subsequent chapters. The recommendations of-

ferred include issuing warnings of potential hazards to developing countries, training and mobilizing the affected populations, and establishing governmental standards restricting the manufacture and sale of these products.

These papers adequately fulfill the modest objective of the editor. They provide useful, if not rigorous, information on a topic that has not been extensively researched. The bibliographies will be helpful. Several authors clearly identify the political and economic dimensions of the problem.

As a treatment of the issue, however, this book has several shortcomings. There is much overlap. Several papers add very little to our understanding of the topic. More importantly, the general focus remains unclear. What are we trying to understand? Are we concerned with the exportation of hazardous substances (e.g., DDT), or with the manufacture of these substances abroad? Should we focus first on occupational safety and health, or on the protection of ecosystems? Should we restrict our concern to chemicals (as most authors seem to assume) or expand it to other products? Is consumer safety to be included? The contributors to this volume do not share a common scope, which makes their respective remarks difficult to integrate.

This book adds little to the explanation of the policy problem and lack of progress. Political constraints are identified, then ignored. After having documented the failure in defining and implementing effective warnings and regulations, the authors basically ask for more of the same. Why should the prospects for success be greater now? There is a general tendency to overlook the political origins of safety and health standards, as if American norms were absolute and universally applicable. Furthermore, LDC's "inaction" is also often the result not only of their incapacity, but also of deliberate political choices. Most authors find the multinational corporations' search for profits at the root of the problem. However, the desire to avoid costly environmental regulations contributes only a little to the decision to transfer activities abroad. The degree to which multinational corporations have contributed to the deterioration of the health of the local population compared with the effect of local industries in this regard is also unclear. One should finally recognize the positive contribution that multinational corporations can

make to environmental protection and safety in developing countries. For example, large businesses tend to be more amenable to improvement than smaller ones, which cannot afford the antipollution technology.

This book is torn between mobilizing for action and analyzing a problem. It deserves a revised and expanded version, perhaps including contributions from political scientists. In the meantime, the information it contains and the problems it identifies can be mined by researchers and activists alike.

PHILIPPE G. LE PRESTRE

Wells College

After Hegemony: Cooperation and Discord in the World Political Economy. By Robert O. Keohane. (Princeton: Princeton University Press, 1985. Pp. 290. \$30.00, cloth; \$8.95, paper.)

The "major puzzle" Robert Keohane sets out to answer in *After Hegemony* is the following: "How can cooperation take place in world politics in the absence of hegemony?" In the background of that question is the theory of hegemonic stability according to which (1) order in world politics is typically created by a single dominant power, and (2) the maintenance of that order requires continued hegemony. While disclaiming authorship of that theory, Keohane tests a version of it against post-1945 evidence on the functioning of international cooperation (in the form of regimes) in trade, money, and oil, and finds that the "decline of the American hegemony provides only part of the explanation" for the weakening of such arrangements, for even after about 1970 (when, on his count, hegemony ended), the advanced industrial countries have continued to coordinate their policies in the world political economy, "albeit imperfectly." To explain that continued cooperation, and in answer to his puzzle, he proposes a "functional theory of international regimes" that asserts that cooperation is possible even in the absence of hegemony, because regimes are superior to nonregime situations in several respects, especially for organizations pursuing bounded rationality, leaders who wish to bind their successors, and governments that empathize with

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others. He hastens to add that his is not a complete explanation, but rather one that needs to be supplemented by a theory of learning, yet to be developed (p. 132).

This is a well-argued and carefully constructed book, sophisticated in its reasoning and aware of its limits. The thought deployed on behalf of international regimes is strong, and the analogies drawn from economic literature (for instance, on market failure) illuminating. After the theoretical chapters, however, I must confess my interest began to flag. The stories of GATT and the IMF, situated though they are amidst the experience of the most explosive growth in world trade ever experienced, proved less than compelling, and the penultimate account of the new oil authority was something of an anticlimax. What, then, does the book really tell us about the larger issues it raises—cooperation, hegemony, and the world political economy?

How surprising is international cooperation, and does it require elaborate theoretical explanation? In truth, it is surprising only to those for whom world politics is an anarchy. If, however, our conception of normal (or ideal) world politics admits of stability at the global level without calling for a world state, then anarchy is hardly the right term for describing it, and cooperation in a variety of forms—regime and nonregime, governmental and nongovernmental, bilateral and multi-lateral—becomes as unsurprising as conflict. Keohane is, in any event, well aware of the "sugary-sweet" aspect of cooperation, and seeks to give it some tartness; reaching beyond realism, to institutionalism, he rejects the dichotomy between anarchy and hierarchy. However, even though he knows, too, that cooperation is hardly a transcendental value (witness the conspiracies of monopolists and of cliques of vested interests), he persists in embracing this vague and slippery term. If anarchy is in fact a deceptive concept, then cooperation does not really call for special explanation, except for special cases. These last concern innovation and learning, and the book does not really have much to say about either.

What about hegemony? Even for those using that concept, can we be so sure of its passing? (Bruce Russett entered a strong caveat in a recent article in *International Organization*.) Maybe only if we take a rather narrow view of "preponderance of material resources." If we

espouse broader views that include politico-strategic resources, the dichotomous picture proposed here—either there is hegemony or there is none—becomes much less clear. If we resort instead to a concept of global leadership, such a broad approach becomes imperative, and must be seen to include not only economic and political resources, but also capacity for community (or empathy) building, and for innovation.

Which brings us, finally, to world political economy, the subject of this book, even though the author's puzzle centers on world politics proper. The former, viewed as the interplay of politics and economics, is a currently fashionable subject, and a welcome corrective to earlier isolationist concepts of the international system. Keohane's command of the temporal (historical) dimension of that field could be firmer. The pre-1945 world hardly seems to exist at all, and as a student of leadership, or at least of hegemony, he should be able to draw freely on earlier instances of that phenomenon (e.g., one article written in 1948 is hardly an adequate basis for judging aspects of the Dutch experience). A historical-evolutionary perspective, in turn, would have taken much of the surprise out of the story of international regimes that must, in the first place, be accounted for as products of functional differentiation in the world system.

Is world political economy, in the long haul, a satisfactory basis for a theory of world politics or a conceptual foundation for political science? On the evidence of this book, it cannot really cope with politico-strategic questions—which is what hegemony and leadership are just as much about—and it leaves out altogether a variety of other systems, such as solidarity, or culture, whose interplay must also be accounted for. What we need, in other words, is an ordered analysis that goes beyond "political economy" toward a truly social-science approach to the world system.

What, then, of Keohane's puzzle? I would rephrase it as follows: in a world of declining leadership, cooperation per se is not necessarily in danger. However, innovation to respond to new global problems—in part, for instance, through the creation of new international regimes to cope with the threat of another global (nuclear) war—might well become an endangered species. The question then becomes, Is such innovation possible?

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Keohane could be read as reassuring us that it is.

GEORGE MODELSKI

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International Relations: A Handbook of Current Theory. Edited by Margot Light and A. J. R. Groom (Boulder: Lynne Rienner Publishers, Inc., 1985. Pp. 245. \$25.00.)

International Relations: British & American Perspectives. Edited by Steve Smith. (Oxford and New York: Basil Blackwell, 1985. Pp. xvi + 242. \$29.95.)

Each of these volumes makes a useful contribution towards understanding the current state of international relations as a discipline. Each is a collection of specifically commissioned essays, and the diverse selections form an integral whole that introduces students to most of the literature on international relations and to various types of theoretical approaches.

Light and Groom's volume is a set of bibliographic entries well selected; however, the volume does not quite serve as "a handbook of current theory." Most theoretical approaches are referred to, but one senses a need for more comprehensive analytical and evaluative presentation than is available.

Smith's volume is more satisfying in terms of analysis, but suffers somewhat from the pretense that it compares "British and American Perspectives." The authors apparently were under constraints to dichotomize along an essentially false dimension, resulting in rather repetitive construction of strawmen ready for deft destruction. As becomes abundantly clear, the authors strain to contrast two supposedly nationally-based perspectives when there are, in actuality, as many perspectives as authors. Interestingly, only one essay in the volume is authored by American scholars—William Olson's excellent piece on "The Growth of a Discipline: Reviewed," updated by Nick Onuf.

The major impact of what may really serve as a contrast in perspectives follows from the reliance upon verbal, analytical scholars all drawn from the British academy. There is a complete absence in the volume of econometric modelers and other quantitatively oriented

scholars. This results in omission of many of the scholars (mostly but not exclusively American) who perform quantitative research or even read what others write. Curiously, Morton Kaplan and James Rosenau emerge as part of the cutting edge of American scholarship, while Zinnes, Rummel, even Singer are barely acknowledged. This may be quite satisfying to those of us who prefer verbal, comprehensive theories to quantitative extrapolations remote from events; however, assaying the field of international relations and omitting such approaches, as do the editors of both volumes under review, limits the usefulness of the collections.

EDWIN FEDDER

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Food Trade and Foreign Policy: India, the Soviet Union, and the United States. By Robert L. Paarlberg. (Ithaca, NY: Cornell University Press, 1985. Pp. 265. \$29.95, cloth; \$12.95, paper.)

Paarlberg's book is lucid and informative, but I think he excludes by definition the main manifestation of "food power," his subject. For Paarlberg, food power is displayed through exports and imports, but governments are seeking food power mainly through developing food self-sufficiency.

Readers will learn much from Paarlberg about food policy making in the U.S., the U.S.S.R., and India. He treats these countries in separate chapters and then presents two case studies: U.S. food aid to India to avert famine from 1965 to 1967; and the 1980 embargo on U.S. grain to the U.S.S.R.

Paarlberg examines—or rather, seeks to refute—two presumptions about food power. The first is that national governments regularly manipulate food imports or exports in search of external advantages. These advantages may be economic, such as improving the trade balance, or political, such as rewarding allies with food aid and punishing adversaries with embargoes. The second presumption is that, when nations do seek food power, exporters

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rather than importers will more likely succeed. Paarlberg concludes (p. 213) that the three nations studied seldom altered their food trade policies in search of external gains; when they did, the exporter (the U.S.) did not gain an expected foreign policy advantage.

India, he finds, was usually ready to sacrifice foreign policy goals in order to improve its domestic food situation. The Soviet Union moved away from a Czarist/Stalinist policy of expropriating food from citizens to be used in foreign trade to a policy of buying grain—even from enemies—to upgrade Soviet diets. Trade policies for the U.S. were dictated, for the most part, by agricultural interests rather than by foreign policy makers.

When these nations did seek food power they were usually disappointed. However, Paarlberg grants that food power seemed to work when exercised quietly and for developmental ends, as when the U.S. insisted that India take larger steps to develop its own agriculture as a condition for receiving U.S. famine aid. When President Johnson then went on to demand that India support U.S. military action in Vietnam, U.S. food power backfired. So did the 1980 U.S. embargo against the Soviet Union, an effort to convert a successful economic policy into a heavy weapon of geopolitics. The lesson seems to be that food power can work for exporters, if used constructively. Raymond Hopkins and Donald Puchala, in their book *Global Food Dependence* (Columbia University Press, 1980), found that the U.S. exercised food power responsibly in a wide range of agricultural trade, aid, and developmental initiatives.

It seems to me that Paarlberg's categories exclude the major current thrust of food power: domestic agricultural development. Paarlberg sees nations as increasingly interdependent, and casts them as food exporters or importers, but instead they are determinedly achieving self-sufficiency in food. Europe, China, and India are prominent and unlikely examples. Paarlberg indeed notes that self-sufficiency in wheat was a "foremost policy objective" for India's successive governments, but he is obliged to conclude that the Indian government, in yielding to domestic demand by importing grain, had thereby given way on its self-sufficiency objective. A more realistic conclusion is that wheat was imported to maintain a stable environment in which agricultural

growth and self-sufficiency could come about.

Nations are more reluctant than ever to be dependent for food, for a number of reasons. One reason is that international grain markets are indeed undependable because of weather and other variables, including the occasional exercise of food power politics. Another reason is that food self-sufficiency has become a mark of status and national pride, especially for populous countries. In addition, increasing food production to meet domestic needs is a feasible way to save foreign exchange. Europe's Common Agricultural Program, championed by Europe's farms and denounced by U.S. agricultural economists, has apparently gained the support of Europe's governments and people despite its cost in program budgets and higher food prices.

With the emergence of prolific agricultural technologies, nations and nation groups are finding food self-sufficiency to be a rather easy goal. For them agricultural trade, though frequently large in quantity, is simply one means of adjusting to the enormous variability which weather and other factors impose upon agricultural production. Exceptions to this move toward self-sufficiency may be found in a few nations with limited agricultural resources, such as Japan, and in a larger number of less-developed countries whose governments have been unable to manage any economic development. Even for these nations solutions may be forthcoming from technology development following the so-called Green Revolution. Particularly for less-developed countries, food self-sufficiency may be a politically feasible strategy superior to pursuing food power through agricultural exports.

After nations expand their agricultures to meet their own effective demand they must also accept the task of managing their abundance. The U.S., which has long coped with the problem of abundance, has tried to regulate production through production controls and price disincentives, while expanding effective demand through measures such as food stamps and Food-for-Peace. India's system of "fair price shops" is another kind of management program described by Paarlberg. Political leaders have, then, already forged large-scale management programs apparently compatible with the interests of producers, consumers, and the nation. Still more ingenuity will be needed if nations, as they achieve the

food power goal of self-sufficiency, are not to be embarrassed by the problem of abundance.

DON F. HADWIGER

Iowa State University

Soviet Perceptions of the Developing World in the 1980s: The Ideological Basis. By Daniel S. Papp. (Lexington, MA: D.C. Heath/Lexington Books, 1985. Pp. xiii + 177. \$20.00.)

Daniel Papp's book provides an extremely thorough discussion of Soviet civilian writing on the Third World. After reviewing the development of Soviet thought on this subject through the 1970s, the book concentrates on the period from 1980 to 1984. For this study, Papp has explored a vast amount of Soviet material, set forth clearly the various aspects of Soviet thinking about the Third World, and identified debates among academicians on several issues.

If there is anything to criticize about this book, it is not what the author examined, but what he did not. It would have been interesting, for instance, if the author had as carefully examined Soviet military literature on the Third World during this period to see what sort of policy implications this had. In addition, a discussion of just what role or influence these Soviet academicians who write about the Third World have on actual Soviet foreign policy would be useful. Can their views actually influence policy, or do they merely reflect the policies and ideas of the foreign policy leadership?

The answer to this question is important in those instances when there is disagreement among scholars about various issues regarding the Third World. Often these differences over theoretical issues imply very different policy prescriptions. An examination of these debates compared with current and subsequent Soviet foreign policy actions may or may not tell us something about how influential Soviet academicians are in policy making, but it could inform us about whether the occurrence of these debates and subsequent changes in Soviet foreign policy are common, and hence, whether we can expect future academic debates to signal foreign policy changes.

Papp makes the point that events in the

Third World seem to provide a growing challenge to what Marxism-Leninism envisions as "historical progress" in the less-developed countries. Specifically, there are now a number of young Marxist regimes facing armed internal opposition that they have been unable to defeat, even after many years of fighting: for example, Afghanistan, Angola, Cambodia, Ethiopia, Mozambique, and Nicaragua. In addition, there are new revolutionary ideologies, such as Islamic fundamentalism, that directly compete with Marxism-Leninism for adherents in the Third World. Finally, Soviet foreign policy actions—such as the invasion of Afghanistan—have themselves alienated many in the Third World.

Papp mentions that all these developments may serve to shake the faith in the USSR concerning the utility of Marxism-Leninism in explaining change in the Third World. A fruitful area of research for other scholars would be what the views of different governments, intellectuals, officers, and others in the Third World regarding Marxism-Leninism and the USSR have been, and how they have changed. The success or failure of Soviet foreign policy in the Third World undoubtedly depends more on how Third World audiences regard the USSR than how Soviet academics regard the Third World.

It must be emphasized that Papp's study is an excellent one. It is a credit to his work that it gives rise to other questions that will be much more difficult for scholars to answer.

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The Other Arab-Israeli Conflict: Making America's Middle East Policy from Truman to Reagan. By Steven L. Spiegel. (Chicago: The University of Chicago Press, 1985. Pp. xv + 512. \$24.95.)

Originally, the author of this book intended to analyze the impact of interest groups and bureaucracies on the U.S.'s Middle East policies. After 200 pages, he decides that neither interest groups nor bureaucracies have much of an impact on those policies. He further concludes that, contrary to common belief,

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American policies regarding the Arab-Israeli conflict are not primarily reactions to events in the Middle East. Rather, Spiegel argues, "American foreign policy can be understood only by studying the ideas, the attitudes, and the experiences of the 'people at the top'" (p. 390). He develops this thesis through a detailed analysis of the processes of foreign policy regarding the Middle East in the administrations of Truman through Reagan.

The thesis is not convincingly developed, in my view. This is partly because of the cursory treatment of alternative approaches, such as systems theory, bureaucratic politics, and pluralist theory. These are all discussed in one brief chapter at the conclusion of the book, but the discussion does not do justice to the richness of the literature (pro and con) on those approaches. This may well be attributable to the suspicions of the publisher (shared by the author?) that the policy-oriented audience for this book would not be terribly interested in a prolonged theoretical debate.

The book's major thesis also seems contradicted by evidence presented by Spiegel himself, especially, if the reader (such as, to be honest, this reviewer) is not inclined to agree with it. The dust jacket of the book announces that Spiegel challenges the belief that U.S. policy on the Middle East primarily reacts to events in that region. However, throughout the volume one reads such statements as "Johnson's policy emerged as an improvisational reaction to events" (p. 125); "After Nixon's second election, events at home and abroad dramatically altered the formulation of foreign policy, especially toward the Middle East" (p. 219); and, finally, "Carter selected associates who would pursue a common Mideast strategy.... Events swamped that strategy" (p. 315). There are similar statements about Kissinger and Reagan. Spiegel claims that "what explains American policy after a crisis are the assumptions and attitudes held by U.S. policymakers before the external event" (p. 384). He shows quite clearly, however, that those assumptions and attitudes are largely a function of previous external events.

None of the critical comments, however, should be allowed to obscure that this book is a magnum opus as well as a tour de force. Spiegel began working on this project in 1973. In the ensuing years he interviewed over 300 people intensely involved in American foreign

policy and Middle Eastern politics, many extremely well placed to provide insights into both, such as Clark Clifford, Harold Saunders, James Schlesinger, Yitzhak Rabin, and even Ronald Reagan (before he became president). The book has over 1600 footnotes. Admittedly, these might constitute a monument to pedantry, but in this case they are an accurate indicator of the meticulous care with which Spiegel has addressed a mountain of complex information. Furthermore, he did so in a relatively dispassionate, even if not exactly disinterested, fashion. The result is that each chapter on policy making in the different administrations contains a masterful analysis of the interplay of external factors, domestic political forces, bureaucratic struggles, and the ideas and desires of presidential elites, however debatable Spiegel's assessment of the relative impact of these may be.

This reviewer would challenge several of Spiegel's assumptions and assertions. For example, if Israel is as valuable an ally, for strategic reasons, as Spiegel suggests, why is it that virtually no country in the world other than the United States takes advantage of the opportunity to establish close relations with it? Might that not have something to do with the impact of the pro-Israeli lobby in the United States, which Spiegel argues (p. 388) does not have great leverage on U.S. policy in the Mideast? It is nonetheless a tribute to Spiegel's essential fairness that a reader can disagree with his outlook and never feel that he is manipulating his data in a misleading way to buttress his own point of view. In short, this is an important book, certain to be of great interest to all students of the U.S.'s Mideast policy, the Arab-Israeli conflict, and the international politics of the Middle East.

JAMES LEE RAY

Florida State University

Making the Alliance Work: The United States and Western Europe. By Gregory F. Treverton (Ithaca, NY: Cornell University Press, 1985. \$24.95.)

Continuity of Discord: Crises and Responses in the Atlantic Community. Edited by Robert J. Jackson (New York: Praeger Publishers, 1985. Pp. 288. \$33.95.)

Both the books under review take as their backdrop assertions and assumptions that trans-Atlantic relations, and particularly NATO relations, have been in a state of crisis during much of the post-World War II period. From there the two part company. *Making the Alliance Work* challenges this view. Treverton sets out to provide a single framework for understanding the repeated ups and downs that have characterized relations between Western Europe and the United States, and emerges in a fairly sanguine mood. *Continuity of Discord* approaches the subject of its subtitle, "Crises and Responses in the Atlantic Community," differently. It is an edited collection of papers presented at the 1983 annual conference of the Committee on Atlantic Studies, with contributions by scholars from both sides of the Atlantic and several different disciplines. Thus we have many points of departure for describing the vagaries of the alliance, and for living together in relative harmony within the context of the unwieldy entity that is NATO.

In the past few years, as Jackson points out, we have been swamped by books and articles devoted to alleged crises in the Atlantic Alliance. Testimony to this, in a footnote extending well over a page, is a list of many prominent names from the European and American scholarly communities, journalism, and government. Almost all the citations are to articles, many of them published in *Foreign Affairs*. Of the five books cited, only A. E. W. De Porte's *Europe Between the Superpowers* and Earl Ravenal's *NATO's Unremarked Demise* are works by single author's. It has long been a puzzle why there have been so few good, meaty books on NATO in recent years, and why most of the articles say roughly the same things at fairly regular intervals. It was encouraging, therefore, to have Treverton's work, and refreshing to read that he intends to provide us not only with a conceptual frame-

work for understanding what has happened between the Western allies during the past three and a half decades, but also with some guidelines for the future—an ambitious undertaking.

Treverton deals with the nature of the alliance, the nuclear dilemma, defense and detente in Europe, defense beyond Europe, the impact of economics, and managing alliance politics. It is broad ranging, historically well documented, and clearly written. In each area the author asks, What has changed in the structure of the alliance? How different are the interests of the Europeans and the United States? How similar do the policies of allies need to be? The answers to these questions are not exactly new, although presumably they cannot be left unsaid. There are many differences in every sphere: changes in the political balance of power within the alliance, with American dominance diminished; changes in the nature of the security concerns and their place in the transatlantic relationship; and, finally, changes in economic concerns in both Western Europe and North America, with economic concerns now taking precedence over security concerns, and much more intermingled than in the early days of the Alliance.

Examples of past difficulties (converted into successes when viewed in retrospect) are nicely documented, but, by the very nature of the subject matter, there is much familiar material. However, Treverton is interesting in the chapter on defense beyond Europe and in his constant reminder that we must not neglect the domestic side to allied political and economic concerns. Unfortunately, his conclusions are a let-down. Although this will be an extremely useful book in graduate schools because it is clear and comprehensive, the reader does not really need to go through nearly 200 pages of history and analysis to be left with the prognosis that the alliance will last for 30 more years because it is better than the alternatives, and that the main prerequisites for longevity will be a modest amount of luck and sensible management.

Given what was said at the beginning of this review about the abundance of articles on the Atlantic Alliance and the paucity of good monographs, this reviewer was not very happy to go through yet another edited collection of papers originally designed for presentation at a conference. With all the editorial skill in

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the world and a firm hand on the contributors, such books almost always suffer from three serious deficiencies: first, with as many as 12 essays there is a disconcerting unevenness in breadth and depth of treatment; second, since all the contributors are well-known authorities in their respective fields and have published extensively, their essays run the risk of repeating what they have written or said in other contexts; third, the kind of contemporaneity that is reasonably possible in a monograph becomes exceedingly difficult to maintain in a collection of conference papers.

Bearing in mind all the differences in style, background, and discipline of the contributors, the coverage of *Continuity of Discord* is excellent. After a most interesting theoretical introduction on the concept of crisis by editor Robert J. Jackson, the book goes into the historical background to current crises and responses with informative pieces by two contributors. It follows with two sets of four essays; one under the heading "Military Crises and Response," the other on "Economic Crises

and Responses." The book concludes with a thought-provoking theoretical essay by Nils Andrén on the possible future of Europe, in which he provides six different models for the 1970's in his *Europe's Futures, Europe's Choices: Models of Western Europe in the 1970's* (New York: Columbia University Press, 1969).

At least Andrén does partly overcome one of the most serious deficiencies in the literature on the Atlantic Alliance: the great lack of imagination in prescriptions for the future. By and large, when authors do venture into that admittedly dangerous territory they opt for either a little bit more of the status quo or a little bit less. The kind of boldness and sweep evinced by Helmut Schmidt in his recently published *A Grand Strategy for the West* can probably come only from a retired statesman answerable to few critics.

GLEND A G. ROSENTHAL

Columbia University



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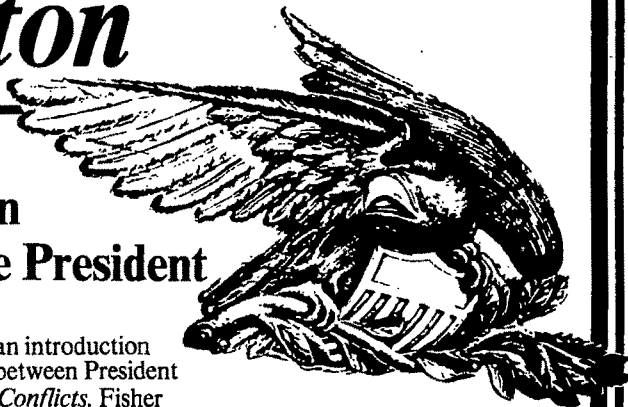
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The American Political Science Review

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- **Congress: Process and Policy** by Randall B. Ripley, W.W. Norton and Company, New York, 1983.

Human Nature in Politics: The Dialogue of Psychology with Political Science

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This article compares two theories of human rationality that have found application in political science: procedural, bounded rationality from contemporary cognitive psychology, and global, substantive rationality from economics. Using examples drawn from the recent literature of political science, it examines the relative roles played by the rationality principle and by auxiliary assumptions (e.g., assumptions about the content of actors' goals) in explaining human behavior in political contexts, and concludes that the model predictions rest primarily on the auxiliary assumptions rather than deriving from the rationality principle.

The analysis implies that the principle of rationality, unless accompanied by extensive empirical research to identify the correct auxiliary assumptions, has little power to make valid predictions about political phenomena.

This article is concerned with the nature of human reason and the implications of contemporary cognitive psychology for political science research that employs the concept of rational behavior. I shall begin with a bit of history, written from a rather personal viewpoint, to provide a setting for the discussion.

The older and/or more scholarly among you will recognize the essay's title as having been plagiarized from Graham Wallas, whose seminal book, *Human Nature in Politics*, appeared in 1908. When I began graduate study, in the middle 1930s, that book, along with Walter Lippmann's *Public Opinion*, was still wholly fresh, and both stood out as harbingers of the "behavioral revolution" that was then just getting under way at the University of Chicago.

Not that we graduate students thought of ourselves as participants in a scientific revolution. The realities of the political process had long since replaced the formal legal structure of political institutions as the main subject for study in political science—at least at the University of Chicago. Merriam's studies of power, Gosnell's quantitative methods, Lasswell's psychoanalytic probes seemed to us merely (paraphrasing Clausewitz) "the continuation of political realism by other means."¹

I was little prepared, therefore, for the violence of the polemic pro and con "behavioralism" that echoed over the land in the first two decades after World War II. Nowadays, my periodic soundings in *The American Political Science Review* reassure me that this civil strife in the profession is largely over, and that the behavioral revolution is now seen as continuity rather than discontinuity in the development of political science. I am not sure it would even qualify, in today's revisionist view, as one of Thomas Kuhn's major paradigm shifts. Perhaps what we were doing was not revolutionary science at all, but just everyday normal science.

This is probably the right moment, while I am alluding to behavioralism, to record a *culpa mea* for my part in popularizing that awkward and somewhat misleading term. It appeared, of course, in the title of *Administrative Behavior* (Simon, 1947/1976a), and also in the title of my chief epistle to the economists, "A Behavioral Model of Rational Choice," published in the *Quarterly Journal of Economics* in 1955. However, I doubt that I was the main culprit. That honor belongs to the Ford Foundation, which at that same time introduced and diligently popularized the phrase "behavioral sciences."

Whatever its origins, the term was picked up with enthusiasm—as an epithet—by the opponents of behavioralism, who frequently employed it as though it were synonymous with the Behaviorism then rampant in the discipline of psychology. In fact, there was never any substantive connection between the two labels, and much of what went on in political science, sociology, economics, and anthropology under the heading of behavioralism would have been anathematized

The present essay is a slightly revised version of the James Madison lecture presented by the author at the Annual Meeting of the American Political Science Association in Washington, D.C., 1984.

¹See David Eastman's perceptive account of this history in his article on political science in the *International Encyclopedia of the Social Sciences* (1968).

by the psychological Behaviorists if they had been aware of it—which they mainly weren't.

However, my aim here is not to reminisce about old battles. We should rejoice that political scientists are devoting all their efforts to advancing the science, and we should do nothing to encourage a renewal of the Methodenstreit. Instead, I shall offer a commentary on the role of the rationality principle in recent political science research.

I emphasize that this is a commentary and not a new piece of substantive research. The basic values for political science to which I and my contemporaries were and are committed include sound empirical data as the foundation for theory and for normative recommendations; new sources of data including polls, structured interviews, and systematic samples; the use of statistics, mathematics, and computer simulation where appropriate as tools for data analysis and theory construction; and the analysis of phenomena in terms of basic categories like power, decision making, rationality, and systems.

The research on which I shall comment exemplifies those values: it is empirically based, employing many different kinds of data-gathering methods, often uses mathematical and other formal techniques, and is sophisticated in its use of theory. My commentary will not touch on any of those aspects of the work except the last, and in particular its employment of ideas derived from the theory of human rationality.

The commentary will take us through three main topics. First, I shall have to say something about the two main forms of theories of human rationality that prevail in social science today—the one of them having its center in cognitive psychology, the other in economics. Next, I shall consider the implications, for the balance in political science between rationalism (or a priorism) and empiricism, of adopting one or the other of these two paradigms of rationality. In particular, I will argue that there is a natural alliance between empiricism and the psychological version of rationality, on the one hand, and an alliance between rationalism and the economic version of rationality, on the other. Finally, I will comment on the balance between reason and passion—"radical" irrationality—in political affairs.

The Forms of Rationality

The term "rational" denotes behavior that is appropriate to specified goals in the context of a given situation.² If the characteristics of the

choosing organism are ignored, and we consider only those constraints that arise from the external situation, then we may speak of substantive or objective rationality, that is, behavior that can be adjudged objectively to be optimally adapted to the situation.

On the other hand, if we take into account the limitations of knowledge and computing power of the choosing organism, then we may find it incapable of making objectively optimal choices. If, however, it uses methods of choice that are as effective as its decision-making and problem-solving means permit, we may speak of procedural or bounded rationality, that is, behavior that is adaptive within the constraints imposed *both* by the external situation and by the capacities of the decision maker.

The terms "procedural" and "substantive" were, of course, borrowed from constitutional law, in analogy with the concepts of procedural and substantive due process, the former judging fairness by the procedure used to reach a result, the latter by the substance of the result itself. In the same way, we can judge a person to be rational who uses a reasonable process for choosing; or, alternatively, we can judge a person to be rational who arrives at a reasonable choice.

There is a fundamental difference between substantive and procedural rationality. To deduce the substantively, or objectively, rational choice in a given situation, we need to know only the choosing organism's goals and the objective characteristics of the situation. We need to know absolutely nothing else about the organism, nor would such additional knowledge be of any use to us, for it could not affect the objectively rational behavior in any way.

To deduce the procedurally or boundedly rational choice in a situation, we must know the choosing organism's goals, the information and conceptualization it has of the situation, and its abilities to draw inferences from the information it possesses. We need know nothing about the objective situation in which the organism finds itself, except insofar as that situation influences the subjective representation.

If we review the history of political science over the past 40 years, I believe we will see that it was mainly the procedural view of rationality that was embraced by behavioralism, but that during the past two decades this view has received growing competition from the substantive view. Anthony Downs's *Economic Theory of Democracy*, published in 1957, may be used to date the first nudgings of this new camel into the tent.

I should now like to develop a little further the fundamental characteristics and theoretical structures of the two views of rationality, and then consider the implications of employing them,

²For a more extensive discussion of the concepts of substantive and procedural rationality, see Simon (1976b), reprinted as chap. 8.3 in Simon (1982).

separately or jointly, in the study of political behavior.

Procedural Rationality and Cognitive Psychology

A central theme for Graham Wallas in *Human Nature in Politics* was the interplay of the rational and nonrational components of human behavior in politics. That, of course, was also a central theme for Harold Lasswell in *Psychopathology and Politics* (1934) and *World Politics and Personal Insecurity* (1935). But while Lasswell's psychological apparatus comes largely from Freud, Wallas acknowledges as his principal mentor William James. Although Lasswell was concerned with borderline and not-so-borderline pathology, Wallas was interested in the ubiquitous workings of instinct, ignorance, and emotion in normal behavior. Wallas, like his mentor William James, is the more closely attuned to the contemporary orientation in psychology.

What is that orientation? I expressed skepticism, earlier, that political science has experienced, since World War II, any change that deserves being called a revolution. I have no such doubts about the field of psychology. Cognitive psychology, in the past 30 years, has undergone a radical restructuring, from a severe Behaviorism (no relation, I remind you, to behavioralism) to a framework that views thinking as information processing.

In psychology, Behaviorism carefully avoided speaking about what went on inside the head—it preferred to stick to the observable facts of stimuli and responses. It preferred rats to humans as subjects in its experiments, presumably because rats could not be induced to give unacceptable introspective accounts of their mental experiences. Even the term “cognitive” was eschewed, as implying an illicit mentalism.

Today, all of these barriers are down. The term “cognition” is uttered openly and proudly to refer to the human thought processes and to distinguish them from the processes of sensation and emotion. Most experiments use human subjects, and many instruct the subjects to speak aloud as they perform the experimental tasks, the tape-recorded protocols from such sessions being now regarded as wholly objective and analyzable data.³ Theories, in modern cognitive psychology, are expected to provide detailed descriptions of the information processes that go on in the human head when it is performing problem solving and other tasks in the laboratory.⁴

Within this new paradigm, cognitive psychology has made great strides toward understanding how an information processing system like the human brain solves problems, makes decisions, remembers, and learns. That understanding has advanced so far that psychology is no longer limited to dealing with “toy” tasks—puzzles and nonsense syllables—in the laboratory, but can give rather impressive accounts of adult performance in professional-level tasks: making medical diagnoses, solving physics and mathematics problems at high school and college level, learning new mathematics and chemistry, and even making new scientific discoveries, to mention just a few examples.

As examples of explicit applications of the new theories to political science, I can mention the models of public budget-making behavior constructed by Crecine (1969) and Gerwin (1969) and their students, and Carbonell's (1979) ingenious “Goldwater machine,” which predicts the response of an appropriately specified political figure to a situation or set of events. Later, I will cite a number of other accounts of procedural rationality at work in the political process, but in most of these the appeal to cognitive theory and research is only implicit.

The human capabilities for rational behavior that are described by contemporary cognitive psychology are very congenial to the paradigm of bounded rationality as that is described in *Administrative Behavior*. The models of problem solving describe a person who is limited in computational capacity, and who searches very selectively through large realms of possibilities in order to discover what alternatives of action are available, and what the consequences of each of these alternatives are. The search is incomplete, often inadequate, based on uncertain information and partial ignorance, and usually terminated with the discovery of satisfactory, not optimal, courses of action.

To understand the behavior of this kind of problem solver, who is provided in advance with a knowledge of neither alternatives nor consequences—and who may even discover what his or her goals are in the course of the problem-solving process—it is necessary to specify what the problem solver wants, knows, and can compute. Within the framework of these conditionalities, the mere assumption of rationality provides little basis for the prediction of behavior. To be of much use, that assumption must be supplemented by considerable empirical knowledge about the decision maker.

Substantive Rationality and Economics

Just as procedural, bounded rationality is most

³See Ericsson and Simon (1984).

⁴See, for example, Newell and Simon (1972), Simon (1979) and Anderson (1983).

extensively developed in modern cognitive psychology, so substantive, objective rationality finds its principal base in neoclassical economics and statistical decision theory.⁵ The two conceptions of rationality are radically different. The foundation for the theory of objective rationality is the assumption that every actor possesses a utility function that induces a consistent ordering among all alternative choices that the actor faces, and, indeed, that he or she always chooses the alternative with the highest utility.

If the choice situation involves uncertainties, the theory further assumes that the actor will choose the alternative for which the expected utility is the highest. By expected utility of an alternative is meant the average of the utilities of the different possible outcomes, each weighted by the probability that the outcome will ensue if the alternative in question is chosen.

The theory of objective rationality assumes nothing about the actor's goals. The utility function can take any form that defines a consistent ordering of preferences. Nor does the theory postulate anything about the way in which the actor makes probability estimates of uncertain events; in fact one version of the theory, the so-called subjective expected utility, or SEU, theory, explicitly denies that these probabilities are to be identified with objective probabilities of the events, determined by some outside observer. In this one respect, the label "objective" for this version of the theory must be qualified.

In principle (i.e., in a wholly idealized laboratory setting), it should be possible to obtain independent evidence about the nature and shape of any particular person's utility function, as well as evidence of the probabilities that person assigns to events. In practice, this is completely infeasible. In fact, when such experiments have been run, it has generally been found that human subjects do not possess consistent utility functions or probability assignments.⁶

In application, therefore, auxiliary assumptions about utility and expectations must usually be supplied before the theory of objective rationality can be applied to real situations. In economic applications, for example, it is customary to identify the utility function of a firm with its profit, and to assume that actors generally are trying to maximize economic well-being—perhaps some weighted average of income and leisure. In applications to political science, it may be assumed that the goal is to maximize power, or to maximize economic well-being as a function of the policies

pursued by the government. (I will have more to say later about the assumptions that are made regarding political "utility" in applying the principle of rationality to problems in political science.)

In the same way, in applying the theory of objective rationality to real-world behavior, either uncertainty must be ignored, or auxiliary postulates must be provided to define the expectation-forming process. In contemporary economics, for example, the very lively "rational expectations" school, whose leaders include such figures as Robert Lucas and Thomas Sargent, assumes that each economic actor has a more or less accurate model of the economic system, and expects that system to proceed toward its equilibrium in the near future. Of course, there is much doubt whether this particular assumption about the formation of expectations bears any close resemblance to the reality, and a majority of neoclassical economists have different, and simpler, beliefs about how economic actors cope with uncertainty.

When neoclassical economics in its purest form addresses itself exclusively to questions of the existence, stability, and Pareto optimality of equilibrium, it can generally get along without introducing auxiliary assumptions about the utility function or the nature of the expectation-forming processes. In fact, it usually finesses the latter by ignoring uncertainty. The price that is paid is that the conclusions reached by this kind of analysis are extremely general and abstract: roughly, that under conditions of perfect competition, the economic system has a stable equilibrium, and that this equilibrium is, indeed, Pareto optimal (not everyone can simultaneously be made better off than the equilibrium).

When economists want to draw conclusions about nonequilibrium phenomena, matters get stickier. The theory of business cycles provides an important illustration of the difficulties.⁷ The economic theory of Keynes and that of neoclassical economists like Friedman or Lucas are only inches, not miles, apart. Most of Keynes's general theory can be (and has been) interpreted as an exercise in quite orthodox neoclassical reasoning—except at one or two critical points, the most important being the supply of labor. At these points economic actors depart from objective rationality and suffer from persistent illusions or confusions. The assumption in Keynes's theory that produces a business cycle and the possibility of long-continuing unemployment is that labor mistakes its money wage for its real (purchasing power) wage. It is not human rationality, but the

⁵A classical treatment is Savage (1954).

⁶For a number of examples and references to the literature, see Kahneman, Slovic, and Tversky (1982).

⁷This account is based on Simon (1984).

limits on that rationality and its breakdown, that accounts for Keynes's important predictions.

But the same thing can be said of the other, non-Keynesian, theories of the business cycle. (I must except Milton Friedman (1968), who essentially denies that there is such a phenomenon as real unemployment.) For example, Lucas (1981), among the most orthodox of neoclassical economists, attributes the business cycle to a different limit on human rationality. In his theory, it is not labor but businessmen who behave irrationally. When general price changes occur (e.g., inflation), they mistake these changes for *relative* changes affecting only prices in their own industry. It is this departure from objective rationality that produces the cycle in Lucas's model.

I have developed this example at some length because it is perhaps the most dramatic illustration of a widespread phenomenon that is not well understood outside the profession of economics, and perhaps not even within the profession: A large part of the "action" of economic models—the strong conclusions they support—does not derive from the assumptions of objective rationality at all, but depends on auxiliary assumptions that are introduced to provide limits to that rationality, assumptions about the process of decision.

This being the case, one would suppose that a great deal of attention would be devoted to the empirical validity or plausibility of the auxiliary assumptions—in the examples just cited, the assumptions that labor or business, as the case may be, suffers from a money illusion. However, this is not the way the practices and traditions of economics have developed. Instead, there is a tradition that is often referred to, within economics itself, as "casual empiricism." Assumptions about the shape of the utility function or the limits on the rationality of economic actors are commonly made in an armchair, on the basis of feelings of "plausibility" or "reasonableness," and without systematic support from empirical evidence. The assumptions are never tested directly, but only in the context of the models in which they are embedded. The goodness of fit of a model, usually to aggregate data, is regarded as the best justification for the assumptions embedded in that model, whatever their source.⁸

⁸Friedman's well-known methodological essay transforms these methodological practices into a strongly defended doctrine. Friedman argues that direct tests of the behavioral assumptions underlying an economic model are superfluous at best, and positively misleading at the worst.

Bounded Rationality Is Not Irrationality

Skepticism about substituting a priori postulates about rationality for factual knowledge of human behavior should not be mistaken for a claim that people are generally "irrational." On the contrary, I think there is plenty of evidence that people are generally quite rational; that is to say, they usually have reasons for what they do. Even in madness, there is almost always method, as Freud was at great pains to point out. And putting madness aside for a moment, almost all human behavior consists of sequences of goal-oriented actions.

When, in spite of the evidence for this goal-oriented character of human behavior, we call some of that behavior "irrational," we may mean any one of several things. We may deem behavior irrational because, although it serves some particular impulse, it is inconsistent with other goals that seem to us more important. We may deem it irrational because the actor is proceeding on incorrect facts or ignoring whole areas of relevant fact. We may deem it irrational because the actor has not drawn the correct conclusions from the facts. We may deem it irrational because the actor has failed to consider important alternative courses of action. If the action involves the future, as most action does, we may deem it irrational because we don't think the actor uses the best methods for forming expectations or for adapting to uncertainty. All of these forms of "irrationality" play important roles in the lives of every one of us, but I think it is misleading to call them "irrationality." They are better viewed as forms of bounded rationality.

To understand and predict human behavior, we have to deal with the realities of human rationality, that is, with bounded rationality. There is nothing obvious about these boundaries; there is no way to predict, a priori, just where they lie.

The Rationality Principle in Politics

After this long excursion into the views of human rationality that are commonly held in psychology and economics, let me come back now to the subject of political science. What kind of rationality does *Homo politicus* exhibit? Is he or she a creature of objective, substantive rationality; or instead, one of subjective, procedural rationality? But I am afraid that I have already tipped my hand and made it quite clear that I believe the latter to be the case.

If that is true, the rationality principle, as it is incorporated in theories of substantive rationality, will provide us with only limited help in understanding political phenomena. Before we apply

the methods of economic reasoning to political behavior, we must characterize the political situation, not as it appears "objectively" to the analyst, but as it appears subjectively to the actors. We can only select the appropriate model of adaptation after we undertake the requisite empirical study to determine this subjective representation both of goals and of the situation or draw upon research in cognitive psychology to tell us about the nature of that representation. A few examples drawn from the political science literature will show what is involved.

An Example: Duverger's Law

Recently, William Riker (1982) provided us with an instructive account of a descriptive generalization that usually goes by the name of Duverger's Law. In its roughest form, the law asserts that plurality election rules bring about and maintain two-party, rather than multi-party, competition. In an informative way, Riker takes us through the history of the empirical research that has been done to test, to confirm, refute, or amend, this law. He also shows that political scientists have not been content simply to assert the law, or to test it empirically; they have also sought to "explain" it. He says

From the first enunciation by Droop, the law has been implicitly embedded in a rational choice theory about the behavior of politicians and voters. This theory has been rendered more and more explicit, especially in the last two decades, so that recent empirical work consciously invokes the rational choice model. (1982, p. 766)

The so-called rational choice argument for Duverger's Law goes something like this. If a number of candidates are running for office under a plurality election rule, and if candidates A and B are well ahead of the pack so that it is unreasonable to suppose that any other candidate will win, then it is rational to limit your vote to your preference between A and B. The argument has to be elaborated somewhat to account for two-party configurations that are stable over time, but I think that I have conveyed the general idea.

What assumptions does this argument make about you, the voter. First, it assumes that you have a preference ranking among candidates and wish to vote so as to secure the election of a candidate who is as high as possible on your ranking. Second, it assumes that you believe that one vote may decide the election (otherwise it is indifferent, in terms of the stated goal to whom the vote goes). Third, it assumes that you have an assessment of the relative prospects of the candidates, and a

considerable confidence in that assessment (e.g., you do *not* believe that one more vote could bring success to any but one of the two candidates judged to have the most support). Fourth, it assumes that you do not attach a large value to providing public evidence that your most preferred candidate has extensive, even if not pluralistic, public support.

Since I have not tried to construct a formal axiomization of this choice, perhaps there are other assumptions that must be made, in addition to those listed above. For the purposes of the present argument, however, my inventory of assumptions will suffice. What the assumptions show is that only a small part of the work of explaining Duverger's Law is being done by the rationality principle. Most of the work is being done by propositions that characterize the utility function of the voter and his or her beliefs, expectations, and calculations—that is to say, the limits of rationality. These propositions are subject to empirical test.

Perhaps the key assumption here is the postulate of "sophisticated voting," that a rational voter believes "his vote should be expended as part of a selection process, not as an expression of preference" (Downs, 1957, p. 48). But this postulate is wholly independent of the usual definition of objective rationality. There is no irrationality in a utility function that regards a vote as an expression of preference rather than an attempt to influence the selection. In fact, it is realistic to believe that one can express a preference (i.e., change the numerical result of the vote, if only by a unit), but seldom realistic to believe that one can affect the outcome of an election. Moreover, a voter might correctly (or incorrectly, but certainly not irrationally) believe that expression of preference for a party could increase the chances of that party's succeeding in subsequent elections.

There are many more changes we can ring on the possible beliefs of voters without impugning their (subjective) rationality. With these alternative sets of beliefs are associated different voting behaviors. It is not at all hard to build a rational model of the voter who stays home from the polls and does not vote at all. Hence, we get very little understanding or explanation of voting behavior simply from invoking the principle of utility maximization. That principle does not exempt us from the arduous task of testing all the auxiliary empirical assumptions about voters' values, beliefs, and expectations. And, as Riker shows us, when we subject an auxiliary assumption like the postulate of sophisticated voting to empirical test, we discover that the actual pattern of human response can be very complex indeed. We are then constructing and testing theories of bounded rationality, not theories of substantive rationality.

Additional Examples

It should not be thought that Duverger's Law is an isolated case and that rational choice theories derived from the assumption of utility maximization and unalloyed with auxiliary assumptions about preferences and beliefs have much more predictive and explanatory power in most other cases. Recent issues of the *American Political Science Review* provide a rich mine of examples that support our analysis of the respective roles of reason and fact. One can stumble upon such examples by opening the pages almost at random, and it appears to make little difference whether the author is a behavioralist or an economic rationalist by persuasion. (Or if there is a difference, it is that the behavioralist makes fewer explicit claims for rationality as the source of his or her conclusions than does the rational choice theorist.)

My next example is a study by Hibbs (1982) of "Economic Outcomes and Political Support for British Governments among Occupational Classes." Hibbs demonstrates that various indicators of the health of the British economy are related to voting preferences. Score one for the objective rationality principle. Presumably voters vote for the party that they think will enhance their economic well-being. But how do we get from that general proposition to a prediction of their vote? We can make the leap only if we can discover how voters *judge* which party will do the better job of managing the economy. There are many ways in which that judgment could be made, none of them, probably, having high objective validity.

Hence, the interesting and significant finding of Hibbs's study is not that people employ a rationality principle. The interesting finding, which does not follow from such a principle, is that "voters evaluate the cumulative performance of the governing party relative to the prior performance of the current opposition," weighting current performance more heavily than past performance (p. 259).

Now I don't know if Hibbs's model will hold up under further analysis or will apply equally well to other times and places. However valid or invalid the model, its powerful motor is not a theory of objectively rational choice but a very specific empirical assumption, based on notions of bounded rationality, about how voters form their beliefs regarding the connections between the economy and government. If Hibbs's model is correct, voters do this not by solving a maximization problem but by setting an aspiration level (the opposition's past performance) against which to measure the performance of the incumbents. This is what modern cognitive theory would lead us to

expect, but not what would be predicted by a theory of utility maximization.

A third example has to do with the application of rationality principles to a game resembling the prisoners' dilemma, but allowing the players the additional alternative of exiting from the situation (Orbell, Schwartz-Shea, & Simmons, 1984). In their abstract, the authors, using the usual distinction between defectors and cooperators in the prisoners' dilemma, sum up the matter very well:

We derive the prediction that the exit option will drain the community or group more of cooperators than of defectors.

But experimental data do not support this prediction; cooperators do not leave more frequently than defectors. . . . [We] present data supporting the hypothesis that cooperators often stay when their personal interest is with exiting because of the same ethical or group-regarding impulse that (presumably) led them to cooperate in the first place.

In this experiment, again, the principle of objective rationality contributes little to predicting or explaining the findings. Everything rests, instead, on the assumptions that are made about the utility functions of two classes of players, those who are prepared to cooperate with the other players and those who are prepared to betray them. What is more, to explain the behavior of the cooperators, a strong component of altruism must be introduced into their utility functions.⁹

Other research within a game-theoretical framework shares many of the characteristics of this study. The predicted outcome depends sensitively upon assumptions not derivable from the principle of objective rationality, about participants' beliefs and values. For example, in a study involving the conditions under which subjects would contribute to the provision of public goods, the authors summarize their findings thus (van de Kragt, Orbell, & Dawes, 1983, p. 112):

We present hypotheses about why designating a minimal contributing set works. . . . The essential property of the minimal contributing set . . . is *criticalness*: the contributions of the members of the minimal contributing set are each critical to obtaining the public good the members desire, and they know it. It is reasonable (albeit not a dominant strategy) to contribute because reasonable behavior can be expected from other minimal contributing set members who are in the same situation.

⁹For a discussion of the problems of reconciling altruism with rationality in systems subject to evolutionary selection, see Simon (1983, chap. 2).

What is called reasonable behavior here is clearly the behavior we might expect of a creature of bounded rationality. And its reasonableness depends on expectations about the behavior of others.

Perhaps the major contribution of game theory to political science has been to demonstrate how rare and unusual the situations are where a game has a stable equilibrium solution consistent with the principle of objectively rational choice. Under these circumstances, the task of determining how people actually do behave in situations having game-like characteristics must be turned over to empirical research: research that seeks to determine what values people actually act on, and how they form their expectations and beliefs.

My final example concerns considerations of economic advantage in voting decisions. Weatherford (1983) points out that the concept of economic voting is ambiguous. It may mean voting in response to perceptions of one's own economic well-being, or voting in response to perceptions of the health of the economy. But this distinction is itself ambiguous, for it may refer to differences in utility functions or to differences in the voter's model of reality.

You, the voter, may want to vote for the candidate who will do best for you (for example, support the "right" kinds of tax laws, impose or remove the "right" kinds of regulations), or for the candidate who will best foster the vigor of the whole economy, even if it costs you, personally, a loss of income or of a job. Put in these terms, the difference lies in the structure of your utility function.

But we can look at the matter in a different way. How do you judge the state of the economy or your well-being? You can use the immediate evidence of your personal situation—your employment or unemployment, your salary, your taxes. Or you can look at published economic indexes. And, because the question before you is not the current state of the economy, but how it is likely to be affected if one candidate or another is elected, there are still other kinds of evidence that may influence you. You may consider the candidates' past voting records or the economic predispositions of the parties to which they belong.

Differences in the kinds of evidence you respond to may have nothing to do with your utility function. Instead, they may reflect the model you have of the world, the beliefs you have formed about the meanings and predictive value of different kinds of available information, and what information has come to your attention.

All of these examples teach us the same lesson: the actors in the political drama do appear to behave in a rational manner—they have reasons for what they do, and a clever researcher can

usually obtain data that give good clues as to what those reasons are. But this is very different from claiming that we can predict the behavior of these rational actors by application of the objective rationality principle to the situations in which they find themselves. Such prediction is impossible, both because, even within the framework of the SEU theory of substantive rationality, behavior depends on the structure of the actors' utility functions, and because it depends on their representation of the world in which they live, what they attend to in that world, and what beliefs they have about its nature.

The obvious corollary is that rationalism can carry us only a little way in political analysis, even in the analysis of the behavior of boundedly rational people. The rest of the path requires continuing, painstaking empirical investigation within the framework of modern cognitive theories of human behavior.

Rationalism and Empiricism

I should not like my comments to be interpreted as a complaint that political science worships at the altar of rational choice theory. On the contrary, I think we political scientists have generally been behaving quite well in this respect. If I take the pages of the *American Political Science Review* as representing the attitudes and methods of our discipline, then I observe that there is a healthy respect for sophisticated empirical research. Assumptions of rationality are used to provide a framework for analyzing behavior, but they are generally used tentatively, and with a sensitivity to the assumptions of value, expectation, and belief that have to be added to the models before they can yield predictions of behavior.

Authors who use rational choice models are not always conscious of the extent to which their conclusions are independent of the assumptions of those models, but depend, instead, mainly upon auxiliary assumptions. Nor is advantage taken as often as it could be of the knowledge of cognitive mechanisms to be found in the psychological literature. But these defects, if defects they be, are easily remedied.

It is also a good omen for the future of our science that empirical work means both the study of social aggregates, whose behavior is recorded in public statistics, and the study of the individual actors at the microscopic and face-to-face level of the interview and the poll. The graduate training we provide our students gives them opportunities to acquire skill in both kinds of empirical methodology, and others (e.g., historical inquiry) as well. In this respect, we are better off than our brethren in economics, who are seldom trained in the skills of observing economic phenomena at first hand.

We sometimes, perhaps, experience a mild malaise in that our research does not seem to be taking us in the direction of a few sweeping generalizations that encompass the whole of political behavior. A hope of finding our "three laws of motion" was probably a major part of the appeal of rational choice theory in its purer forms. But a more careful look at the natural sciences would show us that they, too, get only a little mileage from their general laws. Those laws have to be fleshed out by a myriad of facts, all of which must be harvested by laborious empirical research. Perhaps our aspirations for lawfulness should be modeled upon the complexities of molecular biology—surely a successful science, but hardly a neat one—rather than upon the simplicities of classical mechanics.

Radical Irrationality

Thus far, I have dealt with the picture of procedural rationality that emerges from modern cognitive psychology and the relation between that picture and the economist's notion of substantive rationality. My main conclusion is that the key premises in any theory that purports to explain the real phenomena of politics are the empirical assumptions about goals and, even more important, about the ways in which people characterize the choice situations that face them. These goals and characterizations do not rest on immutable first principles, but are functions of time and place that can only be ascertained by empirical inquiry. In this sense, political science is necessarily a historical science, in the same way and for the same reason that astronomy is. What will happen next is not independent of where the system is right now. And a description of where it is right now must include a description of the subjective view of the situation that informs the choices of the actors.

But you may feel that I have not gone far enough in my skepticism about reason in political behavior. Surely even the concept of bounded rationality does not capture the whole role of passion and unreason in human affairs. Don't we need to listen to Lasswell and Freud as well as to Wallas and James?

Assuredly we do. From the earliest times it has been seen that human behavior is not always the result of deliberate calculation, even of a boundedly rational kind. Sometimes it must be attributed to passion, to the capture of the decision process by powerful impulses that do not permit the mediation of thought. The criminal law takes explicit account of passion in assigning different penalties to deliberate and impulsive acts.

In psychoanalytic theory, passion takes mainly the form of unconscious drives, largely unknown

to the actor, that provide the "real" wellsprings of action. This approach, whether it be correct or false, has always been troublesome for empirical research, because it makes suspect human testimony about motives.¹⁰ If we don't know why we act, if our motives are unconscious, then we can't report them, no matter how much we wish to cooperate with the researcher.

Let me take a more conservative approach, which accords well with what we know about the mechanisms that link emotions to reason (Simon, 1978, chap. 1.3). People are endowed with very large long-term memories, but with very narrow capacities for simultaneous attention to different pieces of information. At any given moment, only a little information, drawn from the senses and from long-term memory, can be held in the focus of attention. This information is not static; it is continuously being processed and transformed, with one item being replaced by another as new aspects of a stimulus are sensed, new inferences drawn, or new bits of information retrieved from long-term memory. Nevertheless, of all the things we know, or can see or hear around us, only a tiny fraction influences our behavior over any short interval of time.

If a particular strong drive takes control of our attention, determining not only our goals of the moment but also selecting out the sensory and memory facts that we will consider, then behavior can be determined by that drive or passion as long as its control persists. But passionate behavior in this extreme form is exceptional and not common in human behavior. The control process is usually more complex.

Even in the case of a person like Hitler, whose behavior might be interpreted by some clinicians as a pure instance of an all-consuming hatred or self-hatred, a large cognitive element intrudes into the behavior. Hitler was not just angry; he directed his hatred toward a particular group of people, Jews, and he made decisions that were arguably rational on the premise that the Jewish people were to be extirpated to satisfy that hatred. For some purposes of political analysis, it may be enough to postulate the overtly expressed values and goals without seeking their deeper roots in the unconscious, or at least without trying to explain how they arrived there.

The methodological lesson I would draw is that we need to understand passion and to provide for

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¹⁰For a review of some reasons why we should suspect testimony about motives, see Nisbett and Wilson (1977). The authors of that study draw conclusions that are rather too broad for their evidence, but their main point about reports of motivation are well taken. See also Ericsson and Simon (1984).

it in our political models, but we need particularly to provide in those models for the limited span of attention that governs *what* considerations, out of a whole host of possible ones, will actually influence the deliberations that precede action. In particular, we need to understand the conditions that predispose human beings to impulsive action that disregards much of the potentially relevant reality. I would like to comment on three aspects of this question: the nature of the attention mechanism, the role of uncertainty, and the process whereby novel ways of viewing situations are evoked or generated.

Attention

The human eye and ear are highly parallel devices, capable of extracting many pieces of information simultaneously from the environment and decoding them into their significant features. Before this information can be used by the deliberative mind, however, it must proceed through the bottleneck of attention—a serial, not parallel, process whose information capacity is exceedingly small. Psychologists usually call this bottleneck short-term memory, and measurements show reliably that it can hold only about six chunks (that is to say, six familiar items) of information.

The details of short-term memory and the bottleneck of attention are not important for our purposes. What is important is that only one or a very few things can be attended to simultaneously. The limits can be broadened a bit, but only modestly, by “time-sharing”—switching attention periodically. The narrowness of the span of attention accounts for a great deal of human unreason that considers only one facet of a multifaceted matter before a decision is reached.

For example, it has been hypothesized that the art of campaign oratory is much more an art of directing attention (to the issues on which the candidate believes himself or herself to have the broadest support) than an art of persuading people to change their minds on issues.¹¹ Similarly, shifts in expressed voting intentions during the course of an election campaign have been explained as caused by evocation of beliefs and attitudes already latent in voters’ minds (e.g., party loyalties) (Lazarsfeld et al., 1948, chap. 9). Another example, highly characteristic of the political process, was the shift of attention from environmental problems to problems of energy supply that took place immediately after the Oil Shock, and that greatly altered public priorities for a number of years.

¹¹For a classic statement of this hypothesis, see Lazarsfeld, Berelson, and Gaudet (1948, chap. 8).

The unreason associated with attention focusing has no necessary connection with passion—cold reasoning can be as narrow and one-sided as hot reasoning. But the existence of these narrow limits on the span of human attention is a principal reason why we must distinguish between the “real” situation and the situation as perceived by the political actors when we try to apply the rationality principle to make predictions of behavior. People are, at best, rational in terms of what they are aware of, and they can be aware of only tiny, disjointed facets of reality.

Uncertainty

Lack of reliable knowledge and information is a major factor in almost all real-life decision making. In our soberer moments, we realize how little we know and can predict about the decision-making premises and processes of the rulers of the USSR. Yet the content of a rational foreign policy is highly sensitive to our hypotheses about these matters. The effects of the policies of the president upon the well-being of the American economy are only slightly less uncertain. At least there is often little consensus in the economics profession about these effects.

Wherever such uncertainties are present, an enhanced opportunity is provided for unconscious, or only partly conscious, drives and wishes to influence deliberation. Where the facts are clear (to the actors as well as to us), we have some chance, by application of the principles of reason, to calculate what the choice will be. Where evidence is weak and conflicting, a rationality principle has little independent predictive power.

Evocation

Finally, to understand political choices, we need to understand where the frame of reference for the actors’ thinking comes from—how it is evoked. An important component of the frame of reference is the set of alternatives that are given consideration in the choice process. We need to understand not only how people reason about alternatives, but where the alternatives come from in the first place. The process whereby alternatives are generated has been somewhat ignored as an object of research.

But not wholly ignored! Turning again to my favorite source of information on the state of the profession, I find in a recent issue of *The American Political Science Review* another imaginative paper by William Riker, in fact his 1983 Presidential Address to the Association, on precisely this issue. (I could wish that he had not invented the word “heresthetics” to conceal the heresies he is propagating.) Riker traces the history of pro-

posals in the Constitutional Convention for electing the President, with particular concern for the generation of new alternatives, and for the shifts in attention and emphasis on issues that accompanied their introduction.

Riker speaks of these matters in terms of "artistry within the rational choice context." I think that the generation of alternatives is much more than that: that it is an integral component of any veridical account of human decision making, or of human bounded rationality generally. The theory of the generation of alternatives deserves, and requires, a treatment that is just as definitive and thorough as the treatment we give to the theory of choice among prespecified alternatives.

But is such a treatment possible? Are we not treading upon the sacred precincts of creativity? Indeed we are; but I think the precincts are no longer sacrosanct. The same cognitive psychology that has been elaborating the theory of human bounded rationality has made considerable progress toward constructing models of the processes of discovery and creativity that can account for these processes in terms quite akin to those it uses to account for ordinary problem solving. Again, I cannot tell that story here but must limit myself to pointers to the literature (Bradshaw, Langley, & Simon, 1983; Lenat, 1983).

Conclusion

In this essay I have tried to provide an overview—a very general one—of our current knowledge of human nature in politics. I first undertook to compare the two principal theories of human rationality that have found application in political research: the procedural bounded rationality theory that has its origins in contemporary cognitive psychology, and the substantive global rationality theory that has been nurtured chiefly in economics. Then, by means of a series of examples, I examined the relative roles played by rationality principles and by the auxiliary assumptions that accompany them, respectively, in predicting and explaining human behavior in political contexts. Finally, I commented on the more extreme deviations from the objective rationality model that exhibit themselves in political affairs, and showed how they could be explained, in considerable measure, in terms of the mechanisms of attention and the severe limits that the architecture of the mind places on the span of human attention.

My overview, if it is even partly valid, carries a number of implications for research in political science. First, it dissipates the illusion, if anyone holds it, that an application of principles of rationality can discharge us, to any considerable degree, from the need to carry on painstaking empirical

research at both macro and micro levels. It is far easier (for the political scientist and for the political actor) to calculate the rational response to a fully specified situation than it is to arrive at a reasonable specification of the situation. And there is no way, without empirical study, to predict which of innumerable reasonable specifications the actors will adopt.

Second, my overview suggests that the study of the mechanisms of attention directing, situation defining, and evoking are among the most promising targets of political research. In particular, the question of where political ideas come from is not only highly deserving of study, but also within the competence of our contemporary research techniques. I join Bill Riker in commending it to you as one of the truly exciting and significant areas of investigation in our field.

Nothing is more fundamental in setting our research agenda and informing our research methods than our view of the nature of the human beings whose behavior we are studying. It makes a difference, a very large difference, to our research strategy whether we are studying the nearly omniscient *Homo economicus* of rational choice theory or the boundedly rational *Homo psychologicus* of cognitive psychology. It makes a difference to research, but it also makes a difference for the proper design of political institutions. James Madison¹² was well aware of that, and in the pages of the *Federalist Papers* he opted for this view of the human condition (*Federalist*, No. 55):

As there is a degree of depravity in mankind which requires a certain degree of circumspection and distrust, so there are other qualities in human nature which justify a certain portion of esteem and confidence.

—a balanced and realistic view, we may concede, of bounded human rationality and its accompanying frailties of motive and reason.

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¹²Or was it Hamilton? Mosteller and Wallace (1964) attribute No. 55 of *The Federalist* to Madison, but it is the least certain of their attributions of the numbers whose authorship has been disputed. Since the sentiment quoted here is certainly consistent with the beliefs of both authors, we need not be too concerned with the uncertainty of authorship.

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Partisan Intentions and Election Day Realities in the Congressional Redistricting Process

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Political scientists have done little to resolve the question of whether or not a state party's command of congressional redistricting leads to payoffs in its share of delegation seats. Studies on the topic differ in their conclusions while being marred by methodological shortcomings. In analyzing districting plans from 1952 to 1982, one finds that partisan control of redistricting does have the expected effect on seat outcomes, but only modestly. This relationship, though, has become even more tenuous over time. Since the imposition of the Supreme Court's "one man, one vote" mandates, the partisan gains intended by redistricting schemes have come to depend less heavily on the identity of the controlling party. And regardless of what is intended by redistricting architects, the electorate has become less willing to conform to their expectations, quite plausibly because of incumbents' growing ability to add residents of the new areas to their supporting coalition by election day.

The people of Pennsylvania are about one-third Federalist, yet all their new members of Congress are Democrats; the legislature took great care in laying out the districts, to make the elections as sure for the Democrats as if they had voted by a ticket at large.

The Salem Gazette, 1802

In 1868 the voice of the people of the State of New York was stifled by the glaring frauds that were committed in the great city bearing that name. . . . And over all was the slimy trail of the serpent of the gerrymander, slowly but surely strangling the right of suffrage to death.

U.S. Congress, House 1893, p. 195

When Bill Brock assumed chairmanship of the Republican National Committee (RNC) in 1977, he placed a high priority on winning state legislative elections. With Democrats in control of so many legislatures, Brock said, the GOP faced the threat of being "gerrymandered out of existence" in 1981.

Buchanan (1979, p. 1751)

Politicians and political activists in the minority never have been loath to blame gerrymandering for many of their party's misfortunes, but during the late 1970s, alarm over the perceived dangers of gerrymandering went well beyond mere ritualistic denunciation. In an unprecedented move by a na-

tional party, the Republican National Committee (RNC) in 1978 launched a \$2 million drive to increase Republican control over the upcoming congressional redistricting by capturing additional state legislative chambers (Buchanan, 1979, p. 1751). Spurred by the successes of that year, a second, \$2.9 million campaign was initiated in 1980, accompanied by the efforts of a new political action committee (GOPAC) established expressly for this purpose (Buchanan, 1980, p. 3188).

In devising a route to Washington success which wound through the state capitals, however, GOP strategists evidently were not being guided by the findings of political science research. Even though the study of congressional elections has become a major growth industry, the discipline has said little about the partisan effects of House redistricting, and the infrequent explorations of this topic provide mixed signals to those who believe that a state party in charge of redistricting machinery can make a real electoral difference. In research done more than a decade ago, Mayhew (1971, pp. 276-278) did infer the existence of a purposeful 1950s Republican gerrymander in large northern states. However, subsequent analysis conducted by Erikson (1972, pp. 1241-1244), even though not specifically done with Mayhew in mind, suggests that most northern pro-Republican bias before the mid-1960s was the accidental result of Democratic voters being concentrated in constituencies that delivered "excessive" majorities for their party. The partisan gerrymander explanation, in contrast, is rejected because of Erikson's discovery of only a weak tendency for state parties controlling redistricting in 1962, 1966, and 1970 to increase their share of seats above 1960-base levels. A further study of the 1960s that draws similar conclusions

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is by Noragon (1973, pp. 324-326), who finds, with regard to seats changing party hands after redistricting, a tenuous relationship between the party that devised the plan and the direction of the partisan shift.

But although Mayhew indeed can be faulted for failing to employ a longitudinal perspective (i.e., not determining whether *changes* in party control of redistricting produce parallel shifts in partisan seat advantage),¹ the latter two studies themselves are flawed by not controlling for the possibly confounding effects of statewide voting trends.² A newly minted Democratic gerrymander may be frustrated come the following election, for example, because of the emergence of an unforeseen GOP electoral surge.

Two studies sensitive to the importance of the statewide vote, as well as the need to examine party advantage across a range of districting schemes, have reached very different conclusions from those of Erikson and Noragon. Sickels (1966, pp. 1302-1303) shows that state parties with narrow vote majorities from 1964-1964 won substantially more seats when they had written existing districting plans. Similarly, Elliott (1974, pp. 227-232) observes that among parties winning the popular vote in large northern states from 1966 to 1972, the degree to which their seat percentage exceeded their vote percentage is much greater when they controlled redistricting.

Here too, however, there are grounds for questioning the significance of the results. Although Sickels does attempt to control for the statewide vote by analyzing marginally victorious parties only, it still appears even within his 50-55% vote range that parties in charge of redistricting do somewhat better in the popular vote. This fact is hardly surprising since a party dominating state government also is likely to be stronger in con-

gressional elections and thus by itself may provide the redistricting party with a large share of seats. Elliott's analysis, on the other hand, controls more precisely for the statewide vote by subtracting from a party's seat share its exact vote proportion. But because a system of single-member districts tends to award the majority more than a 1% seat gain for each 1% increment in votes (Grofman & Scarrow, 1982, p. 452; Scarrow, 1981, p. 8; Tufte, 1973, pp. 542-544), the excess of seats over votes for parties dominating redistricting—because of their normally greater electoral strength—is likely to be inflated (e.g., if each 1% vote gain produced a 2% jump in seats, the seat-vote differential at 55% of the vote would be 4% more positive than the difference at 51%). So both Sickels and Elliott might well overstate the true degree to which party control of redistricting matters.

There is, however, a further problem common to *all* five studies that have been discussed here. No matter how closely their conclusions approximate reality, it is impossible to determine the causal mechanisms that link party control of redistricting to seat outcomes. In fact, this linkage is more complex than previously conceptualized, even when the complications posed by biennial vote shifts are taken into account. Two stages must be traversed if a party is to realize gains: first, it must draft a plan intended to yield a greater haul of seats, and second, voters must then behave at the polls according to that plan.

The importance of the first stage as the explanation for party failure largely has been ignored by researchers, but there is no good reason to implicitly assume as they have that a party in control of state government will be willing or able to devise a scheme enhancing its own partisan self-interest. Mayhew himself suggests, for instance, that in states dominated by a single, weakly organized party, redistricting decisions often take the form of concessions to its congressmen, even if safeguarding their seats should negate any possibility of picking up minority districts (1971, pp. 280-284). Alternatively, a redistricting party may be quite happy to accept a net reduction in its representation, if this is the price to be paid for purging a disfavored incumbent.³ Finally, it is conceivable that since strict standards of district

¹In a research note also not adopting a longitudinal perspective, Abramowitz (1983, pp. 767-770) finds that the 1980-1982 swing ratio (i.e., the increase in the Democratic seat percentage divided by the increase in the Democratic vote percentage) is larger when Democrats control both legislative chambers and the governorship than in states with weaker Democratic strength. Within each partisan category, however, this ratio is computed on the basis of votes and seats *aggregated* across states, rather than calculated as the average of the individual state swing ratios. Thus, his estimates may be skewed in the direction of redistricting effects obtaining in only a handful of large states; for example, of the additional 15 Democratic seats won from 1980-1982 in the 17 states with total Democratic control of redistricting, fully 40% came solely from California.

²Erikson (1972, p. 1242), though, does briefly consider the interaction between the partisanship of a plan and vote shifts when he singles out a handful of specific redistrictings for more extended discussion.

³A celebrated example from the 1981-1982 round of redistricting concerned the efforts of Massachusetts Democratic leaders to eliminate maverick liberal Barney Frank by merging his district with that of adjoining Republican Margaret Heckler (Congressional Quarterly, 1982, p. 20). Despite the fact that the new district contained far fewer of his constituents, Frank easily won in the fall.

population equality have been imposed by Supreme Court rulings in the mid- and late 1960s, parties whose congressmen disproportionately represent underpopulated districts may be resigned merely to holding their losses down rather than pursuing chimerical dreams of maintaining or increasing party strength.⁴

All in all, then, having carte blanche over redistricting does not guarantee that a party will intend partisan gains in the first place, let alone realize any such gains on election day. Both stages of the process outlined above are examined, as I investigate how state parties have fared under various districting plans. Following this, I switch from states to House districts as the units of analysis, focusing on the relationship between the intended outcomes of redistricting decisions and the actual results themselves. Here, attention is devoted to the ability of incumbents to cultivate support from areas newly appended to their districts.

Data and Methods

A total of 91 congressional districting plans employed in 20 states from 1952-1982 are considered in this study. Because a single seat shifting party hands can unduly affect calculations of partisan change for very small delegations, a state is selected for analysis only if it had at least four representatives throughout this period.

The party authorship of each redistricting is fixed by modifying a categorization scheme initially put forth by Erikson (1972, p. 1242). Like Erikson, I define a plan as Democratic or Republican if a single party controlled both state legislative houses as well as the governorship, or if a party controlling both chambers overrode the veto of an opposition governor. Similarly, I call bipartisan a plan drawn by a legislature with divided party control, or one devised by a legislature under single-party domination and then signed into law by an opposition governor.

I differ from Erikson, though, in not excluding from my study those plans ordered by federal courts. In almost all such cases, a Democratic or Republican classification readily can be made, since the judges have approved with minor changes an already existing proposal initiated by a legislative party. But where courts themselves have altered significantly a legislative scheme (e.g., Maryland in 1966) or delegated the redistricting task to special "masters" (e.g., California in 1973), I classify the plan as bipartisan.

One other way that I differ from Erikson is in

not eliminating redistricting carried out by non-partisan state legislatures. It seems clear that the two such cases analyzed—Minnesota in 1961 and 1971—should be regarded as bipartisan, in that the first represented a compromise between the "liberal" controlled House and "conservative" dominated Senate (Minnesota—one seat loss, 1961), whereas the second was negotiated between the "conservative" faction controlling both houses and the Democratic governor (Redistricting in Texas. . . , 1971). Table 1 presents the partisanship assigned to each districting.⁵

The Effect of Districting Partisanship on Election Outcomes: A State-Level Perspective

Within states experiencing at least two of the three kinds of districting partisanship from 1952 to 1982, time series analysis is performed, regressing the Democratic proportion of seats on the Democratic share of the statewide two-party vote (*VOTE*) and a variable (*PART*) representing the partisanship of the current plan. (*PART* is 1 for Democratic, 0 for bipartisan, and -1 for Republican districtings.) Besides providing an overview of the consequences of partisanship, this analysis also has the virtue mentioned above of contrasting a given party's success in winning seats across different state districting schemes. By treating each state as its own control, I thus can avoid a potential problem faced by those who attempt to measure redistricting effects one election at a time—biased estimates in years when states with seat-vote ratios historically favoring one party for reasons other than gerrymandering happen to fall disproportionately within the ranks of a particular districting partisanship category.

Because too many uncontested seats held by one party could lead to a deceptively large calculation of its statewide vote proportion, the only elections included in a state's regression are those in which at least 75% of the seats have major-party competition.⁶ Furthermore, I do not

⁵Because only four Brooklyn districts were reconstructed in 1974 in response to a court finding of racial gerrymandering in New York's 1972 plan, the more recent scheme is not considered a separate redistricting.

⁶Although the percentage of contested seats in Oklahoma falls below the acceptable level only in 1962, the prior years as well are dropped so that the series has no gap. Also, even though the Republican plans used in Kansas across the entire 1952-1980 period were succeeded by a Democratic plan in 1982, this state had to be totally excluded from the analysis here because it had too few contested seats in 1978. It was thus impossible to consider the lone Democratic districting year, lest the series be noncontinuous.

⁴For a similar argument, see Ayres and Whiteman (1982, p. 6).

Table 1. Partisan Classification of 91 Districting Plans, 1952-1982

Election Years in which Plan was Used	State	Partisanship of Plan	Election Years in which Plan was Used	State	Partisanship of Plan
1952-1960	California	Republican	1968-1970	Oklahoma	Bipartisan
1952-1960	Illinois	Bipartisan	1968-1970	Washington	Democratic
1952-1964	Indiana	Bipartisan	1968-1970	West Virginia	Democratic
1952-1960	Iowa	Republican	1970	Missouri	Democratic
1952-1960	Kansas	Republican	1970	New York	Republican
1952-1964	Maryland	Democratic	1972	California	Democratic
1952-1962	Michigan	Bipartisan	1972-1980	Colorado	Republican
1952-1960	Missouri	Democratic	1972-1980	Connecticut	Democratic
1952-1960	New York	Republican	1972-1980	Illinois	Republican
1952-1964	Ohio	Bipartisan	1972-1980	Indiana	Republican
1952-1966	Oklahoma	Democratic	1972-1980	Iowa	Republican
1952-1964	Oregon	Republican	1972-1980	Kansas	Republican
1952-1960	Pennsylvania	Republican	1972-1980	Maryland	Democratic
1952-1960	West Virginia	Democratic	1972-1980	Michigan	Democratic
1958	Washington	Democratic	1972-1980	Minnesota	Bipartisan
1960-1966	Washington	Democratic	1972-1980	Missouri	Democratic
1962-1966	California	Democratic	1972-1980	New Jersey	Republican
1962-1964	Illinois	Bipartisan	1972-1980	New York	Republican
1962-1970	Iowa	Republican	1972-1980	Ohio	Bipartisan
1962-1964	Kansas	Republican	1972-1980	Oklahoma	Democratic
1962-1970	Minnesota	Bipartisan	1972-1980	Oregon	Bipartisan
1962-1964	Missouri	Democratic	1972-1980	Pennsylvania	Democratic
1962-1964	New Jersey	Bipartisan	1972-1980	Washington	Bipartisan
1962-1966	New York	Republican	1972-1980	West Virginia	Bipartisan
1962-1964	Pennsylvania	Bipartisan	1972-1980	Wisconsin	Bipartisan
1962-1966	West Virginia	Democratic	1974-1980	California	Bipartisan
1964-1970	Colorado	Republican	1982	California	Democratic
1964-1970	Connecticut	Bipartisan	1982	Colorado	Bipartisan
1964-1970	Michigan	Republican	1982	Connecticut	Democratic
1964-1970	Wisconsin	Bipartisan	1982	Illinois	Democratic
1966-1970	Illinois	Bipartisan	1982	Indiana	Republican
1966	Indiana	Democratic	1982	Iowa	Republican
1966-1970	Kansas	Republican	1982	Kansas	Democratic
1966-1970	Maryland	Bipartisan	1982	Maryland	Democratic
1966	Missouri	Democratic	1982	Michigan	Democratic
1966	New Jersey	Democratic	1982	Minnesota	Democratic
1966	Ohio	Republican	1982	Missouri	Democratic
1966-1970	Oregon	Bipartisan	1982	New Jersey	Democratic
1966-1970	Pennsylvania	Bipartisan	1982	New York	Bipartisan
1968-1970	California	Bipartisan	1982	Ohio	Bipartisan
1968-1970	Indiana	Bipartisan	1982	Oklahoma	Democratic
1968	Missouri	Democratic	1982	Oregon	Bipartisan
1968-1970	New Jersey	Republican	1982	Pennsylvania	Republican
1968	New York	Bipartisan	1982	Washington	Republican
1968-1970	Ohio	Republican	1982	West Virginia	Democratic
			1982	Wisconsin	Bipartisan

Note: New York is not considered to have implemented a separate additional redistricting in 1974, because the Court-imposed boundary alterations that year only affected four Brooklyn districts.

analyze years in which the current districting plan had been drawn up 20 or more years earlier. A minimum of nine consecutive elections must meet these criteria in order for a regression to be done.⁷

Table 2 presents the regression results, which were generated by the Cochrane-Orcutt technique. Overall, redistricting does seem to benefit the controlling party, but not impressively; 10 of the 16 coefficients (62.5%) for districting partisanship have the hypothesized positive sign, and four are significant at the .05 level.⁸ The .024 average coefficient value indicates that moving from a Republican to bipartisan or bipartisan to Democratic plan can be expected to yield a 2.4% Democratic seat gain.⁹

I also examined whether or not any partisan profits achieved through imposition of redistricting might dissipate over time owing to population migration. In my test, performed within states whether or not they underwent a change in districting partisanship, I applied a simple dummy coding scheme differentiating each specific districting to a time series regression of seats on votes, and I included a trend variable for each plan lasting four elections or longer. The results, however, suggested no systematic tendency for party fortunes to wane or wax across the course of a districting. Election returns from the initial year a plan is used, therefore, seem representative of the overall partisan impact across the entire period it remains in effect.¹⁰

⁷In this analysis and those to come, all calculations of state votes and seats exclude from consideration at-large seats, such as Michigan's in 1962.

⁸Because, as noted above, a state's majority party is likely to write most redistricting plans as well as to receive a majority of the statewide vote, it is not surprising to find a positive correlation between the districting partisanship and vote variables across most of the 16 regressions in Table 2. The average R^2 value, though, is only .126—far too weak to create a collinearity problem of inflated standard errors for the regression coefficients.

⁹Although the argument has been made above in favor of using time series analysis to compute the overall partisan effect of redistricting, I also performed an alternative cross-sectional regression of Democratic seat proportions on the statewide vote, districting partisanship, and the 15 dummy variables needed to differentiate the 16 election years from 1952-1982. Here, data came from the states experiencing changes in districting partisanship which were analyzed in Table 2, as well as the four others considered in this study (Iowa, Kansas, Missouri, and Wisconsin); the only criteria for inclusion were that at least 75% of seats were contested, and the districting plan in effect was no more than 20 years old. The resulting coefficient of *PART*—.032 (s.e. = .010)—indicated a somewhat stronger partisan effect of redistricting than that gauged above.

¹⁰Specific results of this analysis are available by request.

The analysis so far, of course, says nothing about the causal process linking partisan control of redistricting to a certain share of delegation seats. As argued before, a two-equation model must be developed which differentiates intended partisan advantage from actual voter behavior in November.

To gauge the partisan intent of a particular scheme, I aggregate the total pre-districting House vote received in all areas later included within the new district boundaries by the various Democratic and Republican candidates who campaigned there. These recomputations—which estimate what Democratic and Republican vote totals would have been before redistricting had the new lines been in effect—can then be compared with actual pre-redistricting results to determine the changes intended in a party's share of delegation seats. Certainly performing such analysis for each districting blueprint considered in a state—facilitated by sophisticated computer technology—is standard practice for drafters themselves.¹¹

My specific measure of partisan intentions represents a compromise between using all possible House election data from a state's previous districting and ensuring that the number of past elections used in the computation is comparable among states.¹² When retabulations exist for two

¹¹Recomputed data from previous House elections are not the only kind that can be used. Two recent studies focusing on how individual incumbents were affected by redistricting decisions in 1981-1982 have measured district partisan change, respectively, with retabulated presidential election data (Gopoian & West, 1984) and retabulated party registration figures (Cain, 1983). Although such statistics do not require aggregating data for different House candidates of a party who may vary with respect to the personal component of their vote, they are not nearly as available, especially for the earlier period of my analysis. For example, recomputed presidential data can be found for all districts in a state (or computed from county-level returns when districts before and after redistricting consist solely of intact counties) only in 39% of the 1952-1964 redistrictings considered here. And these presidential statistics are by far the most common of any recomputations with non-House data. Furthermore, a substantive question exists concerning the legitimacy of using retabulations based on statewide candidates to make inferences about redistricting changes intended at the House level of competition. For instance, removing a conservative blue-collar area from the district of a mainstream California Democrat in 1973 might well make his constituency *more* Democratic in terms of the 1972 McGovern vote, while still eliminating a part of his own customary supporting coalition.

¹²Recompiled House election data were gathered from a number of sources. Statistics for 1981-1982 redistrictings were taken from Congressional Quarterly (1983) whereas Congressional Quarterly (1973) as well as the

Table 2. Measuring the Effect of Districting Partisanship on Seat Outcomes—
Time Series Regressions for States Districting under Plans of
at Least Two Different Kinds of Partisanship

State	Election Years in Series	Coefficient		Constant	R ²
		PART	VOTE		
California	1954-1982	.035 (.020) .335	.821* (.332) .404	.147	.429
Colorado	1964-1982	-.086 (.108) -.161	.332 (1.164) .082	.327	.099
Connecticut	1964-1982	-.083 (.063) -.312	1.794* (.580) .558	-.225	.630
Illinois	1952-1982	.022 (.016) .224	.818* (.204) .570	.091	.704
Indiana	1952-1982	-.017 (.045) -.048	4.457* (.702) .812	-1.744	.794
Maryland	1952-1982	-.048 (.080) -.121	1.998* (.537) .766	-.435	.548
Michigan	1952-1982	.032 (.024) .181	2.378* (.341) .687	-.788	.806
Minnesota	1962-1982	.084* (.015) .262	2.262* (.104) .946	-.707	.987
New Jersey	1962-1982	.000 (.019) .000	1.852* (.410) .741	-.345	.745
New York	1952-1982	.001 (.027) .004	1.163* (.270) .500	-.033	.615
Ohio	1952-1982	.055 (.042) .266	1.052* (.304) .474	-.123	.614
Oklahoma	1964-1982	.155* (.064) .621	1.148 (.750) .496	.007	.691
Oregon	1952-1982	-.042 (.052) -.079	2.782* (.357) .864	-.907	.843
Pennsylvania	1952-1982	.036* (.014) .434	.945* (.423) .411	.040	.603
Washington	1958-1982	.092 (.067) .224	2.474* (.576) .680	-.624	.672
West Virginia	1952-1982	.141* (.058) .455	2.306* (.506) .959	-.617	.657
Averages		.024	1.786	-.371	.652

*Significant at the .05 level, two-tail *t* test.

Note: Entries appearing on first line of each row are unstandardized regression coefficients and their associated standard errors (in parentheses); entries on second line are standardized regression coefficients. All regression values have been computed with the Cochrane-Orcutt technique. Years eliminated from regressions because districting plan in effect was 20 or more years old are Colorado (1952-1962), Connecticut (1952-1962), Minnesota (1952-1960), New Jersey (1952-1960), and Washington (1952-1956); those dropped because less than 75% of seats were contested are California (1952) and Oklahoma (1962). (The 1952-1960 elections also are not analyzed for Oklahoma to keep its series continuous.)

or more elections held under the old plan, the intended Democratic vote proportion in each new district is calculated as the average Democratic share of the recomputed two-party vote across the two most recent elections. Otherwise, the calculation is based upon the immediately preceding election only. The intended Democratic seat proportion, then, becomes the share with an intended Democratic vote above 50%.

The first of the two equations to be estimated, with each observation corresponding to a distinct 1952-1982 state redistricting, is:

$$INSTS = a_1 + b_1PART + b_2POP + b_3STS + b_4TIME_1 + b_{10}TIME_7 + u. \quad (1)$$

The partisanship of a plan (*PART*) already has been defined; in addition:

INSTS is the intended change in the Democratic share of delegation seats, that is, the intended post-redistricting Democratic proportion minus the actual pre-redistricting proportion.¹³

POP is the average population, under the previous plan, of a state's Democratic districts divided by the average population of Republican districts.

STS is the actual pre-redistricting Democratic seat proportion.

U.S. Dept. of Commerce Census Bureau (1971, 1973, 1975) supplied the data for early 1970s and some late 1960s redistrictings. With regard to other 1960s redistrictings, the U.S. Dept. of Commerce, Bureau of the Census (1963) and its supplements include the necessary computations when new districts consisted only of intact counties. Fortunately, the remaining 1960s data, as well as those for the 1950s, can be acquired from *Congressional Quarterly Weekly Report*, which would generate estimates of the recomputed vote soon after a redistricting bill became law. (On the infrequent occasions when *CQWR* did not present precise quantitative estimates, I used its judgment of the general partisan disposition of the new districts to determine the intent of the plan.)

¹³When retabulated data from the two most recent elections were used to compute the intended Democratic seat proportion, the actual pre-redistricting proportion was calculated as the share of seats in which the average Democratic vote across these elections exceeded 50%. If only the immediately preceding election was used to calculate the intended proportion, the pre-redistricting share simply was the actual proportion won by the Democrats that year. Similarly, when I later calculate the Democratic pre-redistricting proportion of the statewide vote, I figure the average vote proportion across the two most recent past elections if both elections have been used to compute the intended seat share, and the vote proportion in the single most recent past election otherwise.

*TIME*₁ . . . *TIME*₇ differentiate the years when the plans first were used: *TIME*₁ is 1 for 1960-1962 plans and 0 otherwise, *TIME*₂ is 1 for 1964 plans and 0 otherwise, *TIME*₃ is 1 for 1966 plans and 0 otherwise, *TIME*₄ is 1 for 1968 plans and 0 otherwise, *TIME*₅ is 1 for 1970 plans and 0 otherwise, *TIME*₆ is 1 for 1972-1974 plans and 0 otherwise, and *TIME*₇ is 1 for 1982 plans and 0 otherwise (1952 plans serve as the reference group)¹⁴ and

u is the error term.

Besides the partisanship of the plan, the population discrepancy variable is entered into the equation under the assumption mentioned above that intended party gains may be circumscribed to the extent that its districts before redistricting are relatively underpopulated. This is more likely to be true since the Supreme Court's "one man, one vote" mandates, but a de facto policy of achieving greater population equality seems to have operated even earlier; that is, in the plans antedating the 1964 *Wesberry v. Sanders* decision, interdistrict variation always was reduced. In addition, *STS* is included because an intended partisan increment of given size likely will become harder to fashion as the pre-redistricting number of seats held by the party becomes larger. Finally, the seven dummy time variables control for any remaining influences on intended seat change which are unique to particular elections.

To analyze the second step of the process, the following equation is estimated:

$$ACSTS = a_2 + b_{11}INSTS + b_{12}ACVT + b_{13}TIME_1 + \dots b_{19}TIME_7 + u. \quad (2)$$

¹⁴Because there was only one redistricting each in 1960 (Washington) and 1974 (California), they were included with those in 1962 and 1972, respectively.

¹⁵Although a total of 91 different districting plans form the data base for this study, only 79 redistrictings are employed here. Six of the redistrictings that established the 91 plans had to be dropped because before redrawing lines the controlling party held every delegation seat; that is, it was impossible for such a party to attempt any increase in its share of seats, or for any increase actually to materialize. Four others could not be used because they were written in 1941-1942; that is, my analysis here of seat outcomes that occurred in the initial year of a districting plan begins with 1952. California's 1951 redistricting is not included because less than 75% of its seats were contested in the initial year it went into effect (i.e., 1952). Finally, Washington's 1982 plan is excluded as a consequence of its extreme *INSTS* value (the largest for any redistricting); the intended pro-Republican change here is misleadingly high, because of the statistical quirk that two of its eight new districts have recomputed vote proportions falling only

Only *ACSTS* and *ACVT* remain to be defined:

ACSTS is the actual change in the Democratic share of delegation seats; that is, the actual Democratic proportion in the first election after redistricting minus the actual pre-redistricting proportion, and

ACVT is the actual change in the Democratic share of the statewide two-party vote; that is, the actual Democratic proportion in the first election after redistricting minus the actual pre-redistricting proportion.

Here, I expect that the actual seat shift (*ACSTS*) will be positively related to the intended seat

.002 or less below the .5 cutoff point used to differentiate Democratic from Republican seats.

change (*INSTS*) and the statewide vote change (*ACVT*); in addition, I allow for any period effects by including *TIME*₁ to *TIME*₇.

Because all independent variables in equation (1) are assumed to be predetermined, estimation is performed with ordinary least squares (*OLS*). Because *INSTS* on the right side of equation (2) is conceptualized as endogenous, however, *OLS* estimation may yield a biased coefficient for this variable owing to correlation with the error term. Hence, two-stage least squares (*2SLS*) is used to generate the regression coefficients (with *INSTS* regressed on *PART*, *POP*, *STS*, *TIME*₁ . . . *TIME*₇, and *ACVT* to form *INSTS*). The results of these two regressions appear in Table 3.

Districting partisanship is shown in equation (1) to be a significant predictor of redistricting intentions. Its understandardized coefficient indicates

Table 3. A Two-Equation Model of the Redistricting Process:
A Full Analysis of Redistrictings Implemented from 1952-1982

Coefficient	Equation (1)		Equation (2)	
<i>PART</i>	.045* .420	(.013)	—	
<i>POP</i>	.112 .173	(.076)	—	
<i>STS</i>	-.028 -.059	(.062)	—	
<i>INSTS</i>	—		.771* .542	(.277)
<i>ACVT</i>	—		1.372* .533	(.359)
<i>TIME</i> ₁	-.030 -.121	(.038)	.088* .251	(.037)
<i>TIME</i> ₂	-.207 -.069	(.053)	.226* .409	(.059)
<i>TIME</i> ₃	-.035 -.125	(.042)	.054 .134	(.045)
<i>TIME</i> ₄	-.053 -.196	(.041)	.079 .206	(.042)
<i>TIME</i> ₅	.004 .007	(.067)	-.020 -.026	(.064)
<i>TIME</i> ₆	-.051 -.259	(.034)	.049 .174	(.040)
<i>TIME</i> ₇	-.067 -.329	(.035)	.015 .053	(.045)
Constant	-.079		-.036	
<i>R</i> ²	.217		.649	

*Significant at .05 level.

Note: Entries appearing on first line of each row are unstandardized regression coefficients and their associated standard errors (in parentheses); entries on second line are standardized regression coefficients. First equation has been estimated with *OLS*, second equation with *2SLS*. Number of redistrictings analyzed is 79.

Table 4. A Two-Equation Model of the Redistricting Process:
An Analysis of Redistrictings Implemented from 1966-1982 Only

Coefficient	Equation (1)		Equation (2)	
PART	.031* .295	(.014)	—	
POP	.310* .395	(.100)	—	
STS	-.025 -.049	(.072)	—	
INSTS	—		.668* .531	(.282)
ACVT	—		1.319* .637	(.350)
TIME ₄	-.020 -.091	(.038)	.026 .093	(.047)
TIME ₅	.030 .067	(.063)	-.066 -.118	(.068)
TIME ₆	-.014 -.078	(.033)	-.002 -.010	(.049)
TIME ₇	-.026 -.146	(.033)	-.035 -.156	(.056)
Constant	-.299		.012	
R ²	.245		.523	

*Significant at .05 level according to two-tail *t*-test.

Note: Entries appearing on first line of each row are unstandardized regression coefficients and their associated standard errors (in parentheses); entries on second line are standardized regression coefficients. First equation has been estimated with OLS, second equation with 2SLS. Number of redistrictings analyzed is 56.

that a shift of one step toward a Democratic plan will increment the intended gain in Democratic seats by 4.5%. Furthermore, the coefficient of intended seat gain in the second equation suggests that these intentions, in turn, are translated into actual results; on the average, 77.1% of what is intended will be achieved on election day.

More daunting prospects for adding seats, however, have been faced by parties controlling redistricting since the mid-1960s. In Table 4, I estimate the same two equations as before (minus *TIME*₁ to *TIME*₃), now including only redistrictings after the 1964 *Wesberry* decision.¹⁶ The most striking feature of equation (1) is that the population discrepancy variable—not significant in the preceding full analysis—has sharply risen in magnitude,

whereas partisanship has a considerably reduced effect. Comparison of the standardized regression coefficients shows, in fact, that population discrepancy has the single most important impact. In addition, the partisan intention coefficient in the second equation has declined to .668 from the .771 value reached before. Clearly, then, the legal obligation to establish equal-size districts makes it harder today for parties disproportionately controlling underpopulated districts to devise more favorable plans, but even when such plans are invented, the odds are reduced that implementation will lead to the desired gains.

Estimation of equation (2) has been carried out so far only for the first post-redistricting election, but the earlier failure to uncover any tendency for partisan effects to vary over the life of a plan suggests here as well that partisan intentions would not have very different impacts on seat outcomes if subsequent elections were analyzed. I did explore the possibility, though, by modifying *ACSTS* and *ACVT* so that they were based on the average actual Democratic seat and vote shares during the lifetime of a districting, but the results

¹⁶Even though final enactment of 1964 plans for Colorado, Connecticut, Michigan, and Wisconsin did not occur until after the Court's February 17 *Wesberry* edict, I do not analyze these redistrictings in Table 4 because the actual drafting processes themselves were initiated earlier.

differed little from those reported in Tables 3 and 4.

Examining the Electoral Effects of Redistricting on Individual Districts

A limitation of all work done up to now has been the focus on the state as the unit of analysis. This approach may have been somewhat too narrow in inferring the desired share of delegation seats simply from the proportion with a recomputed vote in excess of 50% for the controlling party and then judging the success of the plan in terms of whether the intended share actually materializes. Opposition seats, which are made more marginal but not quite to the 50% point and which then turn over in the fall, will attenuate the state-level relationship between intended and actual seat change, even though a major reason for this turnover obviously would lie in the handiwork of the redistricting architects. Thus, an alternative evaluation of redistricting effectiveness can be achieved by adopting the perspective of individual districts and determining how well intended shifts in the vote correspond to actual shifts.

Toward this end, I originally attempted to devise a two-equation model analogous to the state-level model just estimated. Ultimately, however, it proved impossible to develop an intended-vote-shift equation with anything like the explanatory power of its state-level counterpart. Because a strong *2SLS* instrument for intended-vote change consequently could not be created, I am limited to estimating the actual vote change equation only, treating intended changes as exogenous and applying OLS.

The following equation is estimated (a separate regression is done for each year, because performing district-level analysis provides an adequate number of cases per election):¹⁷

$$\begin{aligned} ACVT = & a_1 + b_1 INVT + b_2 DVET \\ & + b_3 DFR + b_4 DOPEN + b_5 RFR \\ & + b_6 ROPEN + b_7 ST_1 \\ & + \dots b_{7+(k-2)} ST_{k-1} + u. \end{aligned} \quad (3)$$

As before, *ACVT* is the actual pre- to post-redistricting change in the Democratic two-party vote share, now computed, though, for districts.

INVT is the intended change in the Democratic share of the district two-party vote; that is, the

intended post-redistricting proportion minus the actual pre-redistricting proportion.

DVET is 1 if a Democratic incumbent wins reelection before redistricting and runs again afterward, and 0 otherwise.

DFR is 1 if a Democratic nonincumbent wins the seat before redistricting and runs again afterward, and 0 otherwise.

DOPEN is 1 if there is an open-seat election after the redistricting produced by the retirement of a Democratic incumbent, and 0 otherwise.

RFR is 1 if a Republican nonincumbent wins the seat before redistricting and runs again afterward, and 0 otherwise.

ROPEN is 1 if there is an open-seat election after the redistricting produced by the retirement of a Republican incumbent, and 0 otherwise.

*ST*₁ . . . *ST*_{k-1} are dummy variables differentiating the *k* states implementing plans in the year being analyzed.

The dummy variables *DVET* and *ROPEN* control for actual vote changes which can be expected to result from combinations of incumbency status and party membership. For Democratic veterans, this simply is the national partisan shift, whereas for the remaining four groups, the changes are those relative to the national shift caused either by acquisition of a first term of incumbency (pro-Democratic for Democratic freshmen, and pro-Republican for Republican freshmen) or incumbent retirement (pro-Republican in seats with departing Democratic members, and pro-Democratic in seats with departing Republican members). Districts with Republican veterans serve as the reference group. Finally, *ST*₁ . . . *ST*_{k-1} control for any unique state influences on actual vote change.

Because the pre-redistricting vote forms the base from which to compute actual and intended vote change, only seats with identifiable predecessors are analyzed. When opposite-party incumbents thrown together by redistricting face off in November, I always define this base as the vote received by the Democrat and assign the district a value of 1 on *DOPEN* (i.e., treat it like a Democratic open seat, where neither candidate has a net benefit from incumbency). But because new districts originating in a redistricting have no antecedents, they fall outside our purview.

In all regressions contained in Table 5, the intended vote shift has a significant impact on actual change; overall, its coefficient averages .847 in magnitude. Standing out, however, is the way in which actual vote shifts over time have become less responsive to intentions. The mean coefficient has declined from 1.172 over the first three elections to .684 afterward, and furthermore no post-1964 coefficient looms as large as any of

¹⁷No analysis is done for 1958 or 1960, however, because only a handful of districts in each year (all of them in Washington) were redistricted.

Table 5. Measuring the Correspondence between Redistricting Intentions and Actual Electoral Results at the District Level

Year Implemented	Coefficient							Constant	R ²	N
	INVT	DVET	DFR	DOPEN	RFR	ROPEN				
1952	1.497* (.322) .443	.025 (.014) .181	.020 (.025) .073	.047 (.026) .182	-.013 (.016) -.073	.040 (.026) .143	-.102	.344	112	
1962	1.130* (.121) .618	.016 (.011) .109	.064* (.023) .189	.011 (.023) .032	.004 (.017) .016	.023 (.020) .075	-.034	.493	149	
1964	.890* (.079) .889	.038* (.016) .215	.071 (.042) .147	-.090* (.027) -.258	.051 (.029) .147	-	.043	.866	32	
1966	.495* (.139) .278	.051* (.012) .371	.063* (.016) .318	-.074* (.035) -.171	-.088* (.025) -.285	.092* (.041) .176	-.115	.453	116	
1968	.730* (.240) .268	.005 (.020) .026	-.148* (.064) -.193	-.053 (.037) -.127	-.069* (.030) -.204	.096* (.048) .175	.010	.300	125	
1970	.684* (.322) .307	.130* (.051) .444	.181* (.065) .462	.069 (.078) .143	-.032 (.088) -.053	-	-.111	.255	49	
1972	.602* (.112) .331	.047* (.017) .201	.062* (.027) .146	-.017 (.040) -.026	.019 (.028) .041	.100* (.027) .224	-.026	.238	259	
1974	.881* (.235) .547	.087* (.043) .333	.124* (.061) .290	-.151 (.080) -.256	.027 (.060) .062	.080 (.059) .187	.020	.490	41	
1982	.711* (.102) .423	.038* (.016) .164	.075* (.027) .157	-.086* (.029) -.182	-.048* (.022) -.135	.129* (.036) .196	-.009	.418	250	
Averages										
1952-1964	1.172	.026	.052	-.011	.014	.032	-.031	.568		
1966-1982	.684	.060	.060	-.052	-.032	.099	-.039	.359		
1952-1982	.847	.049	.057	-.038	-.017	.080	-.036	.429		

*Significant at .05 level according to two-tailed *t*-test.

Note: State dummy variable coefficients have been omitted to enhance readability. Entries appearing on first line of each row are unstandardized regression coefficients and their associated standard errors (in parentheses); entries on second line are standardized regression coefficients. Equations have been estimated with OLS. ROPEN does not appear in the 1964 or 1970 equations because there were no cases of Republican incumbent retirement there.

those earlier. These findings thus reinforce the inference drawn above from the state-level regressions that whatever the partisan intent of a redistricting scheme, voters have become less inclined to follow the electoral course charted for them.

**The Growth of Incumbents' Electoral
Advantages as a Cause of the Declining
Effectiveness of Partisan Gerrymandering**

What, though, accounts for this increasing voter noncompliance? I suggest that just as with so many other recent changes in electoral behavior (e.g., greater split-ticket voting for Congress and the presidency), the responsible cause can be linked in good part to the rise of incumbents' electoral advantages. Congressional scholars have spent a prodigious amount of time studying how the growing numbers of official perquisites have been applied over two-year terms of office to solicit support from those actually represented. But to my knowledge, no one has ever considered the advantages that perquisite expansion might have afforded, between the time a redistricting law is enacted and the general election, to incumbents seeking votes from nonconstituents residing within the newly attached areas. For example, more reimbursable trips home provide more opportunities to campaign in these new locales during regular weekend district visits. Likewise, a common legislative practice today—facilitated by the expanded numbers of staff—is for members to begin handling casework from their appended territory immediately after redistricting (Ehrenhalt, 1982, p. 1499). Finally, since 1973 members have had the authority to send franked mailings to their new areas (Congressional Mail . . . , 1973, pp. 1024-1026). I expect, therefore, that incumbents nowadays can more rapidly convert constituents-to-be into part of their reelection coalition, possibly upsetting the expectations of a redistricting plan.

Because the county is the smallest unit within a district for which comprehensive election data exist, this expectation must be tested at the county level. The universe of cases thus includes only counties that are shifted intact from one district to another. Incumbents' success in mobilizing constituents-to-be is evaluated by contrasting the electoral impact of transferring counties between incumbents against that produced by transferring counties from incumbent districts to open seats. Even though the party giving up counties will likely suffer a retirement loss in both instances, that is, transplanted residents no longer can vote for the member who had represented them, I still expect that the substitution of a fellow party incumbent running for reelection will minimize these

losses, whereas a successor incumbent of the opposition should make the losses even worse.

The equation to be estimated in each election is:

$$\begin{aligned} ACVT = & a_1 + b_1 DIOP + b_2 RIRI + b_3 RIDI \\ & + b_4 DIDI + b_5 DIRI + b_6 ST_1 \\ & + \dots b_{6+(k-2)} ST_{k-1} + u. \end{aligned} \quad (4)$$

Actual vote change *ACVT* (now computed at the county level) and the state dummy variables *ST*₁ . . . *ST*_{k-1} (needed, as before, to control for state effects) already have been defined. In addition:

DIOP is 1 if the county shifts from a Democratic incumbent district to an open seat district, and 0 otherwise.

RIRI is 1 if the county shifts from a Republican incumbent district to one with another Republican incumbent seeking reelection, and 0 otherwise.

RIDI is 1 if the county shifts from a Republican incumbent district to one with a Democratic incumbent seeking reelection, and 0 otherwise.

DIDI is 1 if the county shifts from a Democratic incumbent district to one with another Democratic incumbent seeking reelection, and 0 otherwise.

DIRI is 1 if the county shifts from a Democratic incumbent district to one with a Republican incumbent seeking reelection, and 0 otherwise.

Counties transferred to open districts from those with Republican incumbents form the reference group.

Once the equations are estimated, $-b_2$ and $b_4 - b_1$ will represent, respectively, the extent to which Republican and Democratic members inheriting territory from fellow party congressmen can limit losses to their party relative to the "pure" retirement losses which instead would result were such territory transferred to a seat without any incumbent. Similarly, $b_1 - b_2$ and b_3 measure how much Republican and Democratic members who inherit counties from opposition officeholders add on to their party's vote relative to the gains that otherwise would occur were these counties shifted to an open seat.¹⁸

So that all comparisons involve an adequate

¹⁸Because *ACVT* is defined in terms of change in the Democratic share of the vote, below-zero values of b_2 and $b_3 - b_1$ support the hypothesis that the post-redistricting Republican vote will be improved if a Republican incumbent, rather than no incumbent, gains the new areas; thus, presenting these two comparisons in the bottom part of Table 6 as $-b_2$ and $b_1 - b_2$ allows positive entries for both parties to be interpreted as consistent with expectations.

number of cases, whenever a particular group of counties transferred from one incumbent to another is smaller than 10 in size, its counties and the corresponding variable (*RIRI*, *RIDI*, *DIDI*, or *DIRI*) are eliminated from the analysis. In addition, because fewer than 10 cases in 1966 existed in the *DIOP* baseline comparison group of counties shifting from Democratic incumbents to open seats, all groups of counties removed from Democratic incumbent districts and their associated variables (*DIOP*, *DIDI*, and *DIRI*) are excluded from the regression. Finally, it is impossible to do any analysis in 1964 (because neither baseline comparison group of counties transferred to open seats contained the minimum number of cases) or in 1958, 1960, 1970, or 1974 (because no group at all had the minimum number).

Table 6 reports the results of the analysis. It is clear that incumbents have become more successful over time in rallying voters in the new counties to their side. As indicated in columns 7-8 of the third row of summary statistics, a party's advantage in having counties transferred from one of its incumbents to another rather than to an open seat district averaged 4.9% in 1952 and 1962; the comparable benefit from 1966-1982 has risen to 7%. Even more dramatic results emerge when I consider incumbents receiving counties taken from the opposition (see row 6, columns 7-8 of summary statistics). These members, who were disproportionately targeted for defeat by hostile redistricting decisions, have been able since 1966 to boost their party's vote by an average of 2.8% more than if the opposition county instead were moved into an open district; before 1966, the corresponding figure of -3.4% signifies that a party's gains actually were larger when opposition counties were thrown into a district with no incumbent than into one of its own. The increased electoral power of incumbency thus seems to have paid dividends even over the relatively brief time available for campaigning in territory not previously represented, better insulating members, as a consequence, from any depredation desired by a partisan gerrymander.

Conclusions

What, then, may be concluded about the concern that prompted this study: whether, as the Republican National Committee so avidly believed in the late 1970s, control of redistricting by a state party leads to partisan gains in its House representation? In general, of course, the answer is affirmative, but since being compelled to adhere to strict standards of equal population districting by the Supreme Court, parties have found it harder to implement successfully partisan gerrymandering. As I showed, the partisan advantage

bestowed by post-1964 schemes has come to depend more on which party's districts before redistricting were relatively underpopulated and less on the identity of the party drawing the lines. Furthermore, whatever the partisan intent of redistrictings, voters have become less inclined to behave as desired by the drafters.

These findings run counter to many predictions during the middle and late 1960s. As argued by Justice White in his dissent to the *Wells v. Rockefeller* and *Kirkpatrick v. Preisler* decisions of 1969, for example, a fixation on eliminating minor disparities among districts

downgrades a restraint on a far greater potential threat to equality of representation, the gerrymander. Legislatures intent on minimizing the representation of selected political or racial groups are invited to ignore political boundaries and compact districts as long as they adhere to population equality among districts. (Baker, 1982, p. 26)

According to my results, that partisan gerrymandering has become *less* pervasive thus suggests that freedom to disregard municipal and county boundaries, even when combined with computer technology able to generate dozens of equal population schemes for partisan benefit appraisal, can only partly compensate for the abolition of past opportunities to concentrate minority party voters in overpopulated districts and for the greater ability of incumbents *today* to work themselves out of an unfavorable redistricting situation.¹⁹

For the future, therefore, the indirect strategy aimed at capture of the House through control of state legislatures seems a less productive use of party funds than directly aiding congressional candidates themselves. Similarly, state legislators might be advised to reconsider their longstanding unwillingness to establish bipartisan commissions with binding redistricting authority. Divesting themselves of the fading potential for achieving gerrymander gains may not be an unreasonable price to pay in order to avoid the squandering of time and frayed inter-party relations that so often have accompanied the redrawing of district lines.

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¹⁹For a similar argument, see Barone and Ujifusa (1982, p. xlii).

Table 6. Support Gained by Redistricted Incumbents in New Areas of Their Districts (County Level)

	Year Implemented					Averages	
	1952	1962	1966	1968	1972	1952-1962	1966-1982
Regression Equations							
DIOP	-.089* (.018) .671	-.145* (.028) -.537	-	-.156* (.027) -.588	-.249* (.030) -.574	-.117	-.232
RIRI	-.058 (.030) -.364	-.033 (.024) -.114	-.059* (.017) -.417	-.091* (.042) -.241	-.082* (.026) -.193	-.046	-.086
RIDI	-	-	.035 (.022) .189	-	.057* (.027) .105	-	.046
DIDI	-.114* (.027) -.819	-.017 (.023) -.074	-	-.157* (.032) -.397	-.189* (.029) -.414	-.066	-.181
DIRI	-.099* (.029) -.460	-.067* (.029) -.251	-	-.148* (.035) -.359	-.283* (.037) -.431	-.083	-.253
Constant	.046	.062	.015	.067	.260	.054	.136
R ²	.303	.408	.322	.587	.602	.356	.559
N	175	153	82	96	295	163	
Summary of regression equation results							
-b ₂	.058	.033	.059	.091	.082	.046	.073
b ₄ -b ₁	-.025	.128	-	-.001	.060	.052	.051
Average of							
-b ₂ and b ₄ -b ₁	.017	.081	.059	.045	.071	.049	.063
b ₁ -b ₅	.010	-.078	-	-.008	.034	-.034	-.001
b ₃	-	-	.035	-	.057	-	.046
Average of							
b ₁ -b ₅ and b ₃	.010	-.078	.035	-.008	.046	-.034	.028
							.007

*Significant at the .05 level according to two-tail t-test.

Note: State dummy variable coefficients have been omitted to enhance readability of table. Entries appearing on first line of each row in presentation of regression equations are unstandardized regression coefficients and their associated standard errors (in parentheses); entries on second line are standardized regression coefficients. Equations have been estimated with OLS.

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Assessing the Partisan Effects of Redistricting

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The purpose of this article is to assess the reality behind the politician's perception that redistricting matters. There are, of course, many dimensions to that perception, because redistricting has many effects. This article focuses on the impact of boundary changes on the partisan composition of seats. In order to do this, it will be necessary to specify what the expected partisan effects of redistricting are and how they can be measured. Thus, I first explain how the impact of redistricting will vary with the strategy of particular plans and then explore some techniques for measuring the partisan impact of boundary changes. I conclude with a detailed analysis of the most important congressional redistricting in 1982—the Burton plan in California.

Most politicians and political insiders believe that redistricting is politically crucial. Although boundary disputes are somewhat esoteric by the standards of normal political discourse, the potential for causing widespread political change by redesigning district lines is great. It is curious, therefore, that the political science evidence about redistricting effects is so undramatic. Early studies indicated that the first reapportionments after *Baker v. Carr* advantaged Democrats, especially in urban areas (Erikson, 1972). However, attempts to link boundary with policy changes uncovered nothing striking (Bicker, 1972; O'Rourke, 1980; Saffel, 1983). Other studies seemed to imply that the major effect of redistricting was to aid incumbents (Mayhew, 1971; Tuft, 1973), but to date there has been very little evidence in support of that thesis either (Bullock, 1975; Ferejohn, 1977). Could it be then that redistricting really does not have any important impact upon the political system?

The purpose of this article is to assess the reality behind the politician's perception that redistricting matters. There are, of course, many dimensions to that perception, because redistricting has many effects. This article focuses on the impact of boundary changes on the political control of districts. Since the impact of redistricting varies with the strategy of particular plans, I begin by specifying what the expected political effects of redistricting will be under different strategies, and how these effects can be measured. Then I explore some techniques for measuring the partisan impact of boundary changes and offer a detailed analysis of a major congressional redistricting plan—the 1981 remapping of the California seats.

A major theme of this article is that redistricting effects are tied closely to incumbency effects. Political scientists have for some time recognized the importance of incumbency in congressional races and the declining relevance of the voter's partisan identification. It should come as no surprise, therefore, that incumbency removal can be more important in determining who wins a redistricted seat than changes in district partisanship per se.

Predicting the Effects of Redistricting

One of the reasons that it has been so difficult to find any systematic or striking redistricting effects is that the types of redistrictings undertaken have varied significantly across states and periods of time. In particular, the way that a plan affects electoral outcomes depends upon the line drawers' strategy and the nature of the demographic constraints they face. As to the first, a redistricting plan can be either partisan or bipartisan in its impact. A partisan effect is one that favors a particular party (usually the majority) over the other, and a bipartisan one favors neither. To be sure, a redistricting plan will have other goals as well, such as the preservation of cities and the protection of minorities, but the political impact is the sole concern of this study.

It is also important to recognize that a plan's effect may be different from its intent. A non-partisan commission might try to ignore partisan considerations, but any plan that it implements will have them nonetheless (Cain, 1984; Dixon, 1968).

Assume that the strategy of a plan is partisan and that the party controlling reapportionment is the one with a majority in both houses of the state legislature, how can the number of majority party seats be maximized, and what will the predicted pattern of changes be? A redistricting strategy has

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two components: partisan reconstruction and incumbency removal. Partisan reconstruction is defined as changes in the balance of party identifiers in a given seat (often measured in terms of party registration in states where such information is available). The aim of partisan reconstruction in this instance is to maximize majority party seats by minimizing its electoral inefficiency to the extent demographically possible. The electoral inefficiency of a particular seat is defined as the amount of excess party support enjoyed by the winning candidate. If there is a registration level r that guarantees that a party will win almost any contest (within some reasonable range of candidate strength), then any level of strength above r is wasted. For example, if the Democrats can win any seat above 60% Democrat in registration, then a 70% seat is inefficient by 10 percentage points. From a partisan gerrymandering point of view, if that excess partisan strength could be traded to a 50% Democratic seat, then the party would have two sure seats instead of one. Classic examples of inefficiently distributed Democratic areas are inner-city minority seats and of inefficiently distributed Republican areas are white, upper-income suburban seats.

Leaving aside for the moment the demographic and bargaining constraints that might obstruct the construction of a partisan gerrymander, what pattern of territorial trades should be observed? To begin with, some number of previously inefficient majority party seats will acquire less favorable territory and experience a drop in partisan strength. To compensate, a certain number of marginal majority party seats will receive favorable areas and so increase their partisan strength. In short, there should be an inverse correlation between the previous level of partisan strength and the reapportionment gain for majority party incumbents.

Just the reverse should apply to minority party seats. The most marginal minority party seats should experience a loss in party strength, and the strongest should experience the gains. Minority party strength is in effect distributed as inefficiently as possible. Hence, the correlation between previous party strength and reapportionment gain should be positive for minority party incumbents.

The second part of a partisan plan is incumbent removal. One common form of this is to parcel the territory of a targeted incumbent into several districts in order to deny the natural advantages of incumbency such as higher name recognition and a good district reputation. By itself, this tactic will not usually be sufficient, since incumbents can use their franking privileges and resource advantages to mail into the new territory to make up some of the difference in the period before the election. Consequently, severe displacement is often used

in tandem with partisan reconstruction to undercut party strength and incumbency advantage simultaneously. A more subtle but equally effective strategy is to use displacement to induce a minority party incumbent to give up a seat that could be won by the majority party in order to run for a neighboring seat that could not be won by the majority party. Some specific examples of this will be discussed later. A partisan plan will attempt to remove, or to induce the removal of, minority party incumbents from as many potentially winnable seats as possible.

The key then to the partisan gerrymander is that incumbents in the party controlling redistricting will be treated differently from those in the party that does not. The average level of electoral safety might actually increase more among incumbents in the noncontrolling party than among those in the controlling party, since greater safety is a by-product of higher electoral inefficiency. If one were to consider the average gain or loss of incumbents by party, one might mistakenly conclude that the noncontrolling party was better off. The point is that many of the individual incumbents in the noncontrolling party will be better off, but if the gerrymander is effective, the party as a whole will be worse off. Indeed, one of the great difficulties for leaders in the noncontrolling party during redistricting is to get individual incumbents to forsake their short-term self-interests (i.e., whether their particular districts are to their liking) for the interest of the party (i.e., whether the plan is good or bad for the party as a whole).

The bipartisan gerrymander is much simpler. In this case, neither party gains an advantage from reapportionment without the consent of the other. Whereas the goal of the partisan gerrymander is to make one party's support more electorally efficient than another's, the object of the bipartisan gerrymander is to protect incumbents in both parties—in short, to make the partisan strength of both parties inefficient wherever there is an incumbent (for this reason, it can also be called an incumbents' gerrymander). From the self-interested perspective of the incumbents, the bipartisan gerrymander has much appeal. Incumbents who want to get stronger will seek to dispose of their least desirable areas. Because one party's undesirables are usually the other's most loyal supporters, Democrats will trade Republicans to Republican incumbents, and Republicans will trade Democrats to Democratic incumbents. Because incumbents tend to be risk averse—no margin of safety is ever too much—the result is greater electoral inefficiency and more noncompetitive seats.

In the bipartisan gerrymander, no incumbent who wants to return will be forced, unless demo-

graphically necessary, to run against any other incumbent. Moreover, the pattern of incumbency removal should be unbiased. If incumbents must be removed for demographic reasons, the burden will be more or less evenly shared under a bipartisan plan.

The strategies of partisan and bipartisan plans as outlined will not necessarily be implemented as they are intended. Various considerations will compromise the best laid plans of reapportioning men. To start with, population needs will constrain the set of feasible trades. It will, for instance, be easier to make a trade when one of the two adjoining seats is overpopulated and the other underpopulated than it will be when both are overpopulated or underpopulated. Trades between seats with noncomplementary population needs only compound initial population deficits and surpluses and cause more difficult adjustment problems in the rest of the state. Secondly, although trades between members of different parties can often be complementary because both want the other's weakest areas, trades between members of the same party will often be conflictual because both will want each other's strongest areas. This means that some strong incumbents will resist sharing their "wealth" with weaker members of their own party, further distorting the logic of the plan. Finally, there are the idiosyncratic concerns of incumbents. Incumbents will in many instances forego the partisan advantages of trades in order to keep amusement parks, fund-raising locations, favorite donors, their residences, and the like in their districts. So even if partisan malice is in the hearts and minds of the line drawers, the pure patterns of the partisan and bipartisan gerrymanders will be blurred by the noise of bargaining and demographic constraints.

Measuring the Partisan Effects of Reapportionment

Having considered the expected patterns of change associated with various types of redistricting strategies, the question is whether or not it is possible to measure the specific effects of various plans in order to determine whether a given plan is partisan or bipartisan in its impact.¹ The tech-

¹There are several alternative ways to measure the political effects of reapportionment. The simplest class of methods compare district registrations or vote totals before and after the territorial changes caused by redistricting. For example, in states where the registration figures are published, it is possible to determine whether and by what amount the Democratic or Republican registration increased:

$$r_{d,o} - r_{d,n} \quad (1)$$

where $r_{d,o}$ and $r_{d,n}$ are the Democratic registrations in the old and new districts

Another popular method is to take the vote totals for candidate j in the last election, subtract the votes j won in the areas j loses in reapportionment, and add the votes for candidate k who ran for the same legislative office in the same election in the areas that have been transferred from k to j :

$$v_{j,n} = v_{j,o} - v_{j,l} + v_{k,a} \quad (2)$$

where

$v_{j,n}$ is the predicted vote for candidate j in the new district

$v_{j,o}$ is the vote for candidate j in the old district

$v_{j,l}$ is the vote for candidate j in the lost areas

$v_{k,a}$ is the vote for candidate k in the newly added areas where candidates j and k ran for the same legislative office in different districts in some year before redistricting.

Finally, where the data are available, it is instructive to compare the totals received by some statewide candidate under the various proposed boundary changes.

$$v_{s,n} = v_{s,o} - v_{s,l} + v_{s,a} \quad (3)$$

where

$v_{s,n}$ is the vote received by a state wide candidate in the new district

o , l and a have the meanings previously defined.

All of these methods have their particular flaws, but more generally, the difficulty with this class of methods is that it does not fully and efficiently use all the available information. For instance, two districts with the same Democratic registrations might have different Republican or minority party registrations. Moreover, since redistricting affects incumbency status as well as the underlying partisan strength of a district, merely looking at the registration figures does not give an accurate estimate of the political impact of a proposed plan.

The second class of methods, therefore, tries to eliminate this flaw by utilizing a multivariate estimation procedure to combine several pieces of information. One such technique, for instance, is to develop an expected vote model in which a candidate's vote at time t is regressed on various demographic data and on a statewide candidate's vote. This technique yields a set of estimated parameters that can be multiplied by the post redistricting political and demographic data to yield new district totals:

$$v_p = a + BZ_p + c_1s_p + u \quad (4)$$

where

v_p is the vote for relevant district race in precinct p

B is a vector of coefficients

Z is a vector of demographic variables for precinct p

c_1 is a coefficient

s_p is the vote for a statewide candidate running in the same election in precinct p

u is the error term.

This is a particularly useful technique for redistricting

nique developed for the present analysis is to try to estimate the probabilities of the Democrats and Republicans winning various seats, given information about changes in registration and incumbency status as a result of the plan. The model is thus:

$$Pr(v_j = 1) = F(a + BR_j + c_1d + c_2r) \quad (5)$$

where

$Pr(v_j = 1)$ is the probability of a Democrat winning congressional seat j

R is a vector of registration data for various parties in seat j

d is a dummy for a Democratic incumbent in seat j

r is a dummy for a Republican incumbent in seat j

B is a vector of coefficients

c_1, c_2 are coefficients relating the incumbency dummies to the vote

The model is estimated with a probit procedure using the registration, incumbency, and outcome data from the 1980 election that preceded the 1981 reapportionment in California. The new registration and incumbency data resulting from the new boundaries are then inserted into the estimated equation, yielding probit scores that can be converted into probability estimates.²

negotiations because it tells an incumbent how he or she specifically would have run in the proposed new district in an election at time t . However, its advantage as a bargaining tool is also its liability as a method for analyzing the general partisan impact of a plan: it is highly candidate specific in its predictions and does not provide a convenient basis for comparing results in open seats with results in seats with incumbents.

²The virtue of this model is that it provides a nice, out-of-sample predictive procedure for assessing the political effects of a redistricting plan. Its chief limitation is that it cannot measure the effect of displacement upon incumbency. In other words, incumbents who acquire a lot of new territory might have less incumbency advantage than the fortunate few who retain their old seats intact. To estimate gradations in the incumbency advantage would require abandoning the out-of-sample framework. As it was, only one incumbent who ran for reelection in 1982 lost his seat (Clausen), and his displacement was not great. The primary displacement effect is on the cost of reelection, since its electoral significance is mitigated by the incumbent's ability to mail into and get acquainted with the new areas a full year before the election. In the model as specified, displacement and the removal of the incumbent's home are part of an implicit equation that influences the incumbent's decision to run for a given seat.

The actual estimated parameters were as follows:

$$\begin{aligned} Pr(v_j = 1) = & -9.43 + .016Demreg \\ & (.004) \\ & - .017Aipreg + .007Libreg - .045Pfreg \\ & (.083) \quad (.036) \quad (.136) \\ & + .015Dec + .822Dinc - 1.60Rinc \quad (6) \\ & (.012) \quad (.460) \quad (.55) \\ R^2 = .83 \quad \text{Chi square} = 32 \end{aligned}$$

where

Demreg is the percentage Democratic registration
Aipreg is the percentage American Independent party registration

Libreg is the percentage Libertarian party registration

Pfreg is the Peace and Freedom party registration

Dec is the Decline to State (i.e., Independent)

Dinc is the dummy for Democratic incumbent

Rinc is the dummy for Republican incumbent.

The signs of the estimated coefficients on the incumbency and Democratic registration variables are significant and in the proper direction. The minor party coefficients are not, but are left in since they improve the fit marginally. The purpose of this model is predictive and not structural. Clearly, the large estimated incumbency effect is picking up a variety of phenomena related to holding office—for example, spending advantages and resource advantages. The point is to show what the effects of partisan reconstruction and incumbency removal are, not to show the causal routes that lead from incumbency or registration to electoral advantage. The equation is in this sense the most parsimonious reduced form.

The pre-redistricting probabilities referred to in the ensuing discussions are obtained from these estimated parameters by inserting the pre-redistricting registration and incumbency data into the model, taking the predicted score and converting it into a probability number. The post-redistricting probabilities are obtained in the same way using the same estimated parameters and the post-redistricting registration and incumbency data.

Assessing the Burton Plan

The 1981 California congressional redistricting was one of the most important and controversial redistricting plans in the country. Its significance lies partly in the size of the California congressional delegation, which grew in 1982 from 43 to 45, but also in the intense partisan battle it

touched off. The plan was authorized by Phil Burton with the technical assistance of Michael Berman—a brother of an assemblyman who won one of the newly created Los Angeles congressional seats in 1982—and Leroy Hardy, a political scientist at Long Beach State who had worked on redistrictings since the sixties. The California delegation had been split 22-21 after the 1980 election and before the redistricting. In 1982, the Democrats held 28 seats and the Republicans held 17, a dramatic shift in power that many Republicans attributed to redistricting. This plan—Burton I—was subsequently rejected by the voters in a Republican sponsored referendum and was replaced in 1982 with a new plan—Burton II. My remarks are directed solely to the now-defunct Burton I plan.

I examine this plan utilizing the framework of expectations discussed earlier to test whether it had the pattern of a partisan strategy. Applying those propositions to California, we get the following:

- 1) Some number of marginal Democratic seats should have been strengthened.
- 2) Some number of marginal Republicans should have been weakened.
- 3) Some number of strong Democrats should have been weakened to assist marginal Democrats.
- 4) Some number of strong Republicans should have been made even stronger.

The question is, do these expected patterns appear in the data? The evidence for these patterns will consist of 1) simple registration data, 2) the estimated probabilities of a Democrat winning the seat under the assumption that all the seats are open, and 3) the estimated probabilities given information about which incumbents actually ran in 1982 and which seats were open.

The first sign of a partisan plan is that some number of marginal seats in the controlling party should have been strengthened. Table 1 shows the four Democratic incumbents who gained the most from the Burton plan. The first is Phil Burton's brother, John, who represented a district in Marin and areas to the north of San Francisco. Burton had received a strong challenge in the 1980 election and the 52.5% registration in his district was by California standards marginal for a Democrat. Typically, the seats with the highest probability of changing hands fall into the 50 to 55% Democratic registration category, and so it was clear that without assistance, Burton's district would remain marginal throughout the eighties. The solution to Burton's electoral insecurity was a highly controversial district that meandered from Vallejo in Solano County, across the water to

Marin, through a narrow corridor on the east side of the San Francisco County, and down into Daly City in San Mateo County. This, more than any of Burton's other districts, brought a great deal of criticism from the press and the public.

The effect of this contorted district was to increase Burton's Democratic registration by about five points to 57.5%. The estimated probability of a Democrat winning John Burton's 5th district in an open race was 83% in 1980. After reapportionment, it was 91%. Adding in the effect of incumbency, the model projects that John Burton, had he run for reelection, would have been elected with a 99% probability, up three points from 1980.

As said before, it is important to note the importance of an incumbent running, a fact that has been much heralded in recent political science research. This analysis clearly shows that the displacement of incumbents is even more important to the outcome of the first post-redistricting election than are any changes in the underlying partisan composition caused by redistricting. (This is quite evident from the size of the estimated coefficients in Table 6.) Democratic incumbency is "worth" an additional 51% Democratic registration (.822/.016), whereas Republican incumbency has the advantage equivalent to a 100% shift in registration (-1.60/.016). The value of incumbency, particularly to Republicans, is clearly enormous.

Many of the changes made in John Burton's seat in 1981 were taken back in 1982. The 1981 plan was rejected by the voters in a June, 1982 referendum, and new lines were redrawn in December. When John Burton chose not to contest the seat in 1982, it was won by Barbara Boxer. In the subsequent redistricting, Boxer's district was neatly shaped into a more marginal seat.

The only other Democrat to receive a boost in 1981 comparable to John Burton's 5th district was George Brown's 36th district. The Mineta and Panetta seats, by comparison, got almost trivial increases that really did not improve their marginal status much. So one can say that in two instances primarily, marginal Democrats were strengthened by the redistricting plan, whereas in the other instances, including some that are not included in this table, the changes were insignificant and did not change the status of the seat.

The second expectation of a partisan plan is that some number of marginal noncontrolling party incumbents—in this case, Republicans—should have been partisanly weakened by the redistricting plan, which appears to be where the Burton plan had its major effect. In several instances, the strategy followed was more subtle than a straight collapse of the Republican incumbent's seat. Rather, the best Democratic portions

Table 1. California Democratic Incumbents Who Benefitted the Most from 1981 Reapportionment (%)

Congressional District	Incumbent	Probability					
		Registration		Open		Democratic Incumbent	
		1980	1982	1980	1982	1980	1982
5th	Burton	52.5	57.5	83	91	96	99
36th	Brown	51.4	57.7	73	81	92	95
13th	Mineta	49.7	51.5	48	63	78	87
16th	Panetta	49.2	50.0	39	48	71	78

were retained in the old district while the most Republican areas were used to create a new seat for the Republican incumbent. By inducing the Republican incumbent to run for the new seat, Burton was able to create an open seat with favorable registration for the Democrats. This was essentially the procedure used in the Hunter and Feidler seats. Both of these incumbents were sitting in seats with dangerously high Democratic registrations, and so it did not take a great deal of inducement—for example, putting their house in the new district—to get them to move into the safer seat. A glance at Table 2 shows that the partisan composition changed slightly in the case of the old Hunter seat and negatively in the case of the old Feidler seat: the key to winning both seats was the removal of the incumbent, which, as the data show, dramatically altered the chances of a Democrat winning in both instances.

The old Dornan seat is a good example of a district created by both partisan reconstruction and incumbent removal. Dornan, the Republican incumbent, did not have to be given an alternative seat to run in, because he had declared himself a candidate for statewide office. Since the seat was strengthened by 9 points in registration and no longer had an incumbent, it changed from one in which the Democrat had a 1% chance of winning to one in which he or she had a 95% chance. The old Rousselot seat was also dismantled, and he

was given no alternative open seat to run in. Portions of his old district were parceled off to various surrounding Republicans, but none of the portions was sufficiently large to give Rousselot a base from which to run. The largest overlap between his old district and the Burton-created districts was the highly Hispanic 33rd, previously represented by George Danielson and then by Marty Martinez after a special election in July, 1982. Rousselot chose to contest the Democrat, Martinez, as a nonincumbent rather than face his Republican colleagues in an expensive primary, and was defeated in the November, 1982 election (Cain & Kiewiet, 1984).

The other two gains by the Democrats in 1982 did not involve the weakening of Republican seats. The 18th congressional district was a newly created central valley district made possible by the allocation of two new districts to California and the rapid population growth in that area. As Table 2 shows, the Clausen seat did not change much in the redistricting plan, and the gain by the Democrats seems to have been the result of the challenger's strength and popularity in the area. So five of the six gains appear to have been reapportionment related, and four of those five involved the forced or induced removal of Republican incumbents.

Although certain Democratic incumbents benefited from the redistricting in 1981, not all of them

Table 2. Gains by Democrats in Congress (%)

Congressional District	Incumbent 1980	Probability					
		Registration		Open		Incumbent	
		1980	1982	1980	1982	Republican 1980	Democratic 1982
2nd	Clausen	51.6	51.2	54	53	7	81
34th	Rousselot	44.8	65.1	8	96	0	99
18th	New Seat	—	61.3	—	87	—	98
44th	Hunter	54.9	58.0	80	91	23	98
26th	Fiedler/Goldwater	60.3	59.3	85	82	28	96
27th	Dornan	46.8	55.9	25	79	1	95

did. In particular, a few had to give up prime areas or had to take unfavorable areas because they were underpopulated. As a result, some Democrats were made worse off by the Burton plan, including Phil Burton himself. Burton's seat, the 6th, gave up some of the "best" areas in San Francisco County to help boost his brother's seat. Indeed, when the Republicans ran a popular moderate Republican state senator against him in the November, 1982 election, there was an enormous amount of speculation in the California press that Burton might have been too cute and left himself vulnerable to a challenge. My model indicates otherwise. The probability of Burton losing was unaffected by redistricting. Given that the seat had a 62.8% Democratic registration, a large, liberal Independent vote and a well-known incumbent, the sacrifice that he made was by no means extravagant.

In fact, one of the most striking things about Table 3 is the high degree of electoral security enjoyed by all the Democratic "martyrs." All had Democratic registrations above 55%, and with the added advantage of incumbency, they all had a greater than a 95% chance of being reelected even after their districts were altered. Nonetheless, redistricting did affect the result in these seats in the subsequent election. Even though 1982 was a more favorable year for Democrats than 1980, all of them suffered a drop in their margin of victory.

The partisan reconstruction of these seats (holding constant the incumbency status) can be viewed in the aggregate as they are in Figures 1 and 2. The vertical axis of these charts shows the computed probability of a Democrat winning the seat in an open race in 1982 and the horizontal axis shows the corresponding probability in 1980. The line at the 45-degree angle indicates points of no change: that is, where the probabilities in 1980 and 1982 were the same. Points above the line indicate seats that were made more Democratic by redistricting and those below it were made less

Democratic. The data are stratified by the party of the incumbent in 1980 so that Figure 1 displays the data for the Democratic seats and Figure 2 the data for Republican seats.

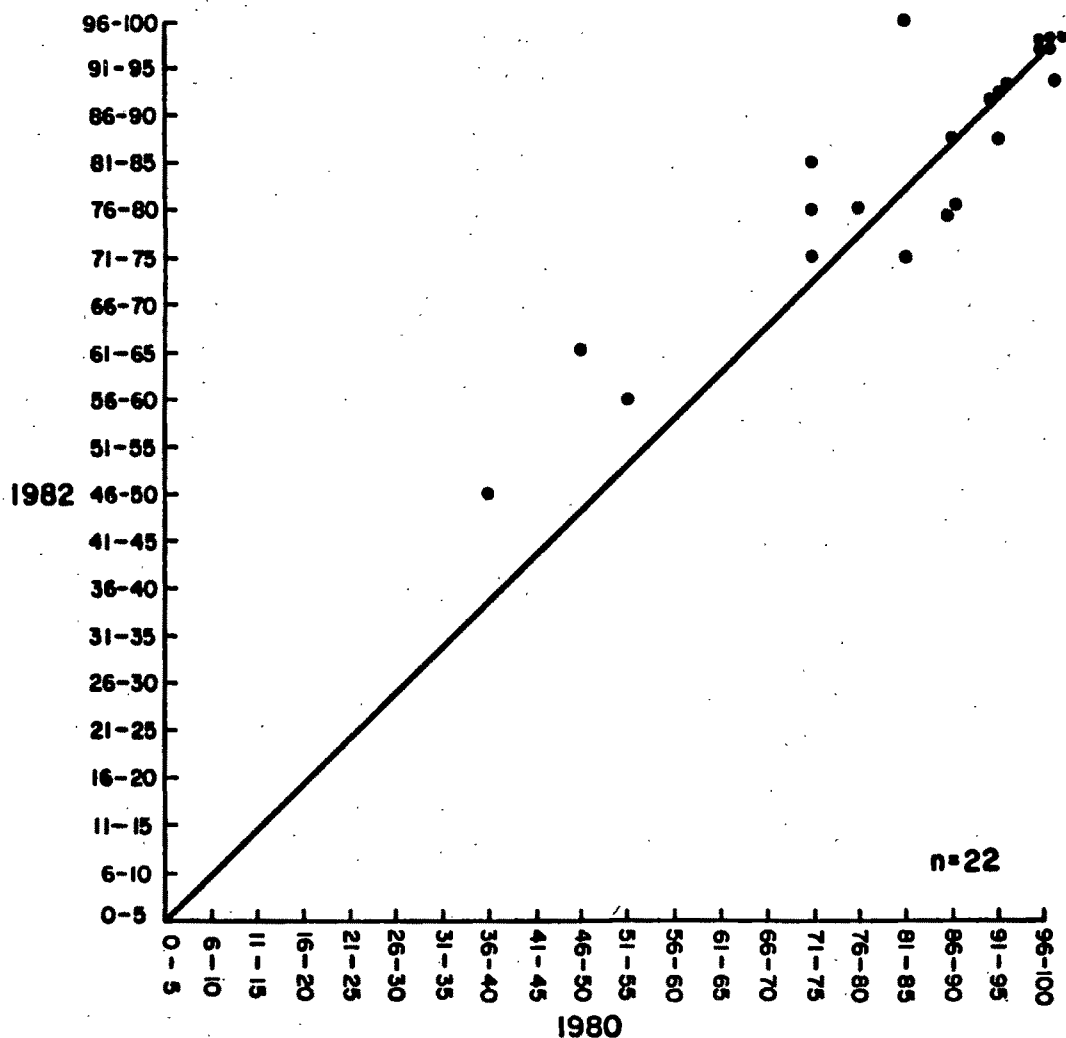
Translating the expectations of a partisan plan as discussed earlier into predicted points on the graph, the pattern in the Democratic seats should be that: 1) some points in the upper right-hand corner, representing the safest Democratic seats in 1980, should fall below the line since they are sharing their partisan wealth in the interests of greater Democratic efficiency; 2) some points in the lower left-hand corner, representing the most marginal Democratic seats, should fall above the line because they would be the natural beneficiaries of Burton's largesse; and 3) most incumbents should stay pretty close to the line because demographic, bargaining, and geographical constraints put severe limits on partisan efficiency. A perusal of Figure 1 would seem to confirm our expectations. The four points furthest above the lines are those discussed in Table 1.

Figure 2 is no less revealing. Once again, our expectations are that: 1) some number of points in the upper right-hand corner, representing the most marginal Republicans, should fall above the line because they are the natural candidates for partisan conversion; 2) some number of those in the lower left-hand corner should fall below the line because the Democrats would like them to be as inefficiently strong as possible; and 3) most points, once again, should cluster fairly close to the line because of demographic, bargaining, and geographical constraints. The data do not conform quite as closely in Figure 2 as they do in Figure 1. To begin with, the three points above the line are scattered across the horizontal axis, implying that the Democrats did not simply target the weakest seats. However, the reader should note that all the points above the 51% category on the vertical axis were won by the Democrats, including all but one of the points to the right of the 51% category on the horizontal axis. In short, the

Table 3. The Democratic "Martyrs" (%)

	Incumbent	Probability							
		Registration		Open		Democratic Incumbent		Margin	
		1980	1982	1980	1982	1980	1982	1980	1982
30th	Danielson/ Martinez	69.8	62.8	99	93	100	99	48	8
4th	Fazio	58.5	56.1	86	79	97	95	44	28
6th	Burton	62.8	60.7	99	97	100	100	44	18
23rd	Beilenson	59.6	57.1	88	79	98	95	32	20
32nd	Anderson	62.2	60.3	92	87	99	97	35	18

Figure 1. Changes in Democratic Seats (Congress)



Democrats won all the marginal seats even without changing the composition of some. The reason, which will be seen even more graphically in a moment, is that the Democrats made effective use of incumbent displacement: that is, they kept the registration the same, but moved the incumbent out in order to open up the seat. The three seats that fall above the line in Figure 2 are the Hunter (extreme right), Dornan (middle), and Rousselot (extreme left) seats.

The increased inefficiency of the Republican seats as a result of Burton I is evident in the cluster of points below the line in the lower left-hand corner. These are seats that are already strongly Republican and are made even more so by the plan. Notice also that the deviations from the line

are somewhat larger, reflecting the likelihood that Burton felt more constrained by the wishes of his fellow Democrats than by those of the Republicans. This can be taken as support for the position I have argued elsewhere that the risk averse, idiosyncratic preferences of legislators form a moderating influence on partisan designs (Cain, 1984). One suspects that because Burton felt a greater need to accommodate the Democratic incumbents, this inertial force minimized changes in their districts to some degree.

Reapportionment and Electoral Competition

There has been a great deal of academic and popular discussion in recent years about the

decline of competition in congressional races (Ferejohn, 1977; Fiorina, 1977a, b; Mayhew, 1974). One particular aspect of this debate is whether redistricting has contributed to the decline of competition in congressional races. Ferejohn and others have expressed doubts about this, and as the hypothesis is stated, these doubts are correct. If the question is whether all incumbents are indiscriminately aided by reapportionment, the answer is, not in all states, and maybe not all incumbents in any state. Not in all states, because some states will have more partisan plans than others; not all incumbents, because geographical, personal, and idiosyncratic considerations will sometimes be more important. However, the hypothesis that reapportionment affects electoral competition may still be accurate

in the sense that how it affects electoral competition will vary with the intent of the plan as well as the degree to which geographical, personal, and idiosyncratic considerations introduce random noise into the final outcome.

Reapportionment affects electoral competition in two ways. First, it helps to determine the odds of a Democrat or Republican winning by restructuring the underlying partisan composition of a seat (i.e., partisan reconstruction). Second, it affects the incumbency factor by removing or keeping incumbents in their territory. The model developed earlier can be used to illustrate both of these effects separately and conjointly. Much of the dialogue about the decline of competition begins with the so-called Mayhew diagrams, which are histograms that display the electoral

Figure 2. Changes in Republican Seats (Congress)

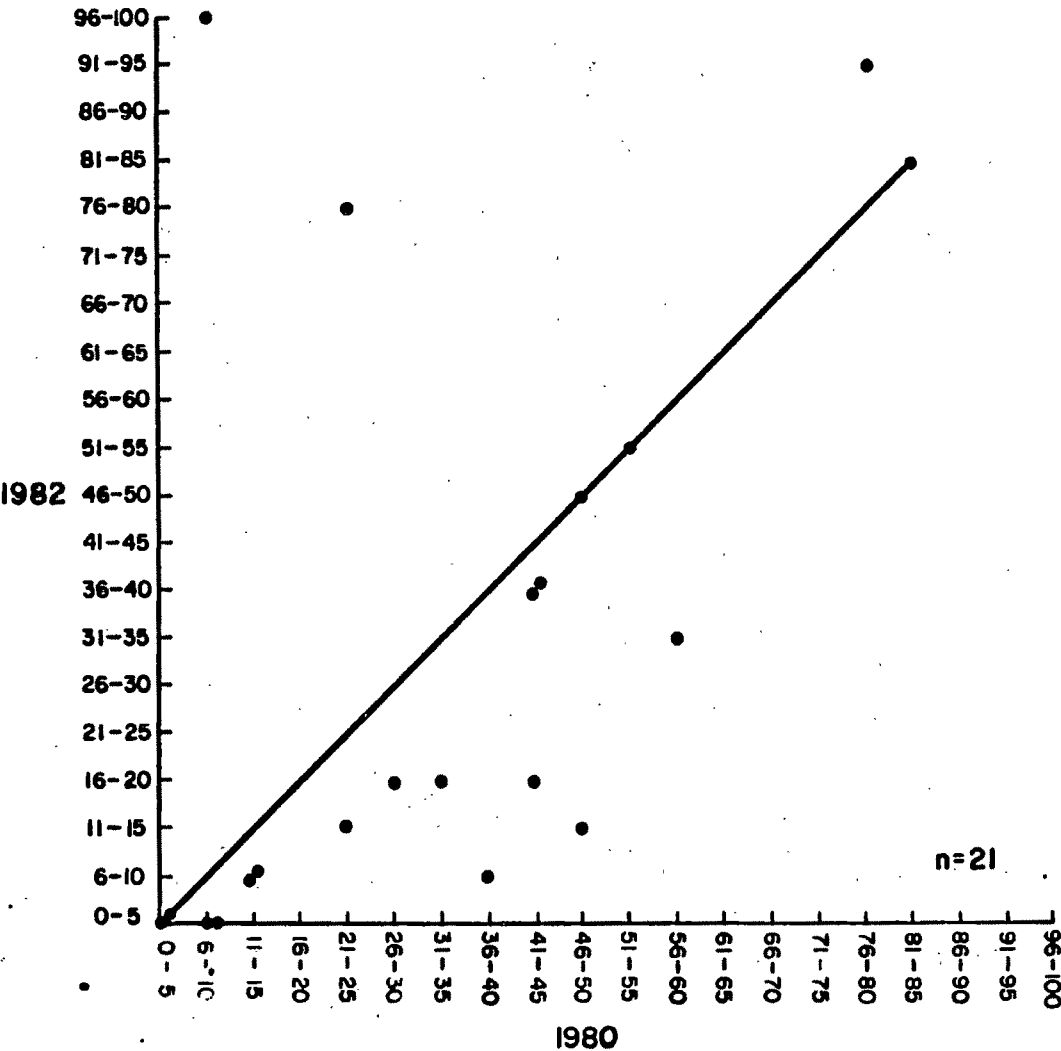
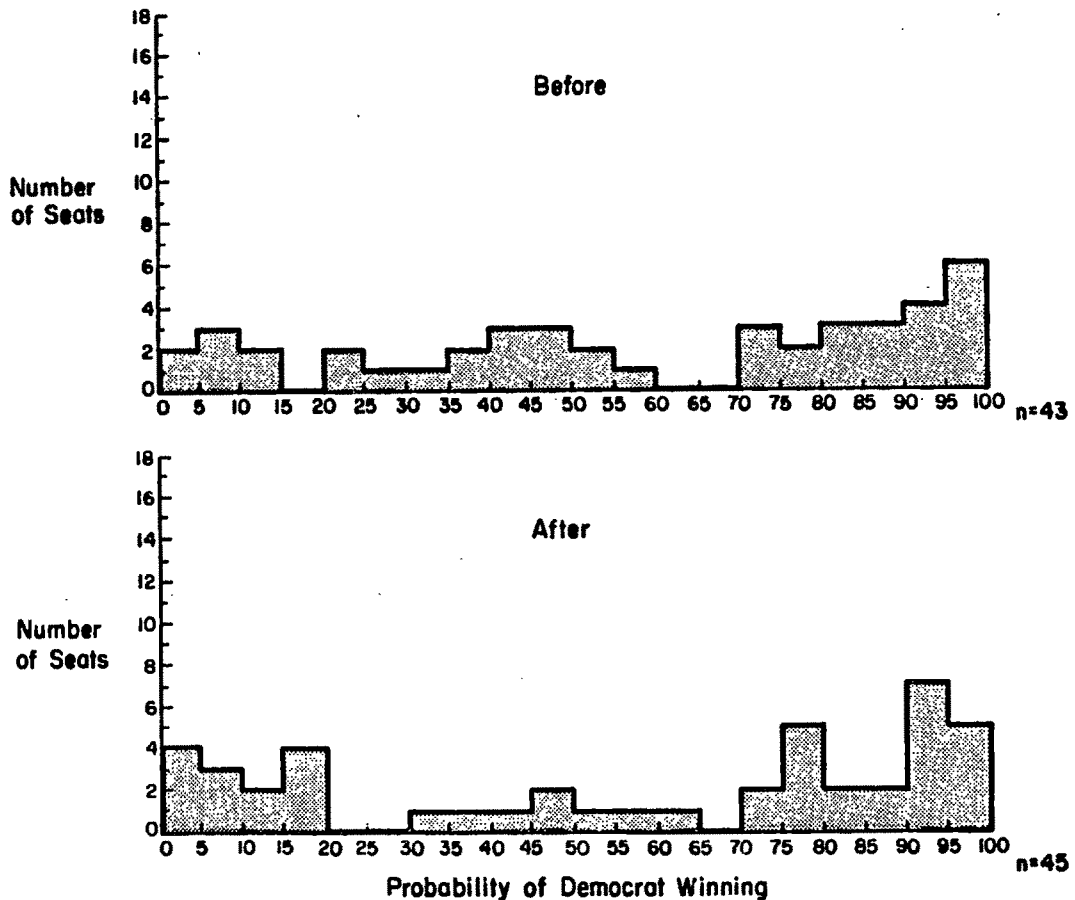


Figure 3. Comparison of Congressional Seats Before and After Reapportionment (Open)



margins of incumbents at various intervals during the postwar period. A variant of this idea is to create a histogram of the estimated probabilities from the probit model and show what happens to electoral competition at various stages in the reapportionment process. This of course leaves unanswered questions about the duration of reapportionment effects and the role that it may have played in the overall trend toward declining competition, but it does at least give us a glimpse of the immediate impact in one state at one period of time.

First, consider the impact of partisan reconstruction. Redistricting changes the competitiveness of seats by increasing the Democratic registration in seats that lean Democratic and the Republican registration in seats that lean Republican. Figure 3 shows the effect of the Burton I on all 45 seats under the assumption that no incumbents would be allowed to run. As the figure demonstrates, the consequence is some visible shrinking of the distribution in the middle. How-

ever, the results are not dramatic. There are still some seats left in the most competitive range and the rest are not simply bunched on the ends. Geographic constraints—for example, not being able to use inefficient inner-city Democratic strength to help out weaker Democratic seats in the rural and suburban areas—and the desire of incumbents to minimize displacement—that is, the acquisition of new constituents and the loss of former ones—explains why we do not observe more radical partisan reconstruction.

What about the separate effect of removing the incumbent? This is shown in Figure 4, which compares the distribution of seat safety in 1980 under the assumption that the seats were all open and versus the assumption that all incumbents ran. Here the effect of incumbency on the distribution in the most competitive, middle range is striking. Large numbers of seats cluster on the ends of the distribution, and no seats fall in the 50% range. Of course the reader should bear in mind that the model assumes the average incumbent, whereas in

reality there will be enormous variation in the strength of both the incumbent and challenger. To some extent, this may be better modeled with campaign expenditure data, but the quality of the candidates will in any case remain difficult to capture.

The next three figures show the progression of changes in the distribution brought about by redistricting, including both incumbency removal and partisan reconstruction. Figure 5 compares the distribution of seat safety right after the 1980 election and then after the 1981 reapportionment. The post-reapportionment distribution assumes that the incumbents who held the seats in 1980 would run in what most closely approximated their old seat in 1982. Thus, for example, it was assumed that Dornan would run again in the 27th. Even with this strong assumption, the distribution

has been changed some by movement to the extremes on both sides of the distribution. However, as was discussed before, the redistricting plan induced some incumbents to abandon their old seats to run for new ones and caused others to lose in the November election. With the new incumbents in place, the situation displayed in Figure 6 shows the almost perfect inefficiency of the Republicans. Even though the Democrats were less clumped on the end of the distribution, only a few of their seats were left in the 75% to 90% area. In short, most incumbents in both parties are in safe positions, but the Democrats are somewhat less inefficiently distributed than the Republicans.

What then has been the total change from 1980 to 1982? The last figure compares the two distributions. The answer would appear to be that

Figure 4. Distribution of Congressional Seats With and Without Incumbents

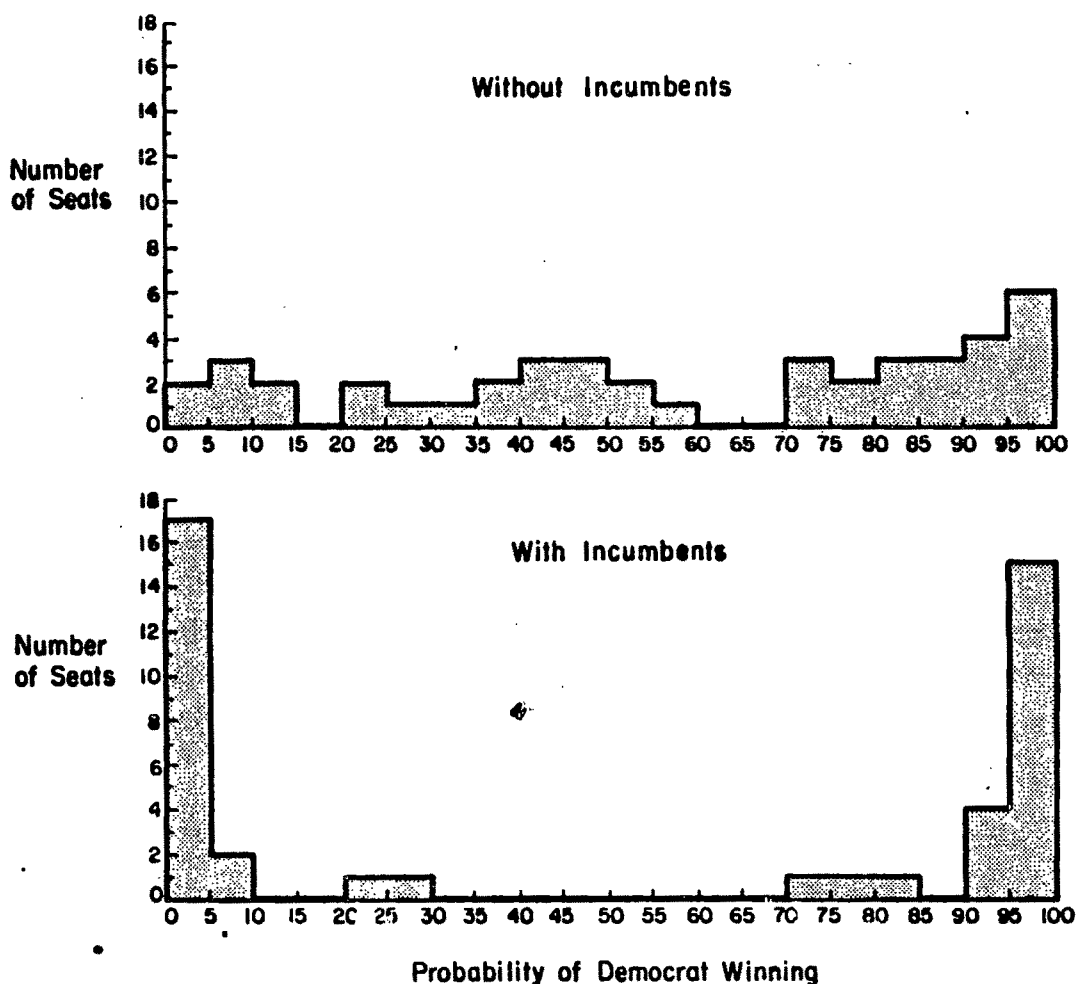
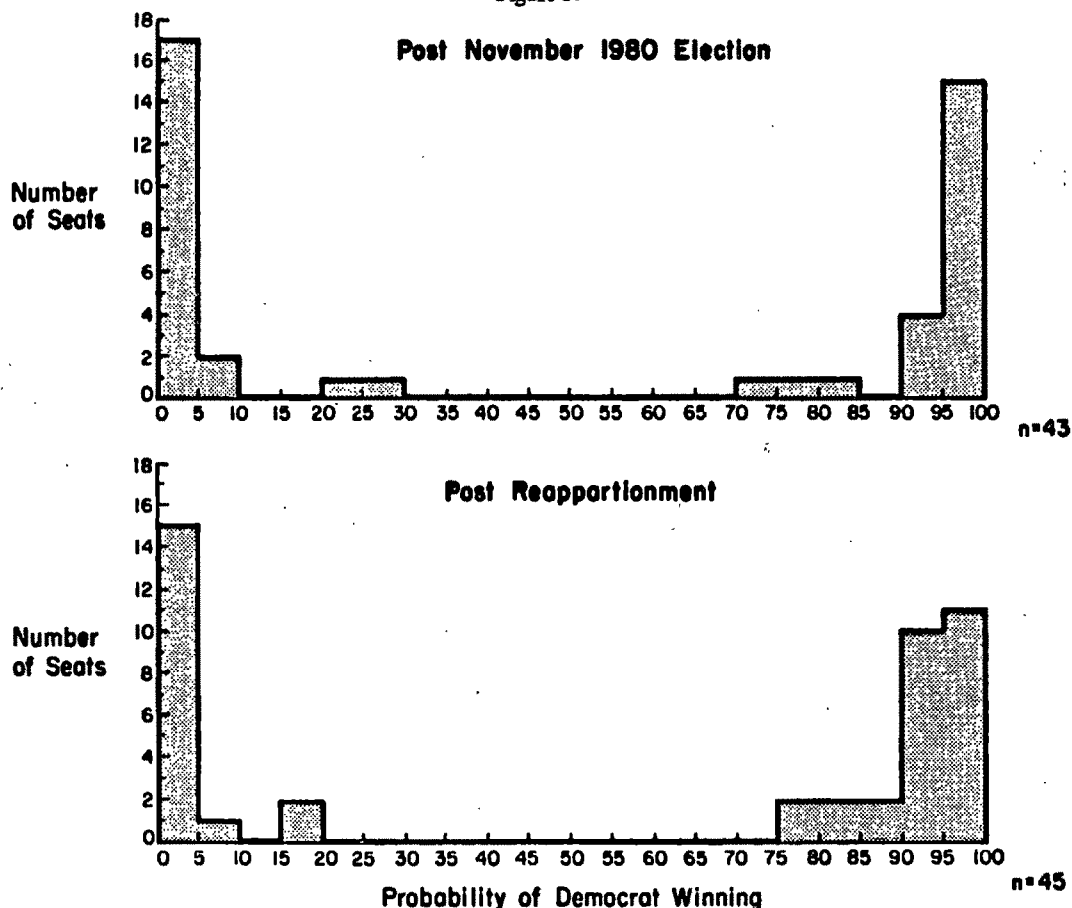


Figure 5.



the combination of partisan reconstruction and the artful removal of inconveniently placed incumbents can alter the seat distribution and make the majority party more efficiently distributed than the minority party. In the case of California, it was enough to help swing five seats to the Democrats.

Conclusion

Are the partisan effects of redistricting important? The answer would seem to be that they are. By changing the partisan composition in a district and removing or retaining the incumbent's base, a reapportionment plan can alter the odds of a party winning a particular seat. The key to a partisan plan is not simply increasing the average margin of victory or even the underlying partisan strength of all majority party legislators. Rather, the key is increasing the efficiency of majority party strength, which will mean a redistribution of electoral strength for the purpose of maximizing

the number of winnable seats. Some majority incumbents will get stronger and others weaker in inverse relation to their initial vulnerability. Simply looking at the average registration or vote margin may be misleading.

A second conclusion from this research is that a proper assessment of the partisan effects of redistricting cannot overlook its impact on incumbency. To be sure, the post-redistricting election will introduce a new set of incumbents who will presumably also enjoy the electoral advantages of holding office. However, the temporary scrambling of incumbents can have momentous importance for the election that follows the redistricting. This should not be too surprising to political scientists since it seems logical that in an era when party loyalty counts for less and incumbency counts for more, redistricting tactics should include incumbent considerations. Indeed, if recent trends toward independence from the parties continue, redistrictings in the future could come to focus more on displacement issues and less on the partisan makeup of districts.

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Figure 6.

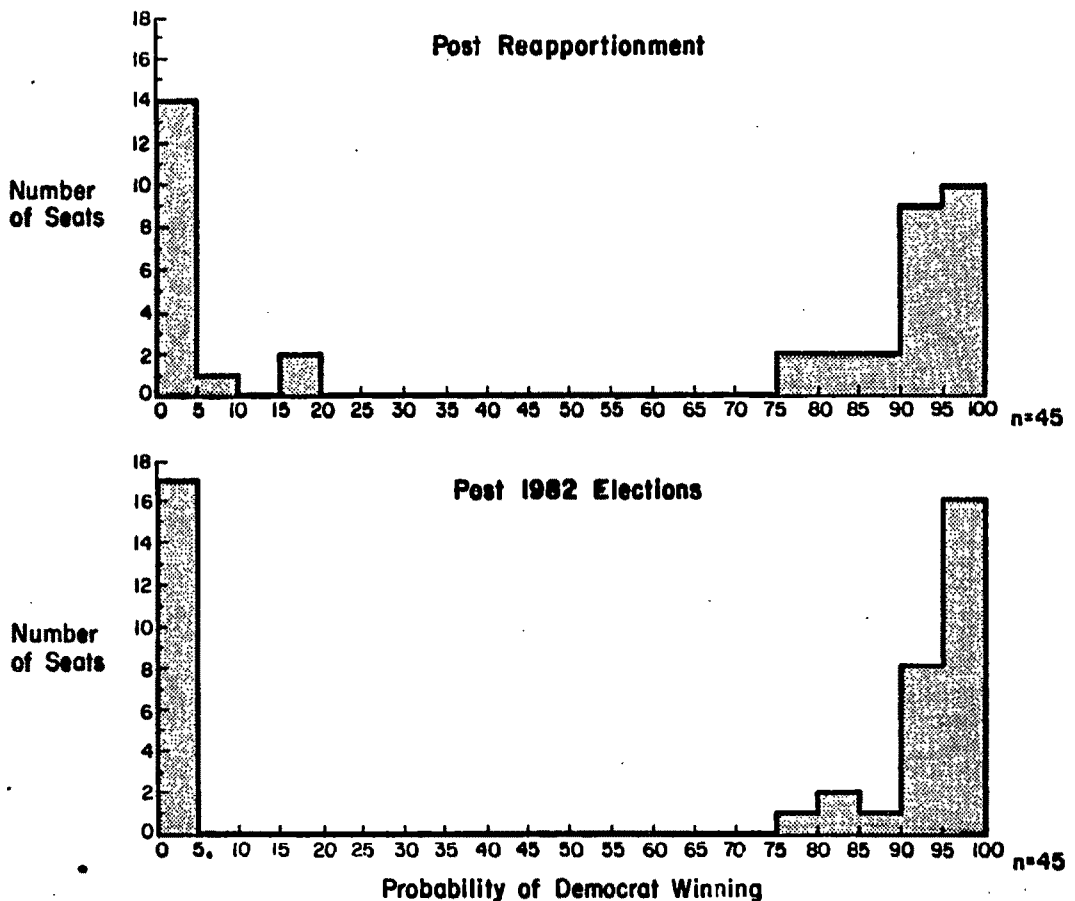
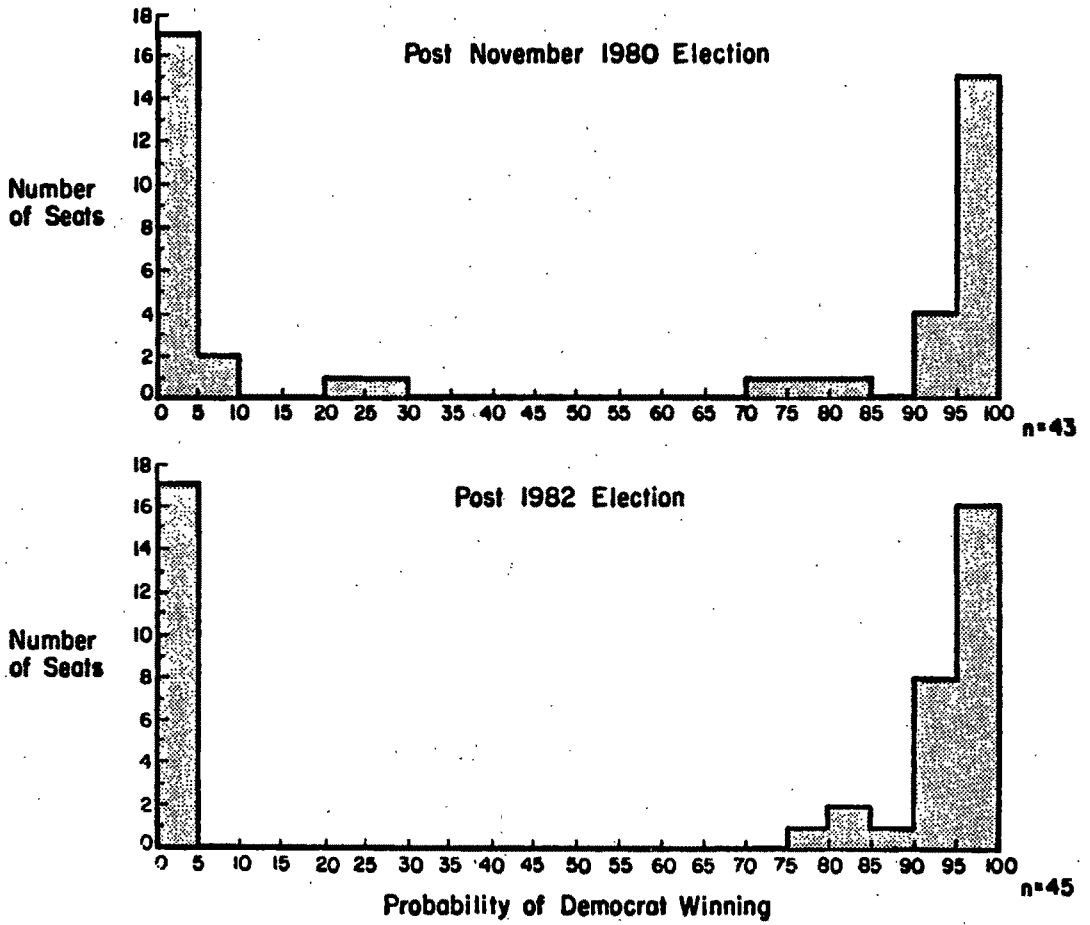


Figure 7. Comparison of Congressional Seats



Promise and Performance

A Dynamic Model of Presidential Popularity

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The growth of public opinion measurement in the last 40 years has added a new dimension to the study of presidential behavior. Not only have public evaluations become more newsworthy, but the importance of public support as a resource and determinant of political survival has been enhanced. Recent scholarship on the presidency has documented the value of public support, attempted to identify its major determinants, and speculated about the manner in which presidents might influence these evaluations.

This research is designed to integrate these concerns into a single model and thereby to examine the interdependence between public support as a product of citizen decisions and as a political resource. First, a characterization of the citizen as an evaluator of the president is developed and used to construct an equation of presidential approval. Next, we develop an equation that explains presidential effectiveness in the legislative arena and illustrates the operation of public support as a presidential resource. The public support and legislative effectiveness equations are specified as a simultaneous equation system, estimated, and evaluated. The results of the model are then used to expand the conventional wisdom about the determinants of public support, to examine the consequences of the reciprocal relationship between public support and legislative success, and to generate ex post forecasts of President Reagan's support from 1981 through 1983.

Unlike their predecessors, occupants of the Oval Office since Eisenhower have been subject to almost continuous evaluation via the public opinion poll. Between 1953 and 1980 the Gallup organization has posed the following general performance question to cross-sections of the American public 538 times: "Do you approve or disapprove of the way (*name of the incumbent*) is handling his job as president?" The average number of such Gallup presidential performance polls has grown from 12 per year during the Eisenhower administration to roughly 24 per year during the Carter presidency.¹

Analyses of public support (e.g., Mueller, 1970; Stimson, 1976) have shown that there is an unmistakable pattern in these evaluations. With the exception of the Eisenhower administration, public support has declined regularly over the course of a presidential term. This pattern of erosion has received substantial attention and comment in the literature on presidential government. The expectation that public support will decline over the course of the term is strongly emphasized in textbook descriptions of the president and public opinion (e.g., Cronin, 1980, pp. 15-16), employed as a basic premise in analyses of presidential policy making (e.g., Light, 1982), and cited as evidence that the demands and problems facing the president have grown increasingly complex and unmanageable (e.g., Cronin, 1980; Hodgson, 1980). In sum, the decline in public support has become an integral part of the conventional wisdom about modern presidential government.

The acceptance of inevitable erosion into the conventional wisdom raises a serious question about the vitality of the institution. Must a president be a passive observer of his declining support

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¹These and all subsequent references to Gallup Poll results have been gathered from the *Gallup Opinion Index* (1983) and the three-volume anthology entitled *The Gallup Poll: Public Opinion, 1935-1971*. The

Gallup question was asked from 1938 to 1952 as well. We have chosen to ignore this period because the question was asked less frequently (averaging approximately five times per year).

or can he, through the prerogatives at his disposal, influence his public standing? If not, a fundamental question arises about a president's ability to lead or influence public opinion. In order to investigate this question, this article seeks to bring together four heretofore separate research questions.

The first pertains to the value of public support. Research has shown that public support exerts a substantial impact on 1) the results of both congressional (e.g., Kernell, 1977; Tufte, 1975) and presidential elections (e.g., Abramson, Aldrich, & Rohde, 1982), 2) the willingness of legislators to support presidential policy initiatives (Edwards, 1980; Rivers & Rose, 1981), and 3) the congressional response to presidential vetoes (Rohde & Simon, 1983). These results underlie the more general conclusion that public support is central to the day-to-day operations of the presidency. In effect, the opinion polls provide a relatively continuous referendum on the president and thereby determine his current power situation. As Hodgson (1980, pp. 210-211) argues, the president's standing in the "perpetual election" provides one piece of information that shapes the perceptions of those decision makers who are potential targets of presidential persuasion. Public support thus provides an indication of whether "it is wise or foolish to resist presidential persuasion" (Brody & Page, 1975, p. 136). In short, the need for public support never abates. This creates an incentive for the president to manage public support and thereby maintain the influence necessary to achieve the objectives of his administration.

The second research concern pertains to the determinants of public support. This literature (e.g., Brody & Page, 1975; Hibbs, 1982a, b; Kernell, 1978; MacKuen, 1983; Mueller, 1970; Stimson, 1976) offers several plausible explanations for the aggregate pattern in public support and demonstrates that declines in support are, in part, tied to the environment. This suggests that a president can influence his standing to the extent he is able to affect the salient environmental events and conditions. As such, these events and conditions constitute levers which, if pulled at the appropriate time, can influence popular support.

Underlying the consensus on this general point, however, is considerable disagreement. One point of contention focuses on the relative importance of environmental events and conditions. Throughout the historical development of the literature, energies have typically been directed to specifying and assessing the impact of the economy on public support (e.g., Hibbs & Fassbender, 1981; Kenski, 1977a, b; Monroe, 1981). This research stresses, either implicitly or explicitly, that the economy constitutes the most important

influence on public support. Others, however, have focused more closely on the impact of events (e.g., Brody, 1983; Brody & Page, 1975; Haight & Brody, 1977) and recent results (e.g., MacKuen, 1983) suggest a relative equality between the economy and political events. Because there is substantial disparity in the ease with which these levers can be manipulated (e.g., Ostrom & Simon, 1984), this issue is vital to understanding whether and how presidents can influence their public standing.

An even more fundamental and elusive issue concerns the characterization of the citizen-as-evaluator. Existing approaches appear to fall into two camps. There are analyses that are firmly anchored in the rational choice tradition. Citizens are assumed to be purposive actors who continuously monitor the environment, draw fine distinctions, and possess substantial political memories (e.g., Hibbs, 1982a, b). Other analyses proceed in the tradition of cognitive psychology and reject the rational choice characterization of the citizen. Such explanations generally rest on the premises that citizens regard politics as a distant concern and thus are seldom motivated to gather information or to monitor presidential behavior. They stress that evaluations of presidential performance may be rather visceral reactions (Stimson, 1976), largely symbolic in nature (MacKuen, 1983), and based upon limited and cheaply acquired information (Brody & Page, 1975). As with the issue of the economy versus events, the characterization of the citizen-as-evaluator is critical to the question of presidential influence on popular support. Essentially, the manner in which citizens acquire information about the environment and react to these stimuli will determine the effectiveness of presidential "pulls" on the levers.

The third research concern focuses on the ability of presidents to pull the levers. Its genesis is based upon the assertion, first articulated by Neustadt (1980), that the existing level of public support will determine a president's ability to influence other decision makers, the political environment, and therefore his future levels of support. This observation introduces the possibility of a *reciprocal* relationship between public support and some of its determinants and suggests that a president's ability to influence the conditions that determine his public support will depend upon his current resource level (Ostrom & Simon, 1984). This reciprocal relationship implies that an ability to influence public support is a necessity; without it, a president may be confronted by a vicious circle of eroding public support and declining influence over both decision makers and the environment which in turn may lead to institutional deadlock, ineffective policy making, and ultimately to the throwaway president (Greenstein, 1982).

The fourth research concern focuses on the generalizability of the causes of presidential approval. Existing research has utilized a number of factors unrelated to the environment to explain the individual character of presidential support ranging from separate intercepts (e.g., Mueller, 1970) to separate coefficients for one or more variables (e.g., MacKuen, 1983). Although this special tailoring enhances the empirical support for a given model in the sample period, it undermines the ability of the models to forecast into nonsample (e.g., future) periods. Lacking this ability, it is difficult to use the model to formulate general principles about popular support.

In sum, prior research on the presidency has at least implicitly identified the potential levers of influence, established a motive for using the levers, and speculated about the president's ability to use them. The objective of this article is to integrate these concerns into a single model and thereby to examine the nature of the interdependence between public support as a product of citizen decisions and as a presidential resource. We pursue this objective in five steps. First, a characterization of the citizen-as-evaluator is developed and used to specify a public support equation. Second, we formulate an equation that explains presidential effectiveness in the legislative arena and thus illustrates the operation of public support as a resource. Third, the support and effectiveness equations will be specified as a simultaneous equation system, estimated, and evaluated. Fourth, the estimates will be used to draw conclusions about the capacity of a president to influence public support and the likelihood of vicious circles. Finally, the model will be used to forecast the popular support of President Reagan from 1981 through 1983.

Citizen Evaluations of Presidential Performance

The model of the citizen-as-evaluator is based upon the assumption that citizens evaluate the president in light of their own preferences and that such decisions are a function of a comparison between expected and actual presidential performance.

Public Expectations

To begin, we assume that there exists a set of public expectations that identify those outcomes in the environment that are used to evaluate presidential performance and serve as standards for assessing the quality of the outcomes. These expectations can be arrayed as a J -dimensional vector

$$O^e_{it} = (o_{i1t}, \dots, o_{ijt}, \dots, o_{Jit}) \quad (1)$$

such that O^e_{it} represents the preferred outcome of citizen i on dimension j at time t . Thus, these expectations are analogous to ideal points (e.g., Davis, Hinich, & Ordeshook, 1970; Riker & Ordeshook, 1973) and summarize the standards that citizens will employ in assessing presidential performance.

Both the genesis and substance of these standards revolves about the fundamental distinction between institution-based and occupant-based expectations. First of all, there are particular expectations that are directed not at the specific occupant of the White House but rather toward the institution of the presidency itself. More specifically, all presidents are expected to maintain peace, prosperity, domestic tranquility, and both the authority and integrity of the office itself. Regardless of the president's stated philosophy, party identification, or prior experience, these institution-based expectations are imposed by the public because it is believed that the office provides *any* president with the necessary means to ensure that these desirable conditions are maintained. Thus, institution-based expectations are constant and cannot be altered by a president. In this sense these expectations are imposed as the givens of presidential politics.

Presidents themselves create the second type of performance expectations. These occupant-based expectations are initially formed during a specific incumbent's campaign for office. It is during the campaign that a preliminary agenda for action is put forth; attention and debate are directed toward particular problems, and alternative proposals for solving these problems are presented. Aside from their value as political currency for attracting votes, a candidate's selection of issues and stated positions on these issues lead the public to expect certain policies from the president-elect. In this manner, the winning candidate assumes office with a number of specific policy commitments. The campaign is but a point of departure and, over the course of a term, this initial agenda is likely to be clarified and revised. In contrast to institution-based expectations, then, occupant-based expectations are variable; their substantive content will vary across specific presidents as well as the arena in which each president chooses to act. In general, however, a president will be evaluated according to what he sets out to do and his effectiveness in accomplishing these tasks.

Presidentially Relevant Outcomes

Each month a tremendous amount of information that might be associated with a president's performance is reported by the national media.

This information covers a wide range of circumstances that can be referred to as outcomes. These outcomes can be divided into two sets on the basis of whether or not they are relevant to citizen evaluations of the president. The presidentially relevant outcome (PRO) set consists of all the outcomes associated with either type of citizen expectation; as such, there is a shared public perception that the president is at least partially responsible for (or has some control over) the outcome (for a similar argument, see Brody & Page, 1975, p. 141). These expectations, in effect, operate as filters, which allow citizens to simplify the rather complex political environment surrounding the president. In this sense, each PRO represents a promise upon which the president's performance may be monitored. To represent this component of the calculus, let

$$O_t = (o_{1t}, \dots, o_{jt}, \dots, o_{Jt}) \quad (2)$$

denote the outcomes that prevail on the J performance dimensions at time t .

Both the outcomes and their associated expectations can be further divided on the basis of their domain, or the arena of presidential government to which they apply. This distinction focuses upon whether an outcome occurs in the international (e.g., peace), domestic (e.g., prosperity), or personal (e.g., integrity) realm of politics. From the standpoint of how a president might influence public support, this distinction is critical. Research on the presidency has convincingly argued, for example, that the prerogatives of the president in international politics differ from those in the domestic realm (Cronin, 1980; Wildavsky, 1966). Additionally, the personal realm is important because the office provides a president with a variety of symbolic mechanisms that might be used to enhance the perception of his authority or the integrity of his actions.

Given these definitions, the environmental events and conditions that prevail at time t can be equated with either the satisfaction of expectations or with a perceived deviation from what is expected. The magnitude of the discrepancy is given by the difference between the O^e_{it} and O_t vectors.

Citizen Awareness and Concern

Next, it is assumed that citizens are cybernetic decision makers. In this regard we depart from the conventional rational choice approach, which depicts the citizen as equally aware and constantly monitoring the outcomes on all performance dimensions. Instead, we argue that a given per-

formance dimension will enter into the calculus of support only if there is some reason for citizens to notice it (e.g., substantial deviations from expectations, a sudden and dramatic change in an outcome, growing media coverage of the dimension, presidential efforts to focus attention on the dimension). Further, this observation implies that the degree of citizen awareness and concern may vary across the J performance dimensions. To capture these notions, define N_{it} as a $1 \times J$ vector which summarizes the degree of awareness and relative concern of citizen i with the J performance dimensions at time t .

Given these raw materials of choice, it is then assumed that citizen decisions are a function of the valuations attached to the weighted discrepancy between expectation and performance. To represent this valuation, define L_{it} as the loss that citizen i attaches to the environmental outcomes and events at time t . This loss function can be written as

$$L_{it} = f\{(O_t - O^e_{it})N_{it}\}. \quad (3)$$

In effect, the loss function can be interpreted as a measure of the perceived benefit that the citizen derives from the president's performance. Loss will be at a minimum when either all outcomes coincide with expectations or when the citizen occupies a state of blissful ignorance (i.e., all elements of N are equal to zero). The expression also implies that the impact of a dimension in the loss function will be proportional to both the outcome-expectation discrepancy and to the degree of citizen concern with the dimension assuming, of course, that the threshold of awareness has been crossed.

Acknowledging that the media play a critical role in transmitting the information necessary to cross such thresholds (e.g., Brody & Page, 1975, p. 140), a final distinction among the performance dimensions pertains to the manner in which information about an outcome is generated. The distinction involves whether news coverage of the outcome could have been anticipated. The primary characteristic of an anticipated outcome (e.g., economic conditions, congressional action on presidential proposals) is that information about the outcome is provided to the public as part of the regular news dissemination process. On the other hand, unanticipated outcomes (e.g., foreign policy crises, scandals) require a stimulus to trigger the attention and reportage of the news media. Consequently, unanticipated PROs are truly protean; their occurrence and impact upon presidential approval is dependent upon the pulse of history.

Table 1. The Presidentially Relevant Outcome (PRO) Types

	Anticipated			Unanticipated		
	International	Domestic	Personal	International	Domestic	Personal
Institution Based	Peace	Prosperity	Presidential succession	Crises	Social unrest	Presidential integrity
Occupant Based	Superpower foreign policy	Legislative activity	Legislative success	Diplomatic activities	Domestic policy	Personal hardship

The Citizen-as-Evaluator

When the two types of expectations are combined with the domain and news coverage distinctions, it is possible to divide all environmental events and conditions that impinge on citizen evaluations of the president into 12 mutually exclusive sets, which results in the 12 types of outcomes displayed in Table 1. As Table 1 illustrates, we have provided a generic descriptor for each of the PRO types. With respect to the institutional category, the president's perceived responsibility includes the outcomes of peace, prosperity, and presidential succession (anticipated) as well as international crises, social unrest, and personal integrity (unanticipated).

In the occupant-based subset, a president is likewise held accountable for anticipated and unanticipated outcomes. In terms of the anticipated distinction, emphasis is placed on the president's foreign policy, domestic policy, and effectiveness as a policy initiator. Finally, the unanticipated occupant-based outcomes include foreign and domestic policy initiatives as well as the personal fortunes of the president.

Letting $\Pr(A_{it} = 1)$ denote the probability that individual i supports the president at time t , the individual decision model can now be summarized via the following expression:

$$\Pr(A_{it} = 1) = b_{i0} + \sum b_{ij} \{(O_{jt} - O^e_{jt})N_{ijt}\} + e_{it}. \quad (4)$$

The equation asserts that the probability of approval is a linear and additive function of the weighted comparison between expectation and performance in 12 distinct types of environmental outcomes and events.

Explaining Aggregate Opinion

Given our concern with explaining the movement of approval over time, it is necessary to augment the individual-level model with an additional

set of assumptions. As specified, the model in equation (4) is basically unworkable in an over-time context because it requires rather detailed panel data. What is required, then, is a set of auxiliary assumptions that allow us to specify an aggregate-level model that is consistent with our characterization of the citizen as a cybernetic decision maker. We wish to stress that these assumptions are necessary owing to limited data availability.

Accordingly, it is assumed that all citizens have similar expectations about presidential performance and hold the president responsible for the same outcomes. With respect to anticipated outcomes, it is assumed that awareness or notice is proportional to the number of people who identify the outcome as a source of concern or view the institutions of government as nonresponsive. In the case of unanticipated outcomes, it is assumed that awareness is a function of the extensiveness of the media coverage given to an outcome. Once noticed, citizen evaluations of promise and performance are presumed to be similar and proportional to the discrepancy between expected and actual outcomes. Finally, the impact of the performance discrepancies are assumed to be constant across the public. This assumption implies that a single coefficient can be estimated for each factor and that the predicted aggregate level of support is obtained in a linear and additive manner. Essentially, these assumptions imply that individual reactions to the environment are homogeneous and are, implicitly or explicitly (Hibbs, 1982a), employed as necessary ingredients in all aggregate analyses of presidential support.

Given these assumptions, the aggregate-level version of equation (4) can be written as:

$$A_t = b_0 + \sum b_j \{(O_{jt} - O^e_{jt})N_{jt}\} + e_t. \quad (5)$$

Thus, presidential approval is specified to be a function of weighted deviations of outcomes from expectations, a constant term, and a random error component.

It is worth noting that this formulation builds upon and extends previous research on public support in three ways. First, it not only incorporates many of the same types of influences found in existing models (e.g., prosperity and peace), but also identifies several outcome-types that have not been considered (e.g., social unrest, actions toward the USSR, and legislative effectiveness of the president). Second, the underlying model of the citizen-as-evaluator offers a plausible compromise between the characterizations embedded in existing research. Observe as well that the expectation-performance component is similar in logic to the "good news/bad news" explanation offered by Brody and Page (1975). In effect, both models rest upon the assumption that citizen choice is based upon the monthly balance of good and bad news. In the promise and performance model, the balance (i.e., L_{it} in expression (3)) arises from a comparison of expectation and performance weighted by attention. Third, the basic model applies to all presidents regardless of party, era, or personal characteristics. There is no attempt to argue that different factors apply to different presidents or to estimate a different set of coefficients for each president or presidential term. This approach results in a general model that can be used as both a forecasting tool and an inference generator. In sum, the model both adds to existing research and integrates it into a general and comprehensive framework.

Operationalizing the Promise and Performance Model

Although the individual PROs could be operationalized in many ways, at least one presidentially relevant variable is provided for each PRO-type displayed in Table 1. A major advantage of the promise and performance model is that it provides explicit directions about the manner in which the concepts are to be measured. According to the logic of the explanation, each variable, X_{jt} , is to be constructed in the following fashion:

$$X_{jt} = (O_{jt} - O^e_{jt})N_{jt} \quad (6)$$

Consequently, each variable consists of the following raw materials: 1) an outcome or an action; 2) an expectation for the outcome; and 3) awareness of the outcome or action. These elements will be combined to construct a measure for each of the 12 types of PROs discussed in the previous section. Although in each case we are constrained by the available data, all of the measures are plausible approximations for each type of variable.

Monthly Approval

A monthly approval variable, A_t , was developed to meet the general linear model's underlying assumption of equal time intervals between observations. We determine A_t for a given month by applying the following coding rules to the Gallup general performance questions: 1) if one and only one opinion poll was conducted during a month, A_t is equal to the approval percentage for that month; 2) if the question was not asked during a month, A_t is interpolated by averaging the approval percentage on both sides of the gap; 3) if the question was asked more than once during a month, A_t is the average of all approval percentages for that month; and 4) when the sampling period encompassed two different months, that approval percentage is used for both months.² Following these rules and collecting the data from January, 1953 to December, 1980 yields 336 monthly A_t values covering the Eisenhower, Kennedy, Johnson, Nixon, Ford, and Carter administrations.

Anticipated/Occupant-Based Variables

To assess the relationship between the level of public approval and the occupant-based expectations, three variables have been formulated.

Legislative Success and Activity—Domestic Policy. In general, occupant-based expectations focus on the twin standards of action and effectiveness. To measure the president's activity in the realm of policy initiation and the effectiveness of these efforts, we have developed two indexes based upon roll-call voting on domestic issues in Congress. This arena is important primarily because many of the policy appeals that the president directs at particular groups and organized interests pertain to the domestic realm and involve a legislative remedy.

Our measure of activity, X_{2t} , is defined as the sum, to each point in the term, of congressional roll-call votes involving domestic policy on which

²The unequal interval phenomenon is a problem because in those months in which there are multiple polls, support can change while the explanatory variables cannot. It is also possible that no poll was administered during a particular month. In such instances, support will remain constant while the explanatory variables may change. Hence, there are monthly changes in the exogenous variables with no corresponding changes in the endogenous variable. Our reliance on interpolation does not substantially alter the approval series. There are 26 interpolated approval scores which represent approximately 8% of the 336 time points in the data set.

the president took a clearly identifiable position. This variable serves two purposes. First, given the time span of our analysis and the paucity of available data on presidential policy initiation, it is expected that differences in activity across administrations will reflect variations in the size and ambitiousness of each president's agenda.³ Second, the variable also provides a test of the coalition-of-minorities hypothesis (Mueller, 1970, p. 20). According to Mueller, this hypothesis asserts that any presidential initiative is destined to alienate some segment of the president's support coalition. Because the effect is triggered by presidential actions, it is a dynamic property of presidential governance—grievances are likely to accumulate as the number of presidential initiatives and decisions increase. Consequently, public support will decline over the course of a term. Even though the operationalization used by Mueller has been criticized (Kernell, 1978), the logic of Mueller's argument leads us to expect that the level of public support will decline as activity increases.⁴

However, unlike Mueller, our explanation also asserts that there is a reward associated with domestic policy making. Our second occupant-based measure, D_i , is designed to reflect effectiveness in the legislative arena. It is defined as the cumulative proportion of the domestic policy votes included in the activity index in which the position advocated by the president was victorious.

With respect to public expectations, the measure of legislative activity is based on the proposition that position-taking per se leads to defections. Although presidential rhetoric does foster a general expectation of action, the translation of

general policy pronouncements into specific proposals is accompanied by opportunity costs; the decision to address a problem in one manner implies that the president must forego other approaches and remedies. Recognition of these costs suggests that, in theory, the impact of any given initiative is likely to be negative. Thus, presidential initiatives are associated with an immediate penalty; the reward depends upon the passage of the initiative. In this manner, it is hypothesized that public support will decline with the number of legislative initiatives.

A similar logic underlies the expectation of legislative success. Research by Fishel (1979) and Light (1982) emphasizes the difficulty experienced by presidents in ushering their programs through Congress. They observe that the actual implementation of major initiatives outlined during the campaign is substantially below 50%, which suggests that the prior probability of success on any specific presidential proposal is likely to be quite small. Viewed in this light, any victory in the legislative domain represents a substantial presidential accomplishment, which in turn implies that presidents will be rewarded for legislative accomplishments and leads to the hypothesis that the reward will be proportional to our success-rate measure.

With respect to notice, it is assumed that awareness of both presidential activity and success is a function of the degree to which the public believes that government policy will resolve the major problems of the day. Research has shown that such beliefs are based upon long-term political attitudes such as external political efficacy (for an extensive overview, see Abramson, 1983). External political efficacy refers to the belief that government is responsive to public concerns about the problems of the day. Inefficacious attitudes thus reflect a general cynicism toward actions, processes, and institutions of government. This usage implies that this long-term attitude will precondition citizen responses to presidential policymaking; that is, inefficacious individuals will be less likely to notice the initiatives of the president and will, if they are passed, place little confidence or credibility in these remedies.

To incorporate this idea, we have chosen to rely upon an aggregate measure of external political efficacy. Our use of efficacy is due to its validity as an indicator of government responsiveness and the availability of data over the entire 1953-1980 period.⁵ The measure is simply the percentage of

³For example, a comparison of the total number of support votes for each four-year term since 1952 produces the following rank order: Johnson, 1965-1968 (1065 votes), Carter (967), Kennedy/Johnson (709), Eisenhower, 1957-1960 (569), Nixon, 1969-1972 (495), Nixon/Ford (408), and Eisenhower, 1953-1956 (338).

⁴This variable was constructed as follows. First, we used the annual *Congressional Quarterly* reports to determine, on a monthly basis, the number of votes in which the president took a clearly identifiable position; similarly, the number of votes in which the president's position won was ascertained. Second, in working with such monthly measures, we periodically encountered the problem of an abnormally small number of roll-call votes in a given month. The problem caused by such occurrences is that the resulting monthly time series contains a number of "unrepresentative" peaks and troughs. To compensate for these distortions, we summed both the test votes and the presidential victories over the term. D_i , our proxy for the legislative effectiveness of the president, represents the ratio of cumulated victories to cumulated votes up to and including month i .

⁵It should be noted that there has been considerable discussion of the measurement and meaning of such long-term attitudes. There are two concerns that are pertinent to our choice of external political efficacy.

respondents who gave efficacious answers to both external efficacy items contained in the biennial CPS surveys (see items 3 and 4, Abramson, 1983, p. 136). Thus, the proportion serves as the notice factor applied to the legislative indexes and will change every two years.

There is also evidence that suggests that domestic roll-call success is itself a function of the president's current public standing. The analysis of annual data by Edwards (1980), for example, has shown that the presidential roll-call support scores of congressional members varies with the level of public support. Using the now-defunct Congressional Quarterly Boxscores, Rivers and Rose (1981) show that the passage of presidential requests for legislation is also determined by public support. Thus, when coupled with the promise and performance argument, this evidence implies that presidential influence in Congress is reciprocally related to public support, which in turn requires us to include in the model a *legislative effectiveness equation* that explains the movement of D_t . Thus, in order to assess the impact of D_t on A_t , it is necessary to take the impact of A_t on D_t into account.

Our specification of the domestic legislative support equation is based upon the assumption that, in general, presidential fortunes in Congress are a function of presidential resources and the size of his legislative agenda. In addition to the president's current approval, A_t , the party control of Congress, Z_{2t} , is identified as a basic resource for generating outcomes favorable to the president. As Light (1982, p. 26) has observed, "The base of presidential capital is always the number of party seats the president has in Congress." This observation is supported by empirical studies which show that roll-call support for the president's legislative program is greater among members of his party (e.g., Edwards, 1980). Moreover, such control affords the president an opportunity to work with a set of chamber and committee leaders who are likely to be more

receptive and less ideologically hostile than would be the case if the opposition party controlled Congress. Our measure is a binary variable which assumes a value of one if Congress is controlled by the president's party. It is expected that D_t will be positively related to Z_{2t} .

There are several arguments that suggest that the size of the president's domestic legislative program constitutes another determinant of effectiveness. First of all, large programs are likely to be both ambitious and controversial—characteristics that are likely to reduce the probability of success. Second, as the number of bills introduced and endorsed by the president increases, the consultation and planning on any given bill are likely to decline. Hence, many bills lack the groundwork necessary for constructing a winning coalition. To the extent that the attention and resources of the administration are dispersed over a variety of policy fronts, the president's program is likely to grow more vulnerable to congressional factions that concentrate their efforts on defeating specific measures. Accordingly, it is hypothesized that the legislative success rate will vary with the size of the president's agenda. To assess this proposition, Z_{3t} is defined as the sum, to each month in a given Congress, of domestic roll-call votes involving a test of the president's position. The summation of presidential support votes is started anew in January of odd-numbered years because the advent of a new Congress involves both changes in personnel and the resubmission of proposals rejected or ignored by the preceding Congress. As such, Z_{3t} reflects the relative size of each president's agenda in a given Congress and is sensitive to the cycle of legislative politics. It is expected that Z_{3t} will be negatively related to D_t .

This leads to the following legislative effectiveness equation:

$$D_t = a_0 + a_1 A_t + a_2 Z_{2t} + a_3 Z_{3t} + v_t. \quad (7)$$

Given the level of aggregation employed,⁶ this

These pertain to whether efficacy is merely a reflection of partisanship and whether public approval of the president operates as a determinant of external political efficacy. Recent research suggests that neither partisanship (Abramson, 1983, p. 177) nor approval (Iyengar, 1980) operates as the driving force of external efficacy. Rather, studies emphasize the importance of the spirit of the times (Zeitgeist) and the cumulative impact registered by the political turmoil of the 1960s and early 1970s (Lipset & Schneider, 1983; Searing, Wright, & Rabinowitz, 1976). This research leads us to believe that the efficacy index used in the analysis is not a redundant measure of popular support, not an artifact of partisanship, and not an attitude determined by public support.

⁶It must be emphasized that the specifications designed to explain congressional roll-call outcomes are extremely dependent upon the level of analysis used. For example, if the variable of interest is the support of individual members for a given initiative, the equation would include various personal characteristics of the members (e.g., ideology, electoral margin). Similarly, if the objective is to explain whether a given initiative passed or failed, then it is necessary to include indicators for the subject matter of the bill and how much effort the president devoted to lobbying for the bill. Given the level of aggregation employed, equation (7) represents a plausible specification which is consistent with the major research findings on the president's fortunes in Congress.

equation represents a plausible specification that is consistent with the major research findings on the president's fortunes in Congress.

Foreign Policy Decision Making. The final occupant-based variable, X_{31} , focuses on the president as a foreign policy decision maker. One key campaign issue during the post-World War II period has centered on the conduct of foreign relations with the USSR. As George and Keohane (1980, p. 225) note, this has led to two conflicting promises. On one hand, presidents have promised to resist Communist aggression wherever it arises to prevent the further expansion of the USSR (the so-called domino effect). On the other hand, presidents have promised to exercise care to avoid a serious confrontation with the USSR because such confrontations could lead to World War III. As a result, a president must at various times be both aggressive and restrained in his reaction to the USSR.

To operationalize this concept we have utilized—and updated—the dyadic conflict-cooperation data from COPDAB (Azar, 1982). Specifically, we focus on the level of conflict and cooperation directed by the United States toward the USSR. It is important to note that this type of conflict is short of war. The variable, X_{31} , takes on the value of -1 times the base 10 logarithm of the absolute value of the cumulative weighted conflict (Azar & Havener, 1976)—each term—when conflict dominates. The variable takes on the value of the logarithm of the cumulative weighted conflict whenever there is an excess of cooperation to that point in the term. The level of public attention is measured by the percentage of the population identifying foreign policy issues as the most important problem facing the country.⁷

Similarly, our discussion of public expectations and the impact of legislative activity and success are the most plausible and testable given the level of aggregation used. A more thorough examination of these issues requires disaggregation on two fronts. The first is a partition of the public into subsets based upon, for example, ideology and issue concerns. Second, the initiatives and successes of the president can be disaggregated along substantive lines parallel to the partition of the public. Given these divisions, specific measures of expectations can be developed and the impact of both initiation and success can be compared across the subsets.

The "most important problem" item periodically administered by the Gallup organization provides the basis for several of the weighting factors used in our analysis. On 79 occasions during the 1953-1980 period, the Gallup Poll included the following question: "What do you think is the most important problem facing the country today?" To obtain the weighting for our notice measures, we grouped the responses into three more

It is our contention that U.S. foreign policy behavior, X_{31} , will be negatively related to A_1 ; that is, conflict is expected to increase popular support for the president. The expected relationship between direct conflict or cooperation with the USSR and popular support is based upon the following logic. First, conflict, short of war, dichotomizes the world into we-they categories and hence produces a rally-type effect; thus there is no political capital to be made by political opponents from openly criticizing a president for adopting a tough stance toward the USSR. Combining the rally-type effect and the absence of outright criticism, conflict will generate a positive surge in presidential support. Second, a policy that emphasizes cooperation is divisive precisely because it diffuses the black-white or we-they view of the world. Presidents who seek accommodation are no longer protected by the unwritten rules of closing ranks during times of national peril. There is no immediate threat from the USSR, and hence it is possible for opponents of the president to question openly the president's foreign policy. In short, sabre rattling is likely to lead to an increase in popular support.

Anticipated/Institution-Based Variables

Economic Prosperity. With respect to the prosperity expectation, the popularity literature is rather ambiguous. On one hand, theoretical discussions generally agree that economic factors should operate as prominent determinants of public support (Hibbs, 1982a; Kenski, 1977a, b; Kernell, 1978; MacKuen, 1983; Monroe, 1981; Norpoth & Yantek, 1983). On the other hand, there is intense disagreement over the choice of indicators (e.g., inflation, unemployment), the measurement of short- and long-term effects (e.g., lag structure), and the empirical impact of the variables selected (symmetric vs. asymmetric impact, multicollinearity problems). The promise and performance explanation leads us to adopt a

general categories: war (specific mentions of Korea and Vietnam), foreign affairs (specific mentions of the USSR, communism, general fear of war or nuclear war), and the economy (specific mentions of unemployment or inflation). Because the survey item is administered on an irregular basis (approximately 2-3 times per year), the percentages are set for the month of a given poll and remain at these levels until the next appearance of a poll containing the most important problem item. We realize that this may create problems. However, given existing data and the chosen level of analysis, it is the only workable strategy for incorporating notice.

more general but limited approach toward the economy.

First, like most previous researchers, we concentrate on both unemployment and inflation rates, which are routinely reported in the print and broadcast media. Further, information about jobs and prices is readily accessible to the public, and these outcomes have historically been the most frequently cited economic concerns of the public.

Second, we have chosen to concentrate on the current values of the variables and to avoid two of the complications associated with the use of an elaborate lag structure. Such a structure assumes that individuals monitor, remember, and discount past values of the economic variables. This type of evaluation is inconsistent with our characterization of the citizen-as-cybernetic-evaluator. Moreover, recent research by Norpoth and Yantek (1983) suggests that the presence of a lagged relationship between presidential support and the economy may be a function of the persistent autocorrelation in each series. Hence they warn that the evidence for lagged variable structures may be spurious.

Third, to deal with the persistent multicollinearity problem, we have constructed an index of economic misery similar to that used by Hibbs (1982c, p. 390);⁸ specifically, we add the unemployment and inflation rates to obtain a misery index. The lack of any a priori weighting of the two components in the index, as Hibbs (1982c, p. 390) notes, can be justified because when individual weights are estimated, their impact on public support is both negative and of approximately equal magnitude (Hibbs, 1982a, see Table 1, pp. 438-439). Further, the additive combination of inflation and unemployment outperforms all other unlagged measures of economic prosperity that we tried.⁹

Fourth, to measure public expectations, we assume that the public has an expected value of no economic misery. In our preliminary analysis, we

developed a measure of expectations based on a moving average that is similar in construction to those used by Hibbs (see Hibbs & Fassbender, 1981, pp. 31-47). We found two problems with this approach. First, the moving average rises to over 10 in 1975 and increases to over 15 in late 1980; it seems implausible that the public would ever view such outcomes as an acceptable level of performance. Lacking another compelling method to estimate expectations, we have relied on zero. Second, the measure that uses a zero expectation significantly outperforms the measure using the moving average expected value.

To capture the percentage of the population that notices the two economic concerns, we use the percentage of the population identifying inflation or unemployment or both as the most important problem facing the country (see Note 7). The discrepancy between actual and expected economic performance is then weighted by this proportion. The measure of economic misery, X_{4t} , introduces the idea that the impact of economic performance on public support is jointly dependent upon the quality of economic outcomes and the number of people paying attention to the prosperity issue.

War. Turning to the impact of war on public support, prior analyses (e.g., Kernell, 1978) have emphasized that battle deaths are the most fundamental outcome produced by war. The number of troops killed in action serves as a graphic indicator of both the magnitude and intensity of a war as well as the collective pain imposed on the public. The magnitude of both the Korean and Vietnam wars are measured by the base 10 logarithm of the sum of U.S. troops killed as a result of enemy action. The sum is used to capture the notion "that people react more to the cumulative human costs of war than to its duration" (Mueller, 1973, p. 59). The use of the sum rather than a monthly figure reflects our view that the pain of war cannot be reduced by short-term fluctuations in battle deaths. Our use of the logarithm acknowledges that, over the course of a war, the public is "sensitive to relatively small losses in the early stages, but only to large losses in later stages" (Mueller, 1973, p. 62).

Insofar as the president is concerned, it is questionable whether all wars and their accompanying casualties can be expected to exert a similar impact on public support. For example, it is conventionally argued that World War II was a "popular war" in the sense that Roosevelt and Truman were not severely punished for the loss of life that the conflict generated. It is also believed that both Korea and Vietnam evolved into unpopular wars and, as a result, exerted a strong negative effect on the public standing of Truman and Johnson. Therefore, what is needed for a more general

⁸Hibbs makes use of what he calls an $(R-P-U)$ index, where R is the real personal disposable income, P is the inflation rate, and U is the unemployment rate. Since R is only reported quarterly and does not receive the intensive coverage given to the other two indicators, we have chosen to rely on the $(-P-U)$ component. Also, the $(R-P-U)$ formulation does not work as well as the $(-P-U)$ form (cf. note 9).

⁹In developing our measure of economic prosperity, we investigated the following measures: 1) inflation and unemployment as separate measures, 2) $R-P-U$, and 3) misery index. The additive misery index outperformed the other two by a substantial margin. Note that the measure of real personal disposable income is only available on a quarterly basis.

treatment of war is a corrected measure of battle deaths which acknowledges these differences. One important distinction pertains to whether the public believes that the substantial costs of waging a war are worth the benefits. One reflection of such a judgment is the proportion of the public who believe that the war is a mistake.¹⁰ Accordingly, our corrected measure weights the battle death index by this proportion. The resulting measure thus reflects both the costs of war and the beliefs about the necessity and importance of waging the conflict. In doing so, we are able to account for the supposed differential impacts on presidential support of Korea and Vietnam on one hand and World War II on the other hand.¹¹

Because most models exclude the first half of 1953 and the period from 1969 to 1972 in estimating the impact of battle deaths, existing research suggests that neither Eisenhower nor Nixon was punished to any measurable extent by the carnage of war. The identification of peace as an institution-based expectation imposed on all presidents leads us to reject this suggestion. It is our contention that although Eisenhower, Johnson, and Nixon were held accountable for the war, they were evaluated much differently for two reasons. First, there was a difference in the attention (or notice) which was focused on the war. By weighting the corrected battle death measure with the proportion of the population identifying war as the most important problem, the resulting measure, X_{5t} , implies that the impact of war varies with its intensity, perceived value, and public concern.

Second, both Eisenhower and Nixon inherited a war. Furthermore, ending the war was a major campaign issue in both 1952 and 1968. It seems plausible, therefore, that a president who starts a war will be held accountable to a different degree than a president who inherits a war. Although the president who inherits a war will be penalized, it

seems reasonable to suppose that the magnitude of the penalty will be reduced sharply. In order to ascertain whether or not the change in administrations (e.g., Eisenhower in 1953, Nixon in 1969) during a war will alter the magnitude of the effect of X_{5t} on A_t , we include another variable X_{6t} , defined as

$$X_{6t} = X_{5t} * P_t,$$

where $P_t = 1$ during a new president's term and 0 otherwise. As such, the coefficient of X_{6t} represents the change in the impact of the war brought on by a change in presidential administrations. Thus, to obtain the impact of the inherited war, it is necessary to focus on the sum ($b_5 + b_6$). We anticipate that b_6 will reduce the impact of the war on presidential support; that is, $(b_5 + b_6) < b_5$.

Sympathy. The final anticipated institution-based measure, X_{7t} , focuses on the sympathetic reaction that arises when a new president assumes office without being elected. Once elected, a president is expected to complete his term in office; hence, the expected value of this variable is zero. Whenever this expectation is not satisfied, this binary variable takes on a value of one during those months in which, because of the president's death or resignation, the vice president serves the remainder of his predecessor's term. Its inclusion is based upon the argument that citizens will rally around the accidental president. First, whether it results from assassination, resignation, or natural death, succession to office is a traumatic event that will generate a wave of sympathy and support for the new president during the times of crisis and uncertainty. Second, and more relevant to the characterization of this concern as an institution-based variable, the new president serves as a symbol of continuity and stability in government, regardless of party or ideology. His automatic assumption of authority provides an example and thus reaffirms the expectation that the institution of the presidency can withstand traumatic change. Consequently, it is hypothesized that, *ceteris paribus*, public support will be uniformly higher during these extraordinary periods than during the normal circumstances of politics as usual. Insofar as the public's attention is concerned, it is assumed that everyone will be aware of the transition in power and hence $N_{7t} = 100\%$ until a new term begins.

Unanticipated Variables

An unanticipated outcome involves factors that occur irregularly and must possess an identifiable stimulus that triggers a reaction and attracts the public's attention. The outcomes must also be

¹⁰The data are the percentage of the people who answered yes to the following question: "In view of the developments since we entered the fighting in Vietnam, do you think the United States made a mistake sending troops to fight in Vietnam?" The percentages range from 24% to 61% and are listed in Mueller (1973, pp. 54-55).

¹¹Mueller (1973, p. 63) notes that the number of people identifying World War II as a mistake is very small. He observes that in 1944, after the war had been going on for three years, only 14% of the populace thought the war was a mistake. To compare World War II and Vietnam in terms of our corrected measure, consider the following. Even though there were eight times as many deaths in World War II as in Vietnam (400,000 to 55,000), the corrected measure for Vietnam was four times larger than the World War II measure.

newsworthy, so that the public will have enough information to make evaluative judgments. To incorporate such events into the analysis, therefore, we focus upon the issues of selection, hypothesized impact, and measurement.

Selection. Our first step was to define six kinds of events, each of which corresponds to a particular combination of the domain and expectation distinctions used in our typology of presidentially relevant outcomes. Thus, three kinds of events are institutional: The first kind involves threats to peace and includes international crises triggered by the use of military force and other aggressive actions by the United States, its allies, and adversaries. The next set, domestic disruptions, includes those that threaten the tranquility of domestic politics (e.g., major civil rights and anti-war demonstrations and the outbreak of urban violence). The third kind of institutional event challenges the integrity of the president and his administration.

Similarly, the occupant-based category includes three types of events. There is, first of all, a set that focuses on the conduct of diplomacy. This kind includes summit conferences, major travels abroad, and other highly publicized statements and actions that involve the president in the international arena. Second, there is a set of events associated with presidential policy initiatives. These initiatives either pertain to domestic concerns or are brought before the public by the president in order to build the congressional support necessary to translate proposals into policy. The third set of events is personal in character and involves incidents that focus attention on the personal and political health of the president.

The next step involves the choice of the events in each category. This was accomplished in the following manner. First, a preliminary list of events for each group was compiled by consulting prominent chronologies (e.g., *Encyclopedia Britannica*) and the public support literature (e.g., Lee, 1977; MacKuen, 1983; Mueller, 1973). Second, to eliminate the more obscure episodes, Mueller's (1973, p. 209) criteria that an event must be sharply focused and dramatic were applied. Third, these "candidate" events were included on our final list only if they were covered on the front page of *The New York Times*. This criterion was applied because such placement reflects an educated judgment by the editors about the comparative importance of the day's events and news stories. Reliance on *The New York Times* is also justified on the grounds that its national coverage is highly correlated with other types of media coverage. This procedure resulted in the 85 events listed in Table 2.

Hypothesized Impact. Because research has

shown that the impact of unanticipated events is not uniformly positive (e.g., Lee, 1977; MacKuen, 1983), it is necessary to distinguish between outcomes that are approval-enhancing and those that are disapproval-enhancing. The set of international events, X_{8t} , is hypothesized to be approval-enhancing because of the perception of an external threat and rally-around-the-flag effect which such threats stimulate. The two remaining institutional and unanticipated sets of events, X_{9t} and X_{10t} , disruptions and scandals, are likely to be disapproval-enhancing because they represent violations of fundamental citizen expectations (domestic tranquility and integrity).

Two of the occupant-based sets are likely to be approval enhancing. The diplomatic actions of the president, X_{11t} , involve perhaps the most publicized uses of the political stage and permit the president to capitalize on his symbolic role of national leader. We expect the set of personal events, X_{13t} , to register a positive impact because of the sympathetic reaction that arises during times of personal difficulty. The final occupant-based set of events focuses on policy initiatives, X_{13t} . These are expected to be disapproval-enhancing because the issues involved are basically divisive and, quite often, a president must exacerbate these divisions to present his case.

Measurement. Our strategy is an adaptation of the news-based approach developed by Brody and his colleagues in their studies of public support (Brody, 1983; Brody & Page, 1975; Haight & Brody, 1977). First, to be selected, an outcome must fit into one of the six unanticipated outcome types and receive front-page coverage in *The New York Times*. If more than one outcome of a given type occurs during a given month, the relevant variable will reflect the coverage given to the event type. Second, the salience of the event is measured as the percentage of the month in which the outcome receives front-page coverage. Third, the expected value for each of the variables is 0; thus, even though experience demonstrates that such events will occur some time during the term, the expectation is that in any given month there will be no unanticipated events. Fourth, the duration of the outcome depends upon the length of time that an outcome remains newsworthy; in most instances, coverage does not extend beyond the initial 30-day base period. However, when events occur which generate outcomes over a more extended period of time (e.g., Iran, the Sherman Adams, Bert Lance, and Watergate scandals), they are treated as a series of unanticipated outcomes, and monthly values are calculated for the duration. After the end of an event, its salience is discounted in an exponential fashion over the four succeeding months reflecting the lingering

Table 2. The Unanticipated Events

Event Type	Date	Description
Crises	November 1956	Soviet forces invade Hungary (.93)
	October 1957	Soviets launch Sputnik I (.57)
	August 1958	Eisenhower sends marines to Lebanon (.63)
	April 1961	Bay of Pigs (.80)
	September 1961	Berlin Wall erected (.97)
	November 1961	U.S.-Soviet tank confrontation in Berlin (.87)
	November 1962	Cuban missile crisis (1.00)
	August 1964	Gulf of Tonkin incident (.60)
	May 1965	Johnson sends Marines to Dominican Republic (1.00)
	September 1968	Soviet forces invade Czechoslovakia (.97)
	May 1975	Mayaguez incident (.40)
	November 1979	Iran hostage crisis (1.00)
	January 1980	Soviets invade Afghanistan (1.00)
Diplomacy	August 1953	Korean Armistice signed (.67)
	December 1953	Eisenhower delivers "Atoms for Peace" Address (.43)
	July 1954	Indochina Peace Agreement (.37)
	July 1955	Big Four Summit in Geneva (.60)
	September 1959	Khrushchev visits U.S. (.67)
	December 1959	Western Alliance summit in Paris (.67)
	May 1960	Paris summit/U-2 incident (.40)
	June 1960	Eisenhower tours Asian capitals (.47)
	March 1961	Kennedy announces "Peace Corps" Program (.27)
	June 1961	Kennedy-Khrushchev meet in Vienna (.40)
	June 1967	Johnson-Kosygin meet in Glassboro, N.J. (.30)
	March 1969	Nixon tours Europe (.33)
	August 1969	Nixon visits Romania/South Vietnam (.50)
	November 1969	Nixon announces "Vietnamization" Plan (.43)
	February 1972	Nixon visits People's Republic of China (.70)
	June 1972	Nixon visits Soviet Union (.60)
	February 1973	Vietnam ceasefire agreement signed (.70)
	July 1973	Brezhnev visits U.S. (.37)
	June 1974	Nixon visits USSR (.40)
	December 1974	Ford and Brezhnev confer in Vladivostok (.27)
Personal	October 1955	Eisenhower suffers heart attack (1.00)
	June 1956	Eisenhower undergoes surgery for ileitis (.83)
	December 1957	Eisenhower suffers minor stroke (.30)
	August 1963	Death of Kennedy's newborn son, Patrick (.13)
	December 1963	Kennedy assassinated; Johnson assumes office (1.00)
	October 1965	Johnson undergoes gall bladder surgery (.30)
	April 1968	Johnson withdraws from presidential race (.87)
	August 1974	Nixon resigns; Ford assumes office (1.00)
	September 1975	Attempted assassination of Ford (.07)
	October 1975	Attempted assassination of Ford (.13)
Scandals	February 1959	Sherman Adams (through January 1959)
	October 1964	Walter Jenkins (.41)
	March 1973	Watergate (through January 1975)
	July 1977	Bert Lance (through January 1978)
	August 1978	Peter Bourne (.36)
	July 1980	Billy Carter (.53)
Domestic unrest	March 1956	Montgomery bus boycott (.13)
	September 1957	Desegregation conflict in Little Rock (1.00)
	June 1961	Freedom Riders attacked in Alabama (.60)
	October 1962	James Meredith enters University of Mississippi (1.00)
	May 1963	Violence in Birmingham, Alabama (.37)

Table 2. (continued)

Event Type	Date	Description
Domestic unrest (continued)	June 1963	Wallace blocks doors to University of Alabama (.03)
	September 1963	Civil rights march in D.C./Violence in Birmingham (.16)
	April 1965	Selma to Montgomery march/Viola Liuzzo murdered (.33)
	August 1965	Violence in Watts (.27)
	August 1966	Outbreak of violence in major U.S. cities (.40)
	May 1967	100,000 demonstrate against Vietnam War (.03)
	August 1967	Outbreak of violence in major U.S. cities (.73)
	November 1967	Pentagon stormed by anti-war protestors (.10)
	April 1968	Martin Luther King assassinated (.33)
	June 1968	Poor People's march in Washington, D.C. (.33)
	August 1968	Violence at Democratic National Convention (.23)
	April 1969	Violence on anniversary of King assassination (.03)
	October 1969	First Vietnam moratorium (.13)
	November 1969	Second Vietnam moratorium (.20)
	May 1970	Violence at Kent State/Jackson State (.43)
	May 1971	Protests and mass arrests in Washington, D.C. (.20)
	June 1980	Violence erupts in Miami, Florida (.10)
Policy initiatives	January 1957	Eisenhower presents Middle East doctrine (.87)
	January 1963	Kennedy proposes tax cut (.43)
	January 1964	Johnson proposes War on Poverty (.17)
	March 1965	Johnson presents Voting Rights Bill (.33)
	May 1969	Nixon proposes lottery to replace draft (.10)
	August 1971	Nixon imposes wage-price controls (.93)
	July 1973	Nixon re-imposes wage-price controls (.93)
	May 1977	Carter proposes comprehensive energy program (.40)
	October 1979	Carter submits Panama Canal Treaty to Senate (.03)
	June 1979	Carter submits SALT II Treaty to Senate (.17)
	February 1980	Carter proposes draft registration system (.17)

impact that such outcomes are thought to have (e.g., Kernell, 1978; Mueller, 1970).

Model Specification

Based upon the discussion in this section, the operational version of the promise and performance explanation can be expressed as follows:

$$D_t = a_0 + a_1 A_t + \sum a_k Z_{kt} + f_t \quad (8)$$

$$A_t = b_0 + b_1 D_t + \sum b_j X_{jt} + e_t \quad (9)$$

This specification produces a general, comprehensive model of legislative success and presidential approval.

Each of the four concerns raised in the Introduction can be investigated in the context of equations (8) and (9). First, the degree to which approval is a potent resource can be determined by assessing the magnitude of a_1 . The value of a_1 provides an indication of the degree to which a president's current public standing affects his domestic legislative success.

Second, the causes of a president's approval level over time can be determined by assessing the

magnitude of the b_j ($j = 1, 2, \dots, 13$). These coefficients will provide an indication of the direct effect that each of the presidentially relevant variables has on the president's current public standing. Given the estimated coefficient set, it will also be possible to account for the varying texture (in terms of approval) of each of the post-1952 presidential administrations.

Third, the importance of the reciprocal relationship between A_t and D_t can be investigated to determine the extent to which it can lead to a vicious circle. One of the more interesting features of the simultaneous equation model specified earlier is that it provides a foundation from which it is possible to assess the degree to which declines in support fuel decreases in legislative support which, in turn, lead to further decreases in popular support. As a consequence of this relationship, each variable will have an indirect effect on A_t and D_t as well. The sum of the direct and indirect effects may create a vicious circle which may have a substantial impact on the ability of any president to maintain sufficient levels of popular support.

Finally, to address the generalizability issue, we will use the model to calculate a series of *ex post* forecasts of Reagan's 1981-1983 approval levels.

Table 3. 2SLS Estimates

Variable	Descriptor	Coefficient	S.E.	t-Ratio	Impact Range
D_t	Legislative success	.371	.060	6.21	[2.55, 23.37]
X_{2t}	Legislative activity	-.037	.006	-5.97	[-15.03, 0]
X_{3t}	Conflict/cooperation with USSR	-1.601	.694	-2.31	[-1.55, 3.14]
X_{4t}	Economic misery	-1.332	.138	-9.63	[-21.41, -.40]
X_{5t}	Cor. battle deaths	-18.065	1.781	-10.14	[-27.46, 0]
X_{6t}	Change in X_{5t} due to new president	14.745	2.328	6.33	[0, 15.78]
X_{7t}	Sympathy	3.515	1.374	2.56	[0, 3.52]
X_{8t}	Crisis	7.171	1.313	5.46	[0, 7.17]
X_{9t}	Social unrest	-5.703	2.072	-2.75	[-5.70, 0]
X_{10t}	Scandal	-13.618	2.116	-6.43	[-13.62, 0]
X_{11t}	International policy	9.385	1.796	5.22	[0, 9.34]
X_{12t}	Domestic policy	-3.313	2.056	-1.61	[-3.31, 0]
X_{13t}	Personal events	9.736	1.662	5.86	[0, 9.74]
Constant		52.106	3.118	16.71	
R^2		.815			
S.E.		5.825			
d		.530			
Legislative effectiveness equation					
A_t	Approval	.410	.045	9.17	
Z_{2t}	Control of Congress	20.623	1.037	19.59	
Z_{3t}	Biannual activity	-.003	.006	-.53	
Constant		-27.283	3.017	-9.04	
R^2		.641			
S.E.		8.932			
d		.330			

Parameter Estimation

In order to estimate equations (8) and (9), it is necessary to utilize a procedure that takes the interdependence of the two equations into account. Two stage least squares (2SLS) has been chosen as the estimation technique because it leads to consistent estimates of the structural coefficients. The results of the estimation are displayed in Table 3.

An examination of the 2SLS estimates for the approval equation indicates that all but one of the variables is statistically significant, in the predicted direction, at the .01 level. Although there are no standard goodness-of-fit measures for the 2SLS estimates, the predicted values of approval were compared to the actual values yielding a R^2 of .82 and a standard error of 5.83 rating points. This is a rather substantial accomplishment, given that there are no special variables (e.g., lag of approval, time into term) in the model.

An examination of the legislative success equation indicates that all but one of the variables are significant, in the predicted direction, at the .01

level. The predicted values of success were compared to the actual values yielding an R^2 of .64 and a standard error of 8.93.

Note that in both equations the residuals exhibit positive serial correlation. Although this does not affect the consistency of the structural estimates, it does create problems for the estimates of the variance-covariance matrix (Kmenta, 1971). To deal with these problems it is necessary to examine the residuals and take the nature of the time-dependent error processes into account.

To accomplish this task, we have followed the three steps suggested by McCleary and Hay (1980). First, we estimated the autocorrelation (ACF) and partial autocorrelation (PACF) functions for the disturbances from each equation. The ACF of the approval series exhibits an exponential decay, and the PACF shows a single spike at lag one. We initially identify the approval residuals as a first-order autoregressive process. Following a similar procedure for the success residuals, we initially identify them as a third-order autoregressive process. In the second step the relevant parameters of the disturbance proc-

esses were estimated. After under- and over-modeling, the best characterization for each disturbance process, for the success and approval residuals respectively, is given by:

$$f_t = p_{11}f_{t-1} + p_{12}f_{t-2} + p_{13}f_{t-3} + w_t \quad (10)$$

$$e_t = p_{21}e_{t-1} + v_t \quad (11)$$

where w_t and v_t are assumed to be white noise processes. All parameters in equations (10) and (11) were statistically significant in the predicted direction. In the final step, the estimated values of w_t and v_t were subjected to an ARIMA analysis. Each of the resulting noise series is in fact white noise. As a consequence, all of the systematic variation has been removed from the residuals. The reason for the persistent serial correlation is not dealt with in the context of our explanation. Instead, we seek to ensure that its presence does not confound the resulting estimates.

The previous analyses have provided information that can be used to obtain consistent estimates of both the structural coefficients and the variance-covariance matrix. We incorporated our model of the residuals process (equations (10) and (11)) directly into equations (8) and (9).¹² The estimates for the augmented set of equations (see equations (8*) and (9*) in note 12) will provide consistent estimates of the parameters and variances in equations (8) and (9).

Klein (1974, pp. 207-210) suggests three possible approaches to the estimation of systems similar to equation (8*) and (9*). The procedure we have chosen estimates both the structural and error-process coefficients simultaneously with the use of nonlinear 2SLS procedure (Amemiya, 1974). The nonlinear estimation process was initialized with parameter estimates from the previous two stages of the estimation procedure. The results of the nonlinear 2SLS are presented in Table 4.¹³ As can be seen, the estimates are quite similar to those presented in Table 3.

¹²This yields the following set of equations, which will be used for purposes of estimation:

$$\begin{aligned} D_t = & (1 - p_{11} - p_{12} - p_{13})a_0 + p_{11}D_{t-1} \\ & + p_{12}D_{t-2} + p_{13}D_{t-3} \\ & + a_1(A_t - p_{11}A_{t-1} - p_{12}A_{t-2} - p_{13}A_{t-3}) \\ & + \sum a_k(Z_{kt} - p_{11}Z_{kt-1} - p_{12}Z_{kt-2} - p_{13}Z_{kt-3}) \\ & + w_t \end{aligned} \quad (8^*)$$

$$\begin{aligned} A_t = & (1 - p_{21})b_0 + p_{21}A_{t-1} + b_1(D_t - p_{21}D_{t-1}) \\ & + \sum b_j(X_{jt} - p_{21}X_{jt-1}) + v_t \end{aligned} \quad (9^*)$$

¹³The nonlinear estimation of the legislative success equation would not converge using the third order auto-

Table 4 provides a picture of approval dynamics which is very similar to that in Table 3. Despite the relatively large value of p_{21} , all but two of the coefficients are statistically significant, in the predicted direction, at the .05 level. The two major types of changes in coefficient values are the drop in b_1 from .371 to .154 and the attenuation of several of the coefficients of the unanticipated variables. Note that the R^2 drops only slightly, and the standard error is of similar magnitude. The goodness-of-fit statistics in Table 4 have been calculated *without* p_{ij} and the lagged values of D_t and A_t . Consequently, they reflect the goodness of fit for equations (8) and (9) after the effects of serial correlation have been removed.

Turning to an examination of the legislative success equation, all of the variables are statistically significant in the predicted direction at the .01 level. Note that the a , coefficient is now statistically significant in the predicted direction once the serial correlation has been taken into account.

Assessing Model Implications

The four basic issues that were raised in the introduction will be investigated in order. To evaluate the substantive explanation offered by the estimated version of the promise and performance model, the impact of the individual variables on success and approval will be assessed. Partial regression coefficients will be examined as well as the impact range of each variable. The latter specifies the minimum and maximum values of the quantity— b *variable—where b (or a) is the partial regression coefficient. The impact range indicates the magnitude of the direct effect that each variable, when weighted, has on success and approval.

Popular Support as a Presidential Resource

The parameter estimates for the legislative success equation confirm our initial observation that public approval operates as a vital presidential resource. As Table 4 shows, the approval coefficient is statistically significant and implies that the cumulative rate of roll-call victories will decline by three points for every ten-point drop in approval. This impact is quite substantial. The impact range

regressive specification for f_t . This is not unusual because convergence is not guaranteed (Amemiya, 1974). Convergence did occur for an AR(2) process specification. The results reported in Table 4 are for the latter specification. Even though there is some serial correlation in equation (8*), this* will have no impact whatsoever on the nonlinear two-stage least squares estimates of the approval equation.

Table 4. Nonlinear 2SLS Estimates (with Correction for Autocorrelation)

Variable	Descriptor	Coefficient	S.E.	t-Ratio	Impact Range
Approval equation					
D_t	Legislative success	.154	.047	3.28	[1.06, 9.70]
X_{2t}	Legislative activity	-.032	.009	-3.45	[-13.00, 0]
X_{3t}	Conflict/cooperation with USSR	-1.627	.823	-1.98	[-1.58, 3.19]
X_{4t}	Economic misery	-1.849	.266	-6.95	[-29.71, -.55]
X_{5t}	Cor. battle deaths	-17.038	3.699	-4.61	[-21.01, 0]
X_{6t}	Change in X_{5t} due to new president	11.667	4.367	2.67	[0, 14.39]
X_{7t}	Sympathy	7.278	2.047	3.56	[0, 7.28]
X_{8t}	Crisis	5.640	1.168	4.83	[0, 5.64]
X_{9t}	Social unrest	-1.791	1.728	-1.04	[-1.79, 0]
X_{10t}	Scandal	-12.437	1.987	-6.26	[-12.44, 0]
X_{11t}	International policy	4.308	1.418	3.04	[0, 4.31]
X_{12t}	Domestic policy	-.699	1.664	-.42	[-.70, 0]
X_{13t}	Personal events	7.069	1.580	4.47	[0, 7.07]
Constant		61.109	2.974	20.53	
p_{21}		.802	.034	23.56	
R^2		.792			
S.E.		6.082			
d		1.962			
Legislative effectiveness equation					
A_t	Approval	.300	.068	4.30	
Z_{2t}	Control of Congress	18.863	2.332	8.09	
Z_{3t}	Biannual activity	-.018	.007	-2.68	
Constant	Constant	46.934	4.508	10.41	
p_{11}		.604	.054	11.18	
p_{12}		.159	.050	3.21	
R^2		.619			
S.E.		9.283			
d		1.745			

reported in Table 4 indicates that the difference between the success rates for the high (80.4%) and low (24.0%) values of approval in our sample is approximately 17 points (or 17 defeats for every 100 roll-call votes). A consideration of the last four presidential administrations in our sample (Johnson to Carter) further illustrates the value of public support in the legislative arena. Each of these presidents experienced at least a 30-point decline in approval during the course of their terms. The estimates in Table 4 imply that such drops in approval lowered the victory rate of each president by 9 points (taking notice into account). Assuming for the purposes of illustration that Z_3 is equal to 350, the 30-point decline in approval translates to roughly 31 additional roll-call defeats.

The estimates in Table 4 also show that the measures of party control and activity exert a significant impact on the success rate. Control of the

Congress by the president's party will increase the average success rate by 18.9 points. Increases in activity will produce a decline in success at the rate of 1.8 points for every 100 presidential position votes.

These results not only corroborate but also extend the findings of previous research (e.g., Edwards, 1980; Rivers & Rose, 1981) on presidential success in the legislative arena. There are three noteworthy points in this regard. First, the estimates show that the "public" and "legislative" presidencies are linked via the reciprocal relationship between approval and success. Although prior research has emphasized the role of approval in legislative success, our results also show that presidential effectiveness in the legislative arena is an important component in maintaining public support. Second, the analysis demonstrates that the impact of actions in the legislative arena is twofold—the penalty of initiation and the

reward of effectiveness. Third, this suggests that there is a strategic component in formulating the president's legislative agenda. The estimate for Z_3 implies that more ambitious legislative agendas demand higher levels of approval to sustain a given level of success. Thus submitting a laundry list of initiatives created to satisfy electoral constituencies is not cost free; unless the president can maintain his level of public support through other means, the list will stimulate the rate of defection and produce no offsetting reward.

The Determinants of Popular Support

The Individual Variables. Turning first to the anticipated, occupant-based variables, it is clear that they have a significant impact on presidential approval. A president's domestic legislative success has a consistent impact on presidential support. As estimated, b_1 implies that approval will decline 1 point for every 13-point drop in legislative success (assuming external efficacy is at 50%).¹⁴ Presidential domestic activity has a significant and negative impact on the president's current approval rating. As estimated, approval drops one point for every 57 bills introduced in a term. Together these results imply that the penalty for policy initiation implied by the coalition-of-minorities can be counterbalanced by legislative success and thereby produce a net benefit for policymaking.

Turning to X_3 , it can be seen that the conflict or cooperation directed toward the USSR has a significant impact on popular support. In those instances in which the United States directs conflict toward the USSR, a president's approval will increase up to four points. Although it is possible for a similar decrease to occur whenever the president is cooperative toward the USSR, the 1953-1980 period suggests that a president is cooperative only when the public's attention is not focused on international problems. Thus, the penalty for cooperation is reduced.

This variable implies that presidential actions in the superpower arena do have an impact on approval, especially when foreign affairs dominate the public's awareness. Even though the maxi-

mum impact of directed behavior toward the USSR is not great, it does indicate that a president is better off acting in a truculent fashion than he is in developing a solid base for cooperation. Therefore, insofar as U.S. foreign policy is concerned, the public reacts most strongly and positively to confrontation with the USSR.

In terms of the anticipated and institutional variables, it is clear that the economy, war, and presidential succession have a substantial impact on popular support. The weighted misery index, X_4 , has a persistent impact on popular support. The coefficient implies that approval will decline by 1.85 rating points for each one point increase in the weighted misery index. The impact range of the variable indicates that the economy has, on occasion, cost the president over 29 rating points. These findings support the general conclusion that economic conditions are very important determinants of presidential approval.

It is noteworthy that high unemployment or inflation are not, in and of themselves, necessary conditions for the erosion of popular support, however. Instead, high levels of the two measures and a substantial degree of public concern with economic problems are required. According to the estimated version of the model, for example, the economic problems of the late 1950s (when the misery index exceeded 10) cost President Eisenhower significantly fewer points (7.3 maximum impact) than the penalty imposed on President Nixon for similar conditions in the 1970s (maximum impact of 21.5 rating points). These results suggest that, insofar as political economy is concerned, the president has at least one additional lever at his disposal. Namely, he can try to deflect attention away from the economy when things are bad. Although such a strategy is likely to be difficult, it does offer the president another option.

The two war variables provide an interesting picture of the impact of war on popular support for the president. The coefficient of X_5 indicates that war had a very substantial impact on President Johnson. The impact range suggests that the war cost him increasingly and finally culminated in a decline of 17 rating points during 1968. The direct impact of war on Eisenhower and Nixon was reduced according to the coefficient of X_6 . Because they inherited the war, the impact was reduced substantially so that its maximal impact on Eisenhower was 4.6 points and on Nixon was 5.3 rating points. There are two major implications of these results. First, unlike previous research, we are able to demonstrate that Eisenhower, Johnson, and Nixon were all adversely affected by war. Second, the results also demonstrate that these presidents were penalized differently, with the difference being attributable to both a reduction in the proportion of the public

¹⁴It is important to remember that each of the presidentially relevant variables is weighted by a factor that accounts for the number of people who notice the factor. Therefore, when calculating the impact of a given change, the change must be discounted by the notice factor. In this particular instance, we are interpreting the impact under the assumption that 50% of the population notices the change.

who noticed the war and the fact that both Eisenhower and Nixon inherited their wars.¹⁵

The impact of X_7 , suggests that the sympathy associated with presidential succession does have a sizable impact on the level of presidential approval. On average, the caretaker president is awarded an increase of 7.3 rating points until he fills out his predecessor's term. Consequently, the evaluation of the successor is substantially different from that of his predecessor.

Six variables constitute the unanticipated subset of the presidentially relevant variables set. The maximum value for each is 1.00, which occurs whenever the kind of outcome receives front-page coverage in *The New York Times* throughout the base period. In the case of continual coverage, an international crisis leads to a rise of over five points, domestic unrest leads to a drop of 1.7 points, a scandal leads to a drop of over 12 points, international policy announcements and changes generate an increase of about four points, domestic policy changes lead to an insignificant drop, and periods of personal hardship lead to increases of approximately seven points. It is noteworthy that the three approval-enhancing unanticipated variables have a similar order of magnitude impact on approval.

The impact of unanticipated events that receive less than continual coverage is reduced in proportion to the magnitude of the coverage. The strength of these estimates provides support for the proposition that irregular events have a substantial impact on the public's assessment of the president. Furthermore, the impact is similar across time. Nowhere is this more startling than in the case of the scandal variable. The results suggest that there is a commonality across all scandals; in this sense, Watergate was not a unique occurrence but, rather, a particular example of the general proposition that perceived violations of integrity will reduce the level of public support.

Insofar as the estimated impact of Watergate is concerned, two points are worth noting. First, many analysts of popular support have attributed the enormous drop in Nixon's approval to the Watergate scandal. The estimated version of our model indicates that the maximum direct penalty was 12.44 points. The reasonableness of this estimated coefficient is underscored, we think, by the fact that all subsequent presidents (i.e., Ford, Carter, and Reagan) have been within 12 points of Nixon's low point without a scandal. Second,

because the economy accounts for a majority of the drop, our model suggests that Nixon's second-term popular support would have dropped dramatically without a hint of scandal.

The Overall Explanation. Using the structural estimates in Table 4, it is possible to account for the fact that the time path of approval during the 1964-1980 period is significantly different from the 1953-1963 period. During the 1953-1963 period both Eisenhower and Kennedy were able to maintain relatively high levels of public support over time with a relatively small amount of variation. The question is: How did these presidents accomplish such a feat?

The promise and performance explanation provides four answers. First, foreign policy issues were dominant. This configuration of public concerns was highly beneficial to Eisenhower and Kennedy for it not only diverted attention from the economy and divisive domestic problems, but it created a context in which the international arena would be noticed and evaluated in a favorable light. Second, the values of X_3 , demonstrate that the actions and rhetoric that these administrations directed toward the USSR were, particularly when compared to later years, primarily conflictual in nature. This aggressive stance reinforced the prevailing structure of public concerns and increased the public support enjoyed by these presidents. Third, both presidents were the beneficiaries of the rally effect generated by approval-enhancing events. Crises, diplomatic activities, and personal events were quite frequent during this period and, as a result, the overall impact of unanticipated factors was positive in nature. Finally, the period from 1953 to 1963 is distinguished by relatively high levels of efficacy (Abramson, 1983). Both presidents were evaluated by a constituency that was substantially less cynical than those faced by their successors. For these reasons, we conclude that there is no "Eisenhower phenomenon" per se; rather, the Eisenhower and Kennedy popularity levels are a result of a relatively efficacious public primarily concerned with foreign policy issues, presidential behavior that reinforced these priorities, and the frequent occurrence of approval-enhancing events.

The dominant feature of the 1964-1980 approval levels is that they exhibit substantially more volatility than the 1953-1963 levels. This change is primarily attributable to the quality of the outcomes experienced by the public during this period. In rather rapid succession, the public endured a prolonged and indecisive war, a scandal of unprecedented magnitude and duration, and a period of substantial economic problems. The impact ranges presented in Table 4 graphically

¹⁵It is interesting to consider the implications of these estimates for the impact of the Korean War on Truman. The estimated coefficient suggests that the maximal impact of the Korean battle deaths on Truman was 18 rating points. This is almost identical to the impact of the Vietnam War on Johnson.

illustrate the penalty imposed on presidents for their failure to satisfy the institutional expectations of peace, prosperity, and integrity.

The overlap of war, economic distress, and scandal during this period generated two additional consequences. First, public attention shifted from a consensual to a divisive set of concerns and thereby dissolved the favorable context in which Eisenhower and Kennedy had operated. Second, as several studies (e.g., Abramson, 1983) suggest, the dissatisfaction generated by war, scandal, unemployment, and inflation was generalized and led to the erosion of political efficacy. As a result, presidents since Kennedy have presented and defended their actions to an increasingly skeptical and less naive audience. Our model illustrates that prominent results of this change include a reduction in the net benefit (i.e., the reward for success less the penalty for action) of successful policy initiation and thus the value of being perceived as an effective policy maker.

The ability of the model to explain the pattern of public support in these two periods leads us to the conclusion that the "throwaway presidency" (Greenstein, 1982) is not an inevitable feature of modern government. Our results show that the dramatic erosion of support experienced by Johnson, Nixon, Ford, and Carter was not generated by some inescapable dynamic; rather, it was a product of the interaction between outcomes and public attention.

A Potentially Vicious Circle

To assess the impact of the reciprocal relationship on presidential support, we turn to the derived reduced form equations. It is necessary, therefore, to solve the structural equations so that each endogenous variable is expressed as a function of the exogenous variables. Applying this procedure to equations (8) and (9) yields the following derived reduced form equations:

$$D_t = \{1/(1-a_1b_1)\} [a_0 + a_1b_0 + \sum a_k Z_{kt} + a_1 \sum b_j X_{jt} + a_1 e_t + f_t] \quad (12)$$

$$A_t = \{1/(1-a_1b_1)\} [b_1a_0 + \sum b_j X_{jt} + b_1 \sum a_k Z_{kt} + e_t + b_1 f_t]. \quad (13)$$

The reduced form coefficients are the impact multipliers, which indicate how the path of the exogenous variables influences the short-run behavior of the endogenous variables. The weight, $\{1/(1-a_1b_1)\}$, represents the amplification that results from the reciprocal relationship.

Substituting the estimated value of a_1 and b_1 into this expression shows that the impact of each exogenous variable is amplified by 5% as a result

of the reciprocal relationship. Multiplying the structural coefficients for the exogenous variables in the approval equation by 1.05 thus provides a measure of their total (direct and indirect) effect on each of the endogenous variables. Such an exercise leads to three conclusions. First, the amplification created by the reciprocal relationship is rather modest: in other words, the circle is not particularly vicious. For example, the total impact of a 10-point change in the economic misery index will reduce the level of public support by 19.4 points (18.4 direct and 1 point via the indirect enhancement). The relatively small magnitude suggests that, in the short run, the reciprocal relationship will not create an irreversible downward momentum in public support. Second, it is possible to estimate the effect that the exogenous determinants of public support exert on the legislative success by combining the impact multiplier ($1.05 \times \text{coefficient}$) with a_1 , the approval coefficient. This procedure shows, for example, that a 10-point decline in economic misery will reduce legislative success by approximately 6 points. Third, although modest, the simultaneous relationship underscores the importance of approval as a resource. Drops in A_t will produce drops in D_t , which, in turn, will fuel further declines in A_t . Thus, a period of adverse change in outcomes may produce a vicious circle in which both the support and presidential influence over decision makers declines and weakens the president's ability to influence the outcomes for which he is held accountable.

Simulating Popular Support

The estimated promise and performance model provides the base from which it is possible to use the model to generate inferences about the path of presidential approval outside the 1953-1980 sample period. In doing so, we have opted to use all of the information at our disposal. Specifically, we utilize a simulation procedure that uses information about both the structural and disturbance term relationships. Our goal is simply the most accurate forecasts possible. Therefore, the forecasts are generated from the derived reduced form equations associated with equations (12*) and (13*).¹⁶

The hallmark of any model is an ability to account for data that are not part of the sample used to estimate the model parameters. Accord-

¹⁶This can be accomplished by taking the equations from note 12 and deriving the reduced form. As stated in note 13, an estimate for p_{13} could not be obtained; consequently, we have set $p_{13} = 0$ to derive the reduced form for the simulation.

ingly, our empirical evaluation will be augmented by applying the model to the first three years of the Reagan administration and conducting a forecast evaluation. The Reagan term offers an especially rigorous test because the course of his popular support has been highly unusual, particularly when examined in light of the inevitable erosion theme. Reagan's initial approval level was 51%, the lowest starting level in the history of the Gallup poll. It rose steadily to 68% in May, 1981, and from that point fell steadily until it reached 36% in January, 1983. This decline was reversed as Reagan's support rose to 55% in December, 1983. This reversal is noteworthy for two reasons. First, no president since Eisenhower has earned a higher approval rating after three years in office. Second, Reagan was the first president in the history of the Gallup poll to drop below 40% and then sustain a 50% + level.

As a first step in the forecast evaluation, the required monthly data were collected for the period from January, 1981, to December, 1983. All variables were calculated according to the same rules used in the estimation phase of the analysis. Thirty-six *ex post* forecasts were generated by combining coefficients displayed in Table 4 with monthly data from 1981-1983. The resulting forecasts are presented in Table 5. The results indicate that the ability of the model to track Reagan's level of approval is quite good. The model captures the early increase in approval which resulted from the attempted assassination as well as the general decline in support from late 1981 to January, 1983. The model does not predict the jump in approval recorded in the months before the assassination attempt and underpredicts the support levels between February and August, 1981. Most gratifying, however, is the ability of the model to predict the substantial rise

in Reagan's support during 1983. There were three developments in 1983 which explain this recovery. The first is an improved economy. After an initial rise in 1983, the misery index declined from 15.4 in June to 11.8 in December. Second, the adverse impact of the economy was further reduced owing to a marked shift in public attention. Between March and December, 1983, the proportion of the public citing inflation or unemployment as the most important problem dropped from 79% to 43%, whereas the percentage identifying foreign affairs rose from 6% to 37%. Third, several crises in late 1983—the Soviet downing of KAL 007, the bombing of the Marine compound in Beirut, and U.S. military action in Grenada—triggered a substantial rally effect. The result of these developments was to create a political context more reminiscent of the Eisenhower-Kennedy years than the context that confronted Reagan's more immediate predecessors.

In sum, the model produces accurate *ex post* forecasts which offer a plausible explanation for the time path of Reagan's public support. This judgment is reinforced by the measures conventionally used to evaluate *ex post* forecasts. The .91 correlation between actual and predicted values and a root mean square error of 3.21 rating points provide strong grounds for concluding that the forecasting performance of the model is very good. The fact that the model is able to generate forecasts of approval outside of the sample period, coupled with the accuracy of these forecasts, suggests that the model has substantial potential for generalizing about future presidents. As such, it stands in marked contrast to previous models, which cannot forecast owing to the presence of term-specific variables and coefficients.

Conclusions and Implications for Subsequent Research

On the basis of this empirical investigation, the promise and performance model has been shown to 1) provide a very accurate explanation for public support during the 1953-1980 period, 2) account for the differences in texture from one administration to another, and 3) generate accurate *ex post* forecasts. Thus, the evaluation of the model offers reliable and extensive evidence in support of what Kernell (1978, p. 520) has termed the "conventional political wisdom." These results indicate that public assessments of presidential performance are anchored in the real world; that is, approval of the president depends upon the quality of social, economic, and international outcomes experienced by the public.

This research also leads to more elaborate and specific description of this conventional wisdom.

$$D_t = \{1/(1-a, b_1)\} [(1-p_{11}-p_{12})a_0 + (1-p_{21})a_1b_0 + (p_{11}-b_1a_1p_{21})D_{t-1} + p_{12}D_{t-2} - a_1(p_{21}-p_{11})A_{t-1} - a_1p_{12}A_{t-2} + \sum a_k(Z_{kt}-p_{11}Z_{kt-1}-p_{12}Z_{kt-2}) + a_1 \sum b_j(X_{jt}-p_{21}X_{jt-1}) + a_1w_t + v_t\} \quad (12^*)$$

$$A_t = \{1/(1-a, b_1)\} \{(1-p_{11}-p_{12})a_0b_1 + (1-p_{21})b_0 + (p_{21}-b_1a_1p_{11})A_{t-1} - b_1a_1p_{12}A_{t-2} + b_1(p_{11}-p_{21})D_{t-1} - b_1p_{12}D_{t-2} + b_1 \sum a_k(Z_{kt}-p_{11}Z_{kt-1}-p_{12}Z_{kt-2}) + \sum b_j(X_{jt}-p_{21}X_{jt-1}) + b_1v_t + w_t\}. \quad (13^*)$$

Note that this procedure is similar to that suggested by Pindyck and Rubinfeld's (1981, pp. 593-605) discussion of transfer function models.

Table 5. Ex Post Forecasts, 1981-1983

		Actual A_t	Predicted A_t	Prediction Error
1981	January	51.0	52.0	-1.0
	February	53.0	46.2	6.8
	March	60.0	52.8	7.2
	April	67.0	61.9	5.1
	May	68.0	65.0	3.0
	June	58.7	57.6	1.1
	July	58.7	56.0	2.7
	August	60.0	57.0	3.0
	September	52.0	57.4	-5.4
	October	54.5	53.8	0.7
	November	52.0	55.4	-3.4
	December	49.0	49.2	-0.2
1982	January	48.0	51.1	-3.1
	February	47.0	47.9	-0.9
	March	46.0	45.5	0.5
	April	44.0	48.0	-4.0
	May	44.5	44.4	0.1
	June	44.5	47.1	-2.6
	July	41.5	43.8	-2.4
	August	41.3	43.2	-1.9
	September	42.0	45.7	-3.7
	October	42.0	42.0	0.0
	November	43.0	44.8	-1.8
	December	41.0	44.0	-3.0
1983	January	36.3	36.1	0.2
	February	40.0	44.1	-4.1
	March	41.0	40.5	0.5
	April	42.0	44.6	-2.6
	May	44.0	41.5	2.5
	June	45.0	43.9	-3.9
	July	43.0	46.9	-3.9
	August	43.5	44.1	-0.6
	September	47.5	47.5	0.0
	October	45.0	51.7	-6.7
	November	53.0	52.5	0.5
	December	55.0	59.4	-4.4

R (Actual with predicted) = .91

$RMSE = 3.21$

First, it illustrates that the president is held accountable for a broader range of outcomes than suggested in prior research. Although ignored in previous accounts, both the president's legislative performance and his actions toward the USSR operate as important determinants of public support. Additionally, personal setbacks, domestic unrest, diplomatic initiatives, and allegations of wrongdoing constitute general classes of events which can be expected to capture public attention and thus influence the path of public support. The discovery of a reciprocal relationship between public support and presidential fortunes in Congress also integrates two related branches of

research. More important, this relationship means that studies of one can no longer ignore the other.

Further, the research demonstrates that the manner in which outcomes influence public support will depend upon the prevailing concerns and attitudes of the public. The results show that the manner in which political stimuli are perceived and filtered by the public is itself a dynamic component of the support decision. For example, the erosion of public support produced by departures from peace and prosperity will depend upon the degree to which public concern is concentrated upon these fundamental expectations; similarly, the reward that flows from an aggressive stance

toward the USSR will be enhanced as public attention increasingly focuses on foreign affairs. With respect to public attitudes, the impact of both legislative activity and success will be enhanced or reduced as the level of efficacy changes. Most significantly, the various contexts created by configurations of these attitudes and concerns are not equally beneficial to the president. Our research suggests that the president's ability to maintain public support and the influence that it produces will be enhanced as the public grows more efficacious and preoccupied with foreign affairs.

Several implications follow from this elaboration of the conventional wisdom. From the perspective of democratic theory, we find that presidents are held accountable by the public. Because the PROs are linked to concerns that impinge upon citizen well-being, presidents must operate before a public that is quick to reward and to punish; it is not, as some accounts suggest, a constituency indifferent to politics or unaware of the political environment.

This accountability assumes a specific and limited form, however. The ability of the presidentially relevant variable set to explain the pattern of support suggests that accountability hinges primarily upon results.¹⁷ This provides a president with certain advantages. The range of instruments available to a president does not appear to be severely constrained by this relationship and, therefore, presidential innovation and experimentation are not circumscribed. There are costs imposed on the president as well. To the extent that problems are intractable or disapproval-enhanc-

ing events uncontrollable, the president will be penalized by this accountability.

Perhaps most important, the results allow us to address a fundamental issue pertaining to public opinion and the presidency. The question revolves about the possibility and methods of presidential management. First of all, our results imply that management is possible. We have shown that the time path of public support is not governed by some repeatable and unalterable term-based dynamic. Rather, it is experiential and evolves from the president's track record in satisfying public expectations. Decay is not inevitable and any decline in public support can be reversed. Thus, we are led to the conclusion that public support is amenable to presidential management and influence.

To say that management is possible does not, however, imply that it is simple. Quite obviously, the president's fundamental task is to move the outcomes for which he is held accountable in a desirable direction. Contrary to the assumptions employed in some analyses, the task is more complex than choosing a lever and deciding when it will be pulled. The problem confronting the president, particularly in the realm of economic decision making, is that his authority to act is constrained by the control that Congress exercises over budgets and program authorization. In this sense, presidential attempts to influence outcomes will depend upon an ability to persuade, an ability that itself is tied to the prevailing level of public support.

The president is not without recourse, however. Although his ability to influence the determinants of public support is constrained in some areas, it is comparatively unconstrained in others. The president has a relatively free hand in the diplomatic and military agenda, and our results indicate that summitry as well as sabre rattling can be expected to enhance the president's public support. Although the political benefits of such actions are transitory, the president can engage in these forms of political drama (MacKuen, 1983) to create windows of opportunity for increasing his leverage over other decision makers and thereby influencing governmental actions under their control. Reliance on the political stage may also prove useful for presidential attempts to alter the concerns of the public, another strategy that is implied by our analysis. In this regard, the president's objective is to divert attention from divisive concerns and focus or reinforce attention on problems (e.g., the USSR) that produce a more benevolent context for evaluating the president's efforts.

In sum, our analysis emphasizes that presidents have a strong incentive to influence public support and feasible strategies for engaging in this

¹⁷Our analysis also raises a question about the extent to which support may be a function of the methods and programs selected by a president to influence the environment. Essentially, this question revolves about the relative impact of outcomes versus ideology on public support. It is not possible, however, to address this concern in the context of an aggregate model. Yet, given suitable data, two possible effects of ideology can be investigated. First, ideology may influence the individual's propensity to support the president in an *a priori* sense. To use the Reagan case as an example, conservatives may be predisposed to support the president regardless of outcomes. Second, ideology may also mediate the impact of outcomes in the sense of reducing or enhancing their effects. Thus, we would expect the reaction coefficients for each PRO to be different for liberals and conservatives. In the Reagan case, we would anticipate that liberals will display a tendency to discount outcomes that are improving and to magnify those that grow worse. A research design for testing these propositions would require disaggregated data at the individual level for each ideological subset. Unfortunately, such data are not available at this time.

task. It also suggests that public support will operate as a determinant of presidential decision making. The level of public support may not only expand or contract the range of decision alternatives available to a president (Light, 1982; Neustadt, 1980), but it also influences the president's choice and timing of alternatives (e.g., Ostrom & Job, 1982; Rohde & Simon, 1983). This, in turn, suggests that the study of public support is simultaneously a study of presidential decision making. Thus, it becomes important to study the manner in which public support influences the president's substantive goals, his agenda for action, and his choices to use the prerogatives of the office. It is our belief that such research will show more clearly that it is the interaction of citizen and presidential decisions which acts as the driving force underlying public support. As such, a growing concern with presidential decision making promises to extend the prevailing research agenda on modern presidential government.

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Throwing the Rascals Out: Policy and Performance Evaluations of Presidential Candidates, 1952-1980

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This article explores two dimensions of public evaluations of presidential candidates on the basis of open-ended survey questions from 1952 to 1980. The first dimension looks at whether citizens evaluate candidates on the basis of policies, performance, or strictly candidate attributes; the second examines the time perspective of these assessments, that is, whether they are retrospective or prospective. It is found that incumbents have been judged primarily on the basis of retrospective performance, challengers on prospective policy, and candidates running in nonincumbent races on prospective performance. Throughout the period from 1952 to 1980 both policy and performance considerations have become increasingly related to the vote. Except for 1964, performance has outweighed policy as a predictor of the vote, with an emphasis on retrospective evaluations whenever an incumbent runs for reelection and on prospective assessments in nonincumbent races. The 1964 case provides the best example of a policy mandate, with the 1972 election also fitting the pattern to a lesser degree. The data for the 1980 election, however, fail to support the claim of a mandate for Reagan's policy stands.

The topic of issue voting is one of the most important and widely researched subjects in the study of American political science. The extent to which issues affect electoral choices has been a controversial question for decades, and with the advent of an extended time series of adequate survey data, a heated debate has also arisen concerning whether the electorate is now more issue oriented than in the past (see Asher, 1980, chap. 4; Converse, 1975; Kessel, 1972 for insightful reviews of this literature).

One of many problems in the issue voting controversy is defining what we mean by issues in the first place. Virtually any topic that is discussed in a political campaign involves a debatable point between the candidates and can therefore be considered an issue. As Stokes (1966) has pointed out, even matters on which the candidates agree about basic goals such as peace and prosperity can be considered issues. These he termed "valence issues," in that they link the candidates with some condition that is either positively or negatively valued by the electorate. What is critical in this case is not the means by which a problem is solved

(i.e., policy preferences), but rather whether or not the problem is solved (i.e., performance in office). Such a distinction is critical for drawing appropriate substantive interpretations from election outcomes as well as for evaluating the functioning of democratic systems. In the former, a policy mandate is conveyed by the voters; in the latter, voters merely reward or punish based on performance considerations.

Another crucial dimension underlying theoretical considerations of issue voting is the electorate's time perspective, as Fiorina (1981) and Abramson, Aldrich, and Rhode (1982) have recently reminded us. The concern here is whether voters make decisions retrospectively, looking to the past, or prospectively, holding expectations for the future. If voters focus solely on the past, then they can only ratify or reject, whereas if prospective voting occurs, then they can also guide in the form of a mandate.

These conceptual distinctions between retrospective versus prospective and policy versus performance voting are thus of great significance. At the individual level these dimensions hold implications for voter rationality and information availability (see Fiorina, 1981). For example, the information-seeking costs necessary to evaluate the past performance of an administration are far less than those needed to assess a set of future government policies. At the collective level, inter-

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pretations of election outcomes often depend on whether people vote retrospectively or prospectively on the basis of policy or performance. These concerns loom particularly large after an incumbent is defeated and the victor inevitably claims a mandate to redirect government policy. But does the electorate really consider a challenger's program and promises for the future, or do voters merely throw the incumbent administration out if it is unable to produce satisfactory outcomes to the nation's problems? The major substantive works on the subject leave us with contradictory answers and a good measure of conceptual confusion.

V.O. Key's work on the subject has been profoundly influential and is usually interpreted as placing "primary emphasis on policy outcomes rather than the means that lead to those outcomes" (Fiorina, 1981, p. 194). Yet Key's writing at times suggests that the focus of issue voting could be either policy or performance. For example, he writes:

If public opinion expresses itself with relative clarity in retrospective disapproval of performance or policy, it may also express itself in the same manner in confirmation or ratification of past policy or performance. Only infrequently is a new program or a new course of action advocated with such force and the attention it receives so widespread that the polling may be regarded as advance approval of a proposed course of action (1961, p. 474).

Downs (1957), on the other hand, gives greater weight to policy alternatives as the basis of the vote decision. Fiorina (1981, p. 13) summarizes the differences between Downs and Key by saying:

Under the Downsian view, elections have policy implications. . . . The Downsian citizen compares the challenger's and the incumbent's platforms, interpreting the matter in light of the incumbent's past performance. But under the traditional theory (V.O. Key), elections have no policy implications other than a generalized acceptance or rejection of the status quo.

Fiorina's use of the term "performance," however, appears to correspond more closely with Key's meaning—how effectively a problem is being handled—than with the Downsian notion of performance. Indeed, although Downs's use of the term "performance" is less than totally unambiguous, it seems clear that performance evaluations deal with a comparison of the policies associated with different parties or candidates. In fact Downs (1957, p. 43) defines the term "performance rating" as involving a comparison of "the utility incomes (voters) are actually receiving with

those they would be receiving if the *ideal* government were in power." Performance ratings, he suggests, are employed by voters when both parties *currently* have identical policies and platforms or the policies of different parties produce identical utility incomes.

Thus when Key and Downs use the term "performance," they obviously have different concepts in mind. Voting for a party because it has worked hard toward peace is not the same as supporting that party because its policies or programs are considered to be the best means of producing peace. Regardless of whether the comparison hinges on current platforms or the preferred positions of an ideal government, any vote decision based on consideration of alternative programs is policy rather than performance voting. Performance rating in the Downsian sense, therefore, seems to be a misnomer. For the purpose of conceptual clarity, Key's definition of performance assessments appears preferable in our judgment.

Turning to the time dimension, Key was a clear proponent of retrospective voting. According to Key the vote decision usually does not reflect a considered endorsement of a proposed set of policies nor does it typically suggest anticipated future performance. In contrast, using the past as a guide to future expectations is a feature of both Downs's and Fiorina's thinking on the subject. If incumbents are judged by the electorate to have done poorly in the past, then they will not be expected to perform well in the future. Of course the incumbent may still be preferred when compared with the expected future performance of the challenger. In short, the past performance of both competing parties is compared and projected onto what the parties can be expected to do in the future. In fact Fiorina (1981, p. 197) states that "In analysis after analysis, future expectations dwarfed the effects of retrospective judgments."

In summary, from V.O. Key's model of retrospective voting emerges an image of voters as an avenging lot who reward or punish the incumbent administration for its past performance. Downs, on the other hand, tends to emphasize policy instruments combined with a concern about which party will provide the maximum satisfaction in the future. Projections into the future, but regarding outcomes rather than the means to attaining those results, form the core of the vote decision for Fiorina.

Assessing the Validity of Available Measures

Despite the significant theoretical implications associated with the two dimensions differentiating retrospective from prospective and policy from performance voting, little attention has been devoted to ascertaining whether or not these

distinctions are empirically verifiable. Although Fiorina and Abramson et al. are sensitive to the importance of adequately measuring this two-dimensional structure, they devote virtually no time to demonstrating the validity of the indicators used to operationalize these concepts. Face validity is surprisingly the only criterion used by these investigators to classify survey questions as measuring retrospective rather than prospective, or policy rather than performance assessments; the personal judgment of these social scientists and the language of the survey questions are the only evidence to support these important theoretical distinctions.

The weakness of this evidence is readily apparent from the lack of agreement in their respective classifications. For example, Fiorina (p. 138) concludes that the question about which party would best handle the most important problem facing the country is clearly prospective. Abramson et al. (p. 146), however, argue that this question could be either retrospective or prospective, and they caution that "some of the most important problems cited are less clearly retrospective than others. Comparability and interpretability problems thereby are raised." Similarly, these authors differ in their interpretations of the seven-point issues scales used in the election studies to measure proximity voting. Abramson et al. (chaps. 6 and 7) are quite emphatic in their use of these scales to measure prospective policy. Fiorina (p. 143), on the other hand, condemns the items noting that "the time perspective is hopelessly muddled. Thus use of the seven-point scales . . . confounds the attempt to analyze the comparative importance of retrospective judgments and future expectations."

Fiorina at least recognizes this validity problem. Throughout his book he tells us that available survey items seldom contain a clear time perspective or enable one to separate policy instruments from policy outcomes. Yet this apparent measurement confusion does not deter him from concluding that future expectations greatly outweigh the effects of retrospective evaluations. Likewise, acknowledged measurement problems do not prevent Abramson et al. from concluding that the 1980 election was evenly balanced between retrospective and prospective voting (p. 197). But how can we be at all certain that these conclusions are correct, given these measurement difficulties?

An initial step that can be taken in assessing the magnitude of these difficulties involves a more rigorous examination of the measurement properties and interrelationships among the available survey items. We therefore conducted a factor analysis of several items from the 1980 National Election Study that were thought to capture the two dimensions of Fiorina's retrospective voting

model. From this we can learn much about the individual items and, furthermore, ascertain whether the public in fact comprehends the world of politics according to the hypothesized latent dimensions.

In general the results of the factor analysis provide support for the hypothesized distinctions between policy and performance and prospective versus retrospective evaluations, as a four-dimensional solution was uncovered. The factor loadings for the varimax rotated solution shown in Table 1 suggest factors representing retrospective performance, prospective performance, personal retrospective evaluations, and prospective policy considerations.

The first dimension consists heavily of retrospective assessments of how well President Carter was handling his job and several specific governmental problems such as inflation, unemployment, and the hostage crisis in Iran. In contrast, the second factor is defined primarily by prospective items which ask how well Reagan *would* do on the economy, providing strong leadership and developing good relations with other countries if he were president. A comparable set of items asking how Carter would do fell onto the first factor, however.¹ Despite the future orientation of these questions, people apparently responded to them in a retrospective fashion, thereby demonstrating the weakness of their face validity. There is some overlap, though, between the two factors on Reagan's expected performance, indicating that people make some comparison of the incumbent's past record to the future potential of the challenger. A similar overlap is also evident on the questions concerning Carter's handling of the Iran and Afghanistan crises. Nevertheless, the evidence is clear enough to conclude that the public does distinguish between retrospective and prospective performance assessments of the candidates.

Evaluations of one's personal financial situation form yet a third distinct factor. These items which tap direct experience with economic conditions are labeled simple retrospective evaluations by Fiorina (p. 80). In contrast, assessments of how a given political actor or agency is dealing with a

¹The only difference between the Reagan and Carter items is that the former ask about how well the phrases describe "Reagan if he were president," whereas the latter ask about "Carter as president." Yet, work on survey research methodology shows that what is important is the direct question, which is clearly written in prospective terms, and not the introduction.

This illustrates how the election study often confuses the time dimension in its questions, although in this case respondents apparently sorted it out and focused on past performance.

Table 1. Factor Loadings for 1980 Survey Questions Presumed to Measure Issue Assessments^a

	Retrospective Performance	Prospective Performance	Personal Retrospective Evaluations	Prospective Policy
Presidential job approval	.78			
Carter handling inflation	.71			
Carter handling unemployment	.61			
Carter handling Iran	.60	.26		
Carter handling Afghanistan	.30	.20		
Government job on most important problem	.27			
Which party would handle inflation	.43	.46		.45
Which party would handle unemployment	.36	.50		.35
Which party would handle most important problem	-.42	-.53		-.29
Carter would solve economic problems	.76			
Carter would provide strong leadership	.81			
Carter would develop foreign relations	.64	.21		
Reagan would solve economic problems	-.29	.73		
Reagan would provide strong leadership	-.30	-.63		
Personal finances last year			.68	
Personal finances next year			.45	
Income stayed ahead of inflation			.65	
Individual hurt by inflation			-.48	
Business conditions last year	.37			
Business conditions next year	.26		.33	-.21
Carter increases defense spending	-.37	-.21		.34
Reagan increases defense spending				.40
Carter decreases government services				.49
Reagan decreases government services		.27		-.38
Carter cut taxes	-.22			-.29
Reagan cut taxes				.31

^aOnly loadings of .20 or larger are presented.

problem he terms mediated retrospective evaluations. We prefer to call these latter judgments politicized evaluations in order to differentiate them from personal retrospective evaluations for which that connection is not salient, such as those in the third factor.

These results appear to confirm Kinder and Kiewiet's (1979) contention of a separation between self-interest and collective judgments. However, our interpretation of this phenomenon differs somewhat from theirs. The factor loadings of the personal economic concerns coincides with the argument that people generally do not connect their own situation with politics because they are self- rather than system-blaming (Brody & Sniderman, 1977; Kinder & Kiewiet, 1979). The difference in interpretation comes with regard to the items presumed to measure collective orientation. The types of measures used by Kinder and Kiewiet to represent collective judgments (including the question on the state of the national economy) all load on the retrospective performance factor in Table 1. In short, the presumed collective evaluations are more strongly correlated with the vote

choice because they reflect *politicized* retrospective judgments, not because they are collective in content.

The final factor appears to reflect a concern with prospective policies. Contrary to Fiorina's contention, the seven-point scales are free of any contamination with evaluations of past candidate performance. Of course the results may have been different if the analysis had been based on proximity measures incorporating both the respondent's position and his or her placement of the candidates on the scale. However, the candidate placements alone are more theoretically appropriate to our concerns as the critical focus of prospective policy evaluations centers on the types of actions voters believe the candidates will take if elected. The factor loadings for these items suggest a set of citizen concerns focused on policy instruments in the future.

Much less clearly conveyed by the factor structure, however, is the reference for the set of items that ask which party would better handle various problems. Although Fiorina argues that these questions are future oriented, the factor analysis

demonstrates that they are thoroughly confounded with both retrospective and prospective performance, as well as overlapping with concerns about future policy actions. Yet the results do provide some evidence for Fiorina's notion that judgments of political parties are reflections of the past projected onto the future. At the same time, the results also confirm the suggestion that beliefs about which party will best handle a particular problem are shaped partly by evaluations of the past and expected future performance of the current candidates for office (Kinder & Kiewiet, 1979, p. 519).

In summary, Table 1 provides evidence for the contention that the public both differentiates a time perspective and distinguishes between policy and performance when evaluating candidates for president. The results also show a common limitation in recent work on retrospective voting, namely that these studies are based on measures that are not well designed to distinguish either retrospective from prospective considerations or policy from performance concerns. Indeed, as Fiorina acknowledges, this problem plagues his whole project. Moreover, even the limited number of questions that can be shown to differentiate these dimensions properly occur primarily in only the very recent election studies, thereby preventing direct comparisons over an extended time span.

An Alternative Approach

In an attempt to overcome some of these limitations we use an alternative approach to the subject based on open-ended questions that ask respondents what they like and dislike about each of the presidential candidates. Because these questions have been asked in an identical fashion in all of the SRC/CPS studies since 1952, direct comparisons can be made across time. Furthermore, these questions are particularly useful for our purposes because they impose no constraints on how respondents conceptualize political issues.

There are, however, some limitations to the open-ended questions that we readily acknowledge. On each individual question many respondents fail to give any substantive response, and roughly 10% in any given year have nothing at all to say to any of the candidate questions. Thus our ability to investigate how people process information relevant to judgments of the candidates is constrained by the lack of articulateness among some respondents. Also, interviewers occasionally fail to probe sufficiently so as to code the answers properly according to our theoretical interests. For example, if someone simply says he likes Nixon because of Vietnam, we do not know

whether this is a policy or performance statement unless the interviewer elicits a further response.

Despite these limitations, the open-ended questions provide the richest set of available evidence for investigating issue evaluations over time. Indeed, the fact that the same limitations hold true in each year provides a control for these shortcomings. Changes that may be evident in the preponderance of retrospective rather than prospective or policy versus performance responses at different time points should thus reflect real differences and not methodological artifacts created by new or revised survey questions.

The only major drawback we faced in utilizing the like/dislike questions was that the data were not suitably coded for our theoretical interests. As coded in the ICPSR released datasets, the responses do not contain a time perspective, nor do they consistently separate policy from performance. Therefore we returned to the original handwritten transcripts of the interviews in order to recode the data—a very substantial task given that over 13,000 respondents have been asked these questions since 1952.

The coding scheme was designed to differentiate retrospective from prospective comments and policy from performance for each of the like/dislike questions. Rather than attempting to code each individual remark, we chose simply to code the complete answer to each question. Because some of the answers contained a mix of comments, we used a multiple category code with varying gradations of certainty. The time dimension code, for example, ranged from clearly retrospective through mixed to clearly prospective. Similarly, the second dimension was coded with a five-point scale running from clearly performance to clearly policy oriented.

On the second dimension we also included a set of code categories that refer to candidate attributes without any obvious policy or performance content. Many candidate attributes, such as past political experience, decisiveness in making decisions, and the ability to follow through on promises clearly convey performance-relevant information. However, other personal attributes such as age, speaking style, and charisma hold less obvious implications. To avoid clouding the distinction between policy and performance, responses about candidate attributes were assigned a separate code unless they contained an unambiguous policy or performance reference.

The task of differentiating a time perspective or separating policy from performance judgments may seem rather difficult in the abstract; in reality, though, most of the comments were relatively easy to code. The examples presented below are taken from the 1964 and 1980 studies, and illustrate the various categories:

Retrospective Performances

Dislike Carter: He didn't get the hostages out of Iran.

So many things—inflation, unemployment, Iran. The hostage situation is deplorable.

He just hasn't done anything right.

Dislike Goldwater: For dogcatcher he's okay. Can't see that he ever did anything good as senator.

Retrospective Policy

Dislike Carter: I don't understand why he cut back on food stamps. People in this country are still poor.

He used the surprise rescue attempt to get the hostages out when he said there'd be no military action. Why didn't he negotiate with them?

Dislike Reagan: I didn't like what he did as governor. His tax changes favored the super-rich not the middle class.

Like Johnson: His foreign policies are more American-oriented. He doesn't give those other countries so much money.

Prospective Performance

Like Reagan: He is the one man who can curb government deficits.

Dislike Reagan: The country will be at war with him as president.

Dislike Goldwater: He won't follow up on his promises.

Like Johnson: He's good with other countries. At least we won't have war with him.

Prospective Policy

Like Reagan: His plan for cutting taxes and reducing the size of government is a sure fire cure for inflation.

Like Carter:

I hope he does something different if reelected. He says older people will be helped by increasing Medicare. I think he's going to get workers to pay more Social Security.

Of course all the responses did not fit as clearly into the code categories as those shown above. Nevertheless, an intercoder reliability check, determined by having different coders duplicate the coding of 625 cases, showed a reliability coefficient of .87 for the time dimension and .80 for the policy/performance distinction. Such a high level of reliability gives us confidence in the quality of the data and reassures us that the concepts are meaningful descriptions of how voters organize their cognitions of candidates and issues. (For further details on the coding procedures, as well as the procedure for the calculating intercoder reliability, please see the Coding Appendix.)

Issue Evaluations of the Candidates, 1952-1980

Initially these data can be utilized to determine the extent to which the policy/performance and retrospective/prospective dimensions are used to evaluate presidential candidates. By aggregating data from the eight presidential election studies between 1952 and 1980 we find that candidate performance assessments have outnumbered policy considerations by nearly two to one. Whereas 41.6% of the open-ended responses focused on performance, only 23.7% were about policy instruments. Another 4.5% of the comments were mixed between policy and performance, and the remaining 30.2% centered purely on candidate attributes. These latter comments were the most likely to be positive (58.7%), adding yet another piece of confirmatory evidence for Sears's (1969, 1983) positivity bias theory. Of the performance responses, 54.2% were positive compared to only 48.5% for the policy comments. Thus, all other things being equal, candidates for office have the most incentive to draw public attention to their personal characteristics and the least incentive to highlight policy matters—a strategy that many have followed in the past.

These categories vary greatly in terms of the time referent that people use with them. Overall, 67.0% of the open-ended responses were coded as retrospective, 29.4% as prospective, and 3.6% as mixed. Virtually all of the candidate-attribute responses were coded as retrospective, with the only major exceptions being comments about Eisenhower's health or Reagan's age, which imply that they might not be able to complete their terms. Retrospective judgments dominated the performance category by slightly over three to

one, whereas the policy category was almost evenly divided between retrospective and prospective comments. One might expect retrospective responses to be more positive than prospective responses, as they are more likely to consist of attribute and performance references. However, this bias notwithstanding we find that only 50.4% of the retrospective comments were positive compared to 59.2% of the prospective comments. This finding is reflective of V.O. Key's notion that retrospective judgments often involve "throwing the rascals out." Looking to the future generally evokes more optimism.

Of course aggregating the data from all eight election studies hides many theoretically interesting differences concerning how individual candidates have been evaluated. On a general level, for example, one would expect that incumbents should be judged quite differently from challengers. Because they have a past record of achievement in office, incumbents are more likely to be evaluated on the basis of retrospective performance. Challengers have to be evaluated more prospectively, however, and with a less visible past performance record policy matters are more likely to be salient. More specifically, examining judgments of the individual candidates will shed light on a number of controversies concerning the role of issues in recent elections.

Table 2 displays the marginals on the policy/

performance dimension for all presidential candidates from 1952 to 1980 according to whether they were incumbents, challengers, or candidates in a race without an incumbent. As hypothesized, performance evaluations have generally been more salient than policy considerations in public likes and dislikes of incumbents, whereas just the opposite is the case for their challengers. Interestingly, performance assessments also dominate candidate evaluations in races without an incumbent running for reelection. Three possible explanations seem most plausible to explain this. First, in open races such as these it may be that the past performance of the candidate's party shapes his public image. This would be especially likely to occur when a candidate is associated with the incumbent administration, such as Nixon in 1960. Second, there is somewhat less incentive for candidates in nonincumbent races to emphasize policies compared to challengers of incumbents, as the latter need such proposals to put themselves on a more equal footing with incumbents whereas the former do not. Finally, it could simply be that when forced to make a choice between two candidates without presidential experience, voters find it much easier to judge who would do a better job than which policy course to pursue. Comparing two new and untested policy programs may require too much information processing for the average voter.

Table 2. Performance, Policy, and Attribute Responses in Evaluations of Presidential Candidates (%)

		Performance	Mixed	Policy	Attribute
Incumbents					
Eisenhower	1956	27.8	7.9	9.2	55.1
Johnson	1964	32.7	6.5	26.4	34.4
Nixon	1972	39.7	4.6	32.7	23.1
Ford	1976	50.5	2.9	14.1	32.5
Carter	1980	42.9	4.8	15.5	36.8
Mean		38.7	5.3	19.6	36.4
Challengers					
Stevenson	1956	11.2	13.1	15.9	59.7
Goldwater	1964	15.0	5.6	38.9	40.5
McGovern	1972	11.6	2.5	45.7	40.2
Carter	1976	31.9	2.1	21.8	44.2
Reagan	1980	20.0	3.6	38.1	38.3
Mean		17.9	5.4	32.1	44.6
Nonincumbent Races					
Eisenhower	1952	32.4	5.1	5.7	56.7
Stevenson	1952	28.9	9.7	8.4	53.1
Kennedy	1960	19.2	2.8	9.5	68.5
Nixon	1960	35.3	4.2	10.0	50.4
Nixon	1968	31.3	2.1	18.9	47.7
Humphrey	1968	31.1	1.7	22.5	44.7
Mean		29.7	4.3	12.5	• 53.5

Given the unknown quality of nonincumbents and the fascination of the media with all aspects of the president's personality, one might expect incumbents to be evaluated more on the basis of personal attributes. However, both challengers of incumbents and candidates in nonincumbent races are more likely to evoke a strictly candidate attribute response. One interpretation for the emphasis on candidate attributes arises from previous work on schematic assessments of political leaders (Conover, 1981; Miller, Wattenberg, & Malanchuk, 1982). These studies demonstrate that people will make inferences about the potential performance of candidates from the candidates' personal attributes when there is little available information on the policy positions or past experience of those running for office. Thus the public places more emphasis on what is readily observable. For relatively less well-known candidates it can be expected that personal attributes form the primary focus of attention. In addition, the performance-relevant personal characteristics can be expected to be more apparent for incumbents. For example, in 1980 many respondents remarked about Carter's weakness and lack of decisiveness in a clearly performance-laden fashion. In other words, it may not be that personal attributes are less important in judging incumbents but rather that they are more politicized.

Turning to the individual candidates, it is clear that although Nie, Verba, and Petrocik (1976) failed to separate policy from performance, they were nevertheless correct in arguing that policy alternatives have become more prevalent in candidate evaluations since 1964. In all the possible comparisons candidates from the 1952-1960 period rank lowest in percentage of policy-related comments. Also confirmed is the conventional wisdom concerning the exceptionally high salience of policy questions in the elections of 1964 and 1972. In particular, 1972 stands out as the most policy-oriented election, because a greater percentage commented on Nixon's policies than about any other incumbent, and similarly McGovern was evaluated more on policy matters than any other challenger.

Somewhat less expected, however, is the increase over time in the percentage of performance responses for both incumbents and challengers, which reached its height in the 1976 contest. One explanation is that with the decline of political parties performance assessments are now much more clearly candidate-centered than in the past (see Wattenberg, 1984). Another possible factor is the tremendous growth in the power of the federal government during this period. As presidential performance has assumed greater significance in people's everyday lives, it seems logical that voters

would be more likely to evaluate candidates on this basis.

Thus far we have yet to consider the time dimension of the open-ended responses for each particular candidate. From Table 3 it can be seen how the policy/performance dimension combines with the retrospective/prospective dimension for each. Because virtually all the candidate attribute responses were coded as retrospective, we have not incorporated them into Table 3 and will therefore forego any use of the time dimension with respect to attributes throughout the remainder of the analysis. Also, for the purpose of greater clarity, we have eliminated any response with a "mixed" code on either dimension in Table 3.

The prevalence of the joint categories displayed in Table 3 holds considerable theoretical interest. The differentiation between the issue voting models of Key and Downs, for example, depends largely on whether evaluations of the incumbent are mainly retrospective or projected into the future. It is clear that the data are more supportive of Key's model, because very few performance comments about incumbents were prospective in nature. Indeed, even assessments that focused on the incumbent's policies were largely retrospective. All told, 87% of the policy and performance responses regarding incumbents were coded as retrospective. Only in the cases of Johnson and Ford were more than 10% of the issue comments prospective. Having not been elected to the presidency and having served only about half a term, these two candidates had less of a past record and therefore were evaluated somewhat more prospectively.

For challengers of course the picture is vastly different. Performance judgments were split fairly evenly between retrospective and prospective comments, whereas policy responses were predominantly prospective. Thus citizens take into account the challenger's past record of performance (e.g., Carter or Reagan's service as a governor) as well as his or her promised future performance. Policies, however, present a clear case where the incumbent's past program is compared to the proposed program of the challenger.

Finally, in nonincumbent races prospective comments are slightly more prevalent than retrospective ones for both policy and performance. This differs, however, according to whether or not the candidate represents the same party as the outgoing president. Of the comments regarding candidates from the incumbent party, 52% were retrospective compared to only 35% for their opponents. Thus the former are apparently held at least somewhat responsible for the recent performance and policies of their party.

In sum, the data presented in this section sug-

Table 3. Combined Retrospective/Prospective, Performance/Policy Assessments of Candidates (%)

		Retrospective Performance	Prospective Performance	Retrospective Policy	Prospective Policy
Incumbents					
Eisenhower	1956	70.2	5.9	20.6	3.3
Johnson	1964	45.0	10.8	30.9	13.2
Nixon	1972	55.0	1.4	38.5	5.0
Ford	1976	67.1	11.2	15.7	6.1
Carter	1980	70.3	3.6	21.8	4.3
Mean		61.5	6.6	25.5	6.4
Challengers					
Stevenson	1956	29.5	29.9	12.1	28.5
Goldwater	1964	9.6	18.3	22.9	49.1
McGovern	1972	11.5	8.4	19.5	60.6
Carter	1976	27.0	32.8	12.0	28.2
Reagan	1980	15.7	18.6	11.3	54.4
Mean		18.7	21.6	15.6	44.2
Nonincumbent Races					
Eisenhower	1952	33.2	52.2	4.6	10.0
Stevenson	1952	42.1	35.6	9.1	13.2
Kennedy	1960	16.4	50.2	6.3	27.1
Nixon	1960	43.8	34.9	7.2	14.0
Nixon	1968	28.0	34.4	17.2	20.3
Humphrey	1968	29.3	29.0	25.1	16.7
Mean		32.1	39.4	11.6	16.9

gest that the issue voting models of V.O. Key and Anthony Downs were both partly correct and partly incorrect in describing how voters evaluate presidential candidates. Only incumbents appear to be judged as suggested by Key—mindful of the past but attentive primarily to outcomes rather than to policy instruments. The Downsian model, on the other hand, seems to fit best as a description of how citizens judge challengers, as they are assessed primarily in terms of the programs they promise to initiate if elected. The data appear to confirm a common sense notion of rational cognitive processes. When information on past performance is available voters readily form judgments based on this criteria. In this case there is less necessity for them to make inferences about the future. The absence of such concrete information for evaluating nonincumbents, however, allows for greater opportunity for policy innovation by the candidates and greater public concern over the direction of policy. In short, the public appears to act in a highly responsible fashion, basing their evaluations on the most reliable and concrete information available.

Issue Evaluations and the Vote

Having examined the relative preponderance of the substantive categories used by the public to

evaluate candidates, we next turn to an assessment of how these evaluations have been translated into the vote. The first step in this investigation is to determine if particular candidates were advantaged or disadvantaged by judgments focused on either policy or performance, past or future. Clearly the categorization of assessments by behavioral focus (policy/performance) and a time perspective will be of little theoretical utility if there is no systematic variation in evaluations with respect to these categories. Table 4, therefore, presents summary figures representing the percentage of voters making positive comments minus the percentage making negative comments for each combination of the policy/performance and retrospective/prospective dimensions, as well as for the attribute dimension.

Clearly the most volatile of the issue categories is retrospective performance. This is largely the result of responses concerning incumbents running for reelection and is reflective of the great public approval of the performance in office of Eisenhower, Johnson, and Nixon, and the unprecedented disapproval of Carter's past performance. In contrast, prospective performance is more likely to yield a substantial partisan advantage in nonincumbent races. Indeed, this is the case in 1952 and 1968, when Eisenhower and Nixon respectively enjoyed an edge on this category.

However, the largest advantage on prospective performance went to Johnson over Goldwater in 1964, with Goldwater receiving the lowest score on this for any candidate in the time series and Johnson receiving the third highest.

The partisan advantage associated with retrospective policy evaluations is the most stable from 1952 to 1980, with the edge usually going to the Democratic candidate. This can be explained by the fact that Democratic candidates are more often positively associated with helping such groups as the working man or any of the variety of groups that made up the New Deal coalition, while Republican candidates are negatively associated with the interests of the rich and big business. Although such responses are sometimes vague, they generally connote helping one group rather than another, that is, a choice of policy directions. The greatest partisan advantage on retrospective policy, however, came as the result of policy regarding one particular individual. This was Ford's pardon of Nixon, which was the focus of many negative retrospective policy comments in 1976.

Only the ideological elections of 1964 and 1972 show much evidence of a substantial partisan advantage on prospective policy. Given a clear choice between future policies in these years, the public responded favorably to the policies they perceived Johnson and Nixon would pursue and

negatively toward those of Goldwater and McGovern. Although prospective policy comments were somewhat more prevalent in 1972 (see Table 2), the advantage that Johnson enjoyed over his opponent was far greater than that of Nixon, thereby indicating a clearer policy mandate in 1964.

Finally, the attribute code displays a clear example of the incumbency advantage in presidential elections. In all five cases the incumbent was the recipient of more positive comments directed toward his personal attributes than the challenger. This is in large part because comments such as "he's more experienced" fall into this category, which is more likely to benefit the incumbent. Even Carter far outdistanced Reagan on this score in 1980, because many respondents complimented him for his experience and integrity whereas Reagan's advanced age and past movie career evoked numerous negative comments. This advantage may well also extend to the candidate of the incumbent party during nonincumbent races, as in each of these three contests the president's chosen successor had the edge.

Yet to be established, however, is whether or not these categories are related to the vote, and if so, to what degree. Taking just the four issue categories in which we are primarily interested and regressing them on the vote, it is clear that candidate policy and performance evaluations

Table 4. Positivity of Responses by Dimension

		Retrospective Performance	Prospective Performance	Retrospective Policy	Prospective Policy	Attribute
Incumbents						
Eisenhower	1956	15.0	1.2	-3.0	0.2	19.5
Johnson	1964	10.6	4.3	1.2	4.4	1.8
Nixon	1972	12.7	0.5	0.6	1.4	3.4
Ford	1976	-3.1	0.9	-5.0	-0.2	10.7
Carter	1980	-25.0	1.4	-2.6	0.9	10.8
Challengers						
Stevenson	1956	-2.0	0.6	0.4	-0.8	-7.6
Goldwater	1964	-3.0	-2.6	-2.2	-8.9	8.6
McGovern	1972	-3.4	-0.5	-1.0	-3.6	-18.6
Carter	1976	-10.8	2.0	1.3	2.1	8.5
Reagan	1980	6.2	1.6	-1.4	3.0	-20.6
Nonincumbent Races						
Eisenhower	1952	5.1	6.0	-1.1	0.1	7.2
Stevenson	1952	0.0	0.4	0.8	0.1	7.4
Kennedy	1960	-1.3	4.2	0.5	0.6	0.8
Nixon	1960	2.3	2.9	-1.4	-1.5	15.4
Nixon	1968	-3.4	6.4	0.0	3.2	-1.7
Humphrey	1968	-1.9	0.3	-0.9	1.8	0.7

Note. Cell entries are calculated by subtracting the percentage of voters offering negative comments on each dimension from the percentage offering positive comments. Thus a positive entry reflects a preponderance of favorable comments.

Table 5. Regression Equations Predicting the Vote: Standardized Coefficients

	1952	1956	1960	1964	1968	1972	1976	1980
Retrospective performance	.11	.17	.14	.20	.18	.24	.27	.22
Prospective performance	.15	.06	.20	.10	.23	.11	.19	.12
Retrospective policy	.04	.13	.04	.26	.10	.21	.13	.17
Prospective policy	.10	.08	.10	.24	.15	.24	.17	.18
Attribute	.21	.25	.31	.30	.23	.33	.27	.22
Party identification	.54	.53	.48	.41	.48	.27	.42	.47
R	.70	.73	.76	.74	.75	.68	.73	.73
N	1175	1249	879	1106	871	822	1253	873

have grown substantially as vote predictors in recent years. In the two Eisenhower-Stevenson races these variables explained only about 10% of the variance in the vote; by 1980 the comparable figure had nearly tripled to 30%. (The multiple *R*s are as follows: 1952, .33; 1956, .32; 1960, .40; 1964, .49; 1968, .46; 1972, .47; 1976, .49; 1980, .53.) Given the open-ended nature of the data, this is a substantial percentage of explained variance. Thus, although Campbell, Converse, Miller, and Stokes (1960) may have been justified in paying scant attention to these factors in *The American Voter*, they have played a more substantial role in recent elections.

A more complete model of the voting decision is displayed in Tables 5 and 6, which incorporate the four issue categories, candidate attribute comments, and party identification for each election study.² We wish to answer two general questions with these regressions. First, what was the relative impact of policy versus performance and retrospective versus prospective evaluations on the vote? Or in other words, how much of the collective voting decision was the result of variations in assessments of such variables as the candidate's retrospective performance and retrospective policies? Second, to what extent did each variable influence the actual election outcome, given its relationship to the vote and advantage for one or the other candidate? Following Achen's (1982) guidelines for interpreting and using regression, we chose to use a measure of dispersion importance to address the first question and a measure of level importance to examine the second. The measure of dispersion importance is commonly known as a "standardized beta," which is the regression coefficient when all variables have been

standardized to a mean of zero and a variance of one. For measuring level importance we simply multiplied the mean of each independent variable by its unstandardized coefficient, thereby producing the net contribution to the level of the dependent variable, that is, the vote. Thus if a variable has a level importance of 1.0 in Table 6, it means that the distribution on it resulted in a change of 1% of the vote in favor of the Republican candidate.

One general theme flowing through this article thus far is the difference in public evaluations of candidates according to incumbency status. Following this, one would expect to find the structure of the vote decision, as measured by the standardized betas, to differ according to whether or not the incumbent was a candidate for reelection. Indeed, the three nonincumbent races of 1952, 1960, and 1968 stand out as the only elections in which prospective performance outweighs the other three issue variables in terms of dispersion importance (see Table 5). Yet as can be seen from the level importance coefficients in Table 6, prospective performance has not been a major factor in shifting the outcome of elections one way or the other. The largest net gain from prospective performance evaluations was only slightly more than 1% by Richard Nixon in 1968.

For races with incumbents, retrospective performance clearly plays a much greater role. In the two most recent elections for which we have data—1976 and 1980—its importance in explaining variance in the vote exceeds not only all the other issue variables but the candidate attribute variable as well. Furthermore, the effect on shifts in the two-party vote has been substantial, averaging 2.9% in the five elections with incumbent presidents seeking reelection. Most notably, the difference between assessments of Carter's and Reagan's past performance in 1980 can be estimated to have cost Carter 5.2% of the vote.

In only two elections, 1964 and 1972, can we conclude that policy considerations exerted more influence over the collective voting decision than performance assessments. The large standardized

²Because of the problems associated with using OLS regression with a dichotomous dependent variable, we also conducted a probit analysis for each year. The results were virtually identical from a substantive perspective, and therefore we present the regression results for their greater ease of interpretability.

Table 6. Regression Equations Predicting the Vote:
Importance Level Coefficients for Short-Term Variables (Positive values are pro-Republican)

	1952	1956	1960	1964	1968	1972	1976	1980
Retrospective performance	.58	2.51	.51	-2.22	-.24	3.09	1.37	5.19
Prospective performance	.76	.08	-.24	-.80	1.16	.25	-.19	.02
Retrospective policy	-.18	-.56	-.16	-.68	.09	.28	-.91	.19
Prospective policy	.00	.15	-.26	-2.51	.22	.96	-.44	.34
Attribute	.00	2.69	3.54	-1.78	-.47	4.91	.35	-4.23

Note. The entries are computed by multiplying the mean of each independent variable by the unstandardized regression coefficient. The regression equation is identical in form to that used for Table 5.

betas for both retrospective and prospective policy in 1964 combined with the substantial advantage Johnson enjoyed over Goldwater in positive versus negative policy comments (see Table 4) lends strong support for interpreting the 1964 election as a policy mandate for Lyndon Johnson. To a lesser degree the 1972 election also fits the requirements for a policy mandate. As noted earlier, the frequency of policy responses was even higher in the Nixon-McGovern contest. However, the beta weight for retrospective policy is somewhat lower in 1972, and comments about Nixon's policy stands were not as positive as those for Johnson, nor were McGovern's policies seen as negatively as Goldwater's. Thus although the two policy categories combined contributed 3.2% to Johnson's victory, they only gave Nixon about 1.3%. Finally, the 1980 election outcome, for which a policy mandate has also sometimes been claimed (cf., Miller & Shanks, 1982) shows even less evidence of being determined by policy factors. The comparative lack of policy responses and the absence of any sizeable candidate advantage resulting from these comments, plus the much lower policy coefficients shown in Table 5 all work to dissuade us from interpreting the 1980 result as a policy mandate. While evaluations of Reagan were as policy laden as those for Goldwater, evaluations of Carter were far more performance oriented than those of either Johnson or Nixon (see Table 2). Furthermore, these comments about Carter's performance provided Reagan with virtually all of his advantage on issues. As can be seen in Table 5, Reagan gained only .5% because of policy evaluations in 1980 compared to more than 5% from performance evaluations.

Conclusion

The work reported above begins to provide some empirical support for a theory of issue voting. Despite data limitations, respondents could be classified by applying a two-dimensional retrospective/prospective, policy/performance

coding scheme to their verbal assessments of the candidates. In addition, a factor analysis of available closed-ended items from the 1980 election study showed a similar structure of voter cognitions. These two very different approaches both show that assessments of Carter primarily reflected concern about his past performance, whereas evaluations of Reagan focused on future policy actions as well as expected performance. This striking similarity helps assure us that our results are valid.

The data demonstrate that people are not only capable of judging the past performance of incumbents, but they frequently become concerned about the future application of policy instruments. Historically, neither retrospective nor prospective policy considerations have been the dominant focus of thinking about politics. Nevertheless, when candidates articulate discernible policy positions, the public can incorporate this information into their electoral decision. When this information is available, citizens can vote in a manner that indicates a policy mandate for future government actions. Such cases have been historically rare, however.

The data suggest that under varying circumstances the voting theories of Key, Downs, and Fiorina each have substantial merit. Key's emphasis on retrospective performance applies best to public assessments of incumbents running for reelection. The Downsian model, in contrast, seems best to fit challengers, whom are often evaluated on the basis of their proposed future policies. Finally, Fiorina's concentration on prospective performance provides the best description of public judgments of candidates running in nonincumbent races.

Our ability in this article to test such a variety of theories throughout the entire 1952-1980 period demonstrates the great utility of open-ended questions in electoral research. It is extremely difficult to test models or theories if measures of critical concepts are developed ad hoc and with little guidance from the theory itself, as has all too often been the case in the election studies. Over

the years, however, diverse use has been made of the open-ended questions, which can be reanalyzed with different theories in mind. The richness of the open-ended questions suggests that more of these questions should be asked in future studies, and a means of making the verbatim responses more accessible should be sought.

Coding Appendix

Coding of the distinction between retrospective/prospective and policy/performance utilized the responses to the eight candidate and party likes/dislikes questions which have been asked in every presidential election survey conducted by the University of Michigan since 1952.

The coding of the open-ended responses was done from the original protocols on file at the Center for Political Studies at the University of Michigan. For the surveys conducted before 1968 those materials are on microfilm; whereas the actual interview schedules are still available for the more recent years. We wish to thank CPS for making these materials available to us.

Some 30 different individuals were engaged in the coding project, which took one and a half years to complete. The average number of interviews coded per person was approximately 435. Each coder was trained with a set of 50 selected interviews before they began their production coding.

The training procedure was as follows. Before coding we met with the coders to discuss the project and to specify the concepts of policy, performance, retrospective, and prospective. The coders were then asked to read Fiorina (1981, pp. 1-16) and Abramson et al. (1982, chaps. 6 and 7) as a further overview of the theoretical dimensions we wished to differentiate. Next the coders were assigned to code 50 interviews which had been systematically selected for the purposes of coder training. Each of these examples was then discussed in depth, and any discrepancies were clarified and resolved. After training each coder was assigned a designated set of interviews to complete. Coders were instructed to discuss ambiguous statements with the coding supervisor (a coder trained on the first study coded) and to bring only unresolvable coding problems to the principal investigators. This procedure was followed in order to avoid the unwitting introduction of a coding bias by the investigators.

The studies were also coded in a particular order in an effort to avoid the artifactual introduction of a trend. First the 1980 study was coded, followed by 1956, 1964, and 1972. Next the 1976, 1960, 1952, and 1968 studies were coded in that order. Given this sequence for coding the studies, as well as the large number of coders, it is

highly unlikely that any of the results arose because of systematic coder bias.

The coding scheme was designed to allow for gradations of clarity in the respondents' statements, thereby reducing the difficulty of the coding task. The policy-performance dimension, for example, ranged from "clearly policy" to "clearly performance" with a "mixed" or combination policy and performance category in the middle of the scale. In addition the code categories allowed for candidate attributes and any performance comments that had no issue content (e.g., "he can't get things done," "he is doing a good job"). The values assigned in the coding and the code categories are summarized below.

Retrospective/Prospective

1. Pure retrospective
2. Emphasis on retrospective
3. Mixed
4. Emphasis on prospective
5. Pure prospective
9. Inapplicable

Performance/Policy

1. Performance with respect to issues
2. Emphasis on performance
3. Mixed
4. Emphasis on policy position
5. Pure policy position
6. Candidate attribute
7. Emphasis on candidate attribute
8. Performance with no issue focus
9. Inapplicable.

A sample of 625 of the interviews coded were coded again by a second person to obtain reliability coefficients. Intercoder reliability was calculated with the formula: $1.0 - (\text{total disagreements} / \text{total entries})$, where an entry was a codable response, that is, other than missing data, for each variable. A disagreement occurred whenever the two coders had a discrepancy that indicated a difference in the direction of the result; that is, where one coded retrospective and the other prospective or mixed, or one coded policy and the other coded performance or mixed or candidate attribute. It should be noted that many of the coding errors were found to be related to the difficulty of reading the questionnaire responses from microfilm. When the actual questionnaires were available, as they were for the 1968-1980 studies, the reliability coefficients were well over .90.

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Ideology, Party, and Voting in the U.S. Congress, 1959-1980

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Current methods of roll-call analysis have practical as well as theoretical shortcomings. We propose here a method based on a spatial theory of voting that overcomes these problems. We apply metric multidimensional unfolding to interest-group ratings of members of Congress in order to obtain a Euclidean spatial configuration of congressmen. Each roll-call vote is then mapped into the configuration of members in a way consistent with spatial theory. Based on 190,000 ratings issued from 1959 to 1980, our empirical analysis demonstrates that a single liberal-conservative dimension accounts for more than 80% of the variance in the ratings. A second dimension, associated with party unity, accounts for 7% of the variance. Approximately 86% of all roll-call voting for the 22 years of our study is consistent with a simple one-dimensional spatial model. The votes that best fit the liberal-conservative dimension are drawn from the government management, social welfare, and foreign policy areas. The votes that best fit the two-dimensional configurations are drawn from the agricultural area.

The United States Congress is the most widely studied legislative institution in the world. The literature written on the structure, procedures, social norms, policymaking processes, voting positions, and other elements of the congressional environment would fill a large library, with the most shelf space undoubtedly taken up by studies of congressional voting. Since Orrin Libby (1897) first issued a call for the study of congressional voting, political scientists, social psychologists, historians, sociologists, and even economists have responded with an ever-increasing flow of research.

Creating a geometrical representation of the legislators or the roll calls is an approach that has been used for some time to study congressional voting (e.g., Rice, 1924). The legislators and roll calls are represented as points in a space where the distances between the points are related to measures of association between the legislators and roll calls. Typically, these measures of association (e.g., Yule's Q , ϕ , ϕ/ϕ_{\max}) are analyzed by factor analysis, cluster analysis or multidimensional scaling in order to produce the spatial representation of legislators or roll calls. The results of a properly designed factor analysis may also be used to select votes for a Guttman scaling. Representative works are MacRae (1958), Weisberg (1968), Clausen (1973), and Hoadley (1980).

This approach is fraught with practical and theoretical difficulties. Practically, the use of such techniques is very cumbersome. The researcher

must select the roll calls and legislators, decide how to handle missing data and item directions, choose the appropriate measure of association, and choose a response model (either dominance or proximity) (Weisberg, 1968, p. 233). Performing a factor analysis or a multidimensional scaling of a 100 by 100 or a 435 by 435 matrix of associations is a formidable and expensive undertaking. Working with subsets of legislators or subsets of votes (a vote-by-vote matrix of associations) and then overlapping the subsets is possible but is a clumsy and time-consuming procedure.

In addition to these serious practical limitations, this general methodology suffers from an important theoretical limitation. Suppose that an ideological space exists and that legislators and the yea-and-nay alternatives on each parliamentary notion can be represented as points in this space. Furthermore, assume that the legislators have single-peaked utility functions over the space and vote for the alternative closest to them. Morrison (1972) has shown that, given this model, these scaling techniques are unlikely to recover the true positions of the legislators in a legislator-by-legislator analysis. The input to these techniques are measures of association based on the proportion of disagreement between pairs of legislators. What Morrison showed was that the proportion of disagreement depends upon the distance between the pair of legislators, the angle they form with the (arbitrary) origin of the space, and the distribution of the policy outcomes associated

with the roll-call votes. Because the scaling methods described above treat the associations only as measures of distance (multidimensional scaling) or only as measures of angles (factor analysis), distortion in the recovery of the space is unavoidable.

We propose here a new method of roll-call analysis which overcomes these difficulties and enables a researcher to engage in cross-chamber comparisons of both legislators and roll calls within a year and between years. Our method is an extension of that developed by Poole (1981, 1984). In a nutshell, we use multidimensional unfolding on interest-group ratings of legislators (in accordance with a spatial model developed in the next section) to obtain a spatial configuration of the legislators. We then map the roll-call votes into the legislator configuration using a simple spatial model of voting.

A Spatial Model of Interest-Group Ratings

Interest groups are close observers of congressional voting, and each year many of them publish ratings of the members of Congress. To rate a member of Congress, an interest group normally chooses between 10 and 40 votes for each house of Congress from the total set of roll calls taken during the particular session under study. These votes are chosen for their relevance to the group's interests, and the rating is determined by calculating the ratio of "correct" to total (correct plus incorrect) votes. (Some groups treat absences as "incorrect.") Each rating thus represents the legislator's percentage agreement with the stated positions of the group.¹

¹We were able to obtain ratings for the following groups for the years indicated: American Civil Liberties Union (ACLU), 1979-1980; American Conservative Union (ACU), 1971, 1972, 1974-1980; Americans for Constitutional Action (ACA), 1959-1980; Americans for Democratic Action (ADA), 1960-1961, 1963-1980; American Farm Bureau Federation (AFBF), 1978-1980; American Federation of Government Employees (AFGE), 1973-1977; American Federation of State, County and Municipal Employees (AFSCME), 1973, 1975-1980; American Federation of Teachers (AFT), 1975-1980; American Security Council (ASC), 1969-1980; Bread for the World (BFW), 1979-1980; Building and Construction Trades Department AFL-CIO (BCTD), 1979-1980; Chamber of Commerce of the United States (CCUS), 1975-1980; Child Welfare League of America (CWLA), 1976-1977, 1979-1980; Christian Voice (CV), 1979; Coalition for a New Foreign and Military Policy (CFNFMF), 1977-1980; Committee for the Survival of a Free Congress (CFSC), ratings on all issues, economic issues, defense and social issues, 1979-1980; Committee on Political Education of

There are two basic types of rating interest groups—general and specific. The general interest groups (e.g., the Americans for Democratic Action) construct their ratings from a broad range of issues, whereas the specific groups focus on a narrow range of issues (e.g., the Child Welfare League of America concentrates on issues related to the health and welfare of children). The positions that a general group takes on a wide range of issues are very likely to be systematically related (what Converse (1964) called "constrained"). A group that opposes CETA, OSHA, and busing is likely to favor work requirements for welfare recipients and a U.S. military buildup. Consequently, an interest group's positions on a range of issues are largely determined by the group's positions on a small number of underlying evaluative dimensions. The issue-specific groups can also be viewed within the same framework. Single-issue groups are usually the most committed of the interest groups; they tend to attract more ideologically motivated members. Although their focus is normally on a single issue, their beliefs frequently carry into other issue areas as well. Then, by this argument, the overall rating by both a general or a specific group is a measure of how close in spatial terms the member of Con-

the AFL-CIO (COPE), 1959-1980; Common Cause (CCS), 1978-1980; Congress Watch by Nader's Public Citizen (CW), 1975-1980; Conservative Coalition Support Scores (CC), 1959-1980; Consumer Federation of America (CFA), 1971-1980; Friends' Committee on National Legislation (FCNL), 1977-1980; League of Conservation Voters (LCV), 1971-1977, 1979-1980; League of Women Voters (LWV), 1971-1975, 1977-1980; Lower Federal Spending Support (LFS), 1959; Larger Federal Role Support Score (LFR), 1959-1968; Liberty Lobby (LL), 1961-1969, 1973, 1975, 1977; National Alliance of Senior Citizens (NASC), 1977, 1979-1980; National Council of Senior Citizens (NCSC), 1977-1980; National Education Association (NEA), 1969-1979; National Farmers' Organization (NFO), 1973, 1975-1980; National Farmers' Union (NFU), 1961-1965, 1969-1980; National Federation of Independent Business (NFIB), 1977-1980; National Women's Political Caucus (NWPC), 1979; New Republic (NREP), 1961-1974; National Taxpayers' Union (NTU), 1971, 1973-1980; Presidential Support Scores—Eisenhower, Kennedy, Johnson, Nixon, Ford, Carter, 1959-1980; Ripon Society (RIPON), 1969-1978; Citizens for a Sane World (SANE), 1973-1978; Taxation with Representation (TWR), 1977-1978; United Auto Workers (UAW), 1969-1980; United Mine Workers (UMW), 1979-1980; Whenever possible we tried correcting the ratings to remove absences. Some groups, mainly liberal ones such as the ADA, count absences as negative votes. We found that correcting for the absences made almost no difference in the unfolding results.

gress is to the interest group on the evaluative dimensions.

Along the lines of the spatial model of party competition, we assume that each issue can be represented as a dimension, and each interest group and member of Congress has a most preferred position on each issue which is the ideal point of the group or member on that issue dimension. Let m be the number of issues and let z_{jk} denote the j th ($j = 1, \dots, q$ where q is the number of interest groups) interest group's position on the k th issue. Then the m ideal points can be denoted as the vector:

$$z_j = \begin{bmatrix} z_{j1} \\ \vdots \\ z_{jm} \end{bmatrix}$$

Similarly, the vector

$$x_i = \begin{bmatrix} x_{i1} \\ \vdots \\ x_{im} \end{bmatrix}$$

represents the i th ($i = 1, \dots, p$ where p is the number of members of Congress) member's ideal points on the m issues. The Euclidean distance between the j th interest group and the i th member is

$$d(x_i, z_j) = d_{ij} = \left[\sum_{k=1}^m (x_{ik} - z_{jk})^2 \right]^{1/2}$$

If no error is present and there exists s common evaluative dimensions, $s < m$, then there are vectors \tilde{z}_j and \tilde{x}_i of length s such that

$$\begin{aligned} d_{ij} &= \left[\sum_{k=1}^m (x_{ik} - z_{jk})^2 \right]^{1/2} \\ &= \left[\sum_{k=1}^s (\tilde{x}_{ik} - \tilde{z}_{jk})^2 \right]^{1/2}, \end{aligned}$$

that is, each of the m -issue dimensions is a linear combination of the s evaluative dimensions. Or, put another way, all the ideal points lie on an s -dimensional hyperplane (the evaluative, or what Ordeshook (1976) refers to as the "basic" space) through the m -space of issues.

We assume that each roll-call vote has two outcomes, one corresponding to yea and one corresponding to nay; that is, let

$$\psi_{y^l} = \begin{bmatrix} \psi_{y^l 1} \\ \vdots \\ \psi_{y^l m} \end{bmatrix} \quad \psi_{n^l} = \begin{bmatrix} \psi_{n^l 1} \\ \vdots \\ \psi_{n^l m} \end{bmatrix}$$

be the vectors of the outcome positions, respectively, where $l = 1, \dots, t$ indexes the roll calls and t is the number of roll calls. Normally, most of the entries in ψ_{y^l} and ψ_{n^l} are zeroes because a roll-call vote usually touches only on a small number of issues. If each of the m -issue dimensions is a linear combination of the s evaluative dimensions, then there are vectors $\tilde{\psi}_{y^l}$ and $\tilde{\psi}_{n^l}$ which are the projections of ψ_{y^l} and ψ_{n^l} , respectively, onto the basic space.

By this model, the interest groups select roll-call votes with outcome locations near their ideal points in the basic space to construct their ratings. We assume that the legislators have symmetric single-peaked utility functions and will vote for the outcome nearest them in the space. Consequently, setting aside perceptual error, the number of correct votes is monotonic with the distance between the interest group and the member of Congress. To see this, inspect Figure 1. The top part of Figure 1 displays the utility function for one hypothetical interest group near the end of a single evaluative dimension. The horizontal dotted line represents the utility threshold for the interest group. Consider a given set of votes with outcomes that can be represented as positions in this one-dimensional basic space. Then, for any roll call, the interest group will include the vote in its ratings only if there is an outcome in the interval $[O_1, O_2]$. Suppose that the number of roll calls is substantial and that there are a large number of outcomes in $[O_1, O_2]$, with the opposite or "anti" choices for $[O_1, O_2]$ falling in $[O_3, O_6]$. (The distance between pairs of outcomes need not be the same across roll calls.) Assuming no perceptual error, legislators located in the interval $[O_1, O_3]$ would all receive rating scores of 100. (O_3 is the midpoint of O_1 and O_5 , and O_4 is the midpoint of O_2 and O_6 .) Legislators to the right of O_3 , however, will receive scores of less than 100 because they will be closer to some outcomes in $[O_3, O_6]$. The closer a legislator comes to O_4 , the lower his or her rating. Legislators to the right of O_4 all receive scores of 0 because, for each roll call used by the interest group, they are closer to the anti outcome. The lower part of Figure 1 shows the rating issued by the interest group as a function of the position of a legislator in $[O_1, O_6]$.

Figure 1 illustrates one of the weaknesses of the interest group ratings, namely, that the ratings are confined to the interval $[0, 100]$ when in fact they should be able to assume any value in the interval $(-\infty, 100]$. However, this becomes a less serious

problem as the number of interest groups increases. When there are multiple groups, the zones of indifference like $[O_1, O_3]$ and $[O_4, O_6]$ get whittled down and the legislators can be uniquely located.²

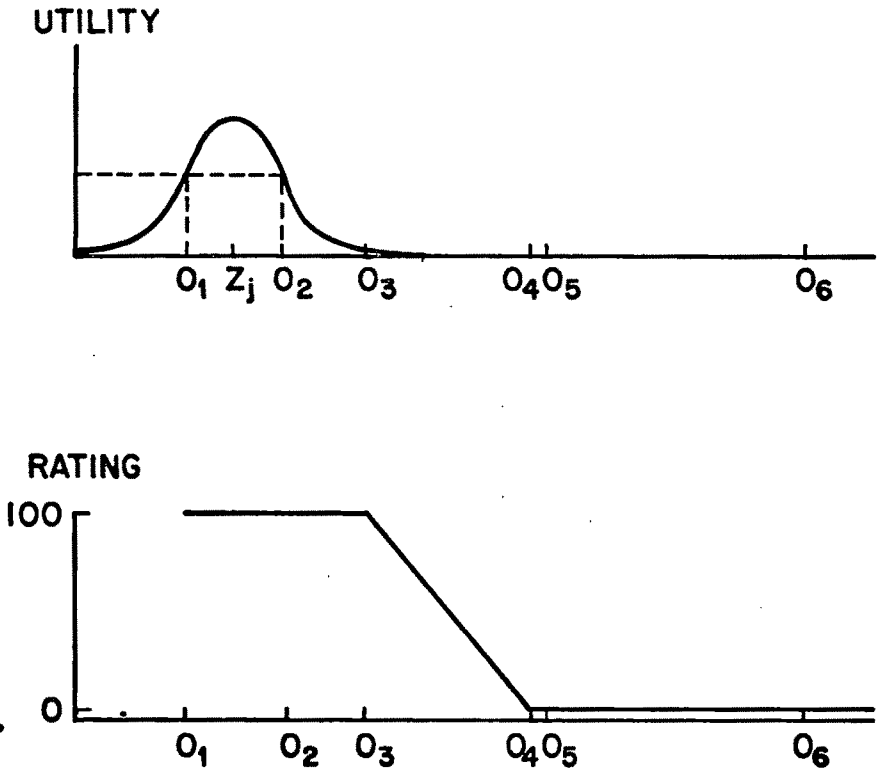
Because the interest groups select roll-call votes with outcome locations near their ideal points in the basic space to construct their ratings, it is appropriate to treat a score of 85 given by an interest group to a senator in the same way as a score of 85 given to a representative. The comparability of the ratings does not rest on the inter-

est groups selecting roll calls from the same issue area(s) in both houses. All that matters is that the groups select votes with outcomes near them in the basic space. A comparability problem could arise if a group chooses roll calls in one house consistent with the spatial model but utilizes non-spatial criteria in the other house. For example, a group could choose votes on food stamps and arms control in the Senate to construct its ratings and on agricultural votes in the House of Representatives. (We show below that votes on food stamps and arms control are highly consistent with the spatial model, whereas agricultural votes generally are not.) In any case, the House and Senate ratings can be analyzed separately as well as together to guard against this problem. Empirically, we find its occurrence to be rare (see below).³

²Another weakness of the ratings closely related to this one is that it is unclear how many roll calls there has to be for the number to be "substantial"—that is, enough votes in the area of interest of a group with outcomes close enough to be used to construct its ratings. In general, the more votes that are used, the more "accurate" the ratings, that is, the narrower the zones of indifference. This is clearly more of a problem in the earlier years of our analysis than in the later years. However, we believe that the argument we just made applies to this weakness as well; namely, when there are multiple groups, the zones of indifference get whittled down and the legislators can be uniquely located.

³A more subtle objection to our model stems from a lack of agenda control in the Congress by the interest groups. Because of this, the number of roll calls that an interest group regards as important may not be very large. As a consequence, the group may have to use some roll calls with outcomes not as close to them as

Figure 1. Rating Example



Because the ratings are measures of closeness or preference, they can be regarded as inverse distances (the higher the rating, the closer a member is to a group) and are therefore suitable for a multidimensional unfolding analysis. In this specific instance, the aim of an unfolding analysis is to locate points representing the legislators and points representing the interest groups in a space of minimal dimensionality such that the Euclidean distances between the two sets of points reproduce the ratings as closely as possible. Formally, let δ_{ij} denote the j th interest group's rating of the i th member of Congress. We convert the ratings to distances by the linear transformation

$$d_{ij}^* = (100 - \delta_{ij})/50 = d_{ij} + e_{ij} \quad (1)$$

where the error term, e_{ij} , is picking up three effects: 1) perceptual error, 2) idiosyncratic evaluative dimensions, and 3) the substitution of zero for negative ratings. The loss function that we minimize is

$$\begin{aligned} \mu &= \sum_{i=1}^p \sum_{j=1}^q e_{ij}^2 \\ &= \sum_{i=1}^p \sum_{j=1}^q \left\{ d_{ij}^* - \left[\sum_{k=1}^s (\hat{x}_{ik} - \hat{z}_{jk})^2 \right]^{1/2} \right\}^2. \end{aligned} \quad (2)$$

they would like. If the set of available roll calls of interest in one chamber were restricted in this way, then this could systematically bias the group's ratings.

We don't think this is a serious problem. First of all, in recent years more than 500 roll calls were taken each year in both houses. Literally dozens of votes are cast on most issues of concern to the interest groups, so they have a lot to choose from. Secondly, most groups use a variety of issues to construct their ratings, and if some issue has only roll calls with outcomes distant from them there are plenty of other issues with closer outcome points to choose from. Third, even those groups that we label as specific use a variety of subjects in their issue area of interest. For example, the National Women's Political Caucus uses abortion, child care, the ERA, and other related roll calls to construct its ratings. Finally, when a group is very narrowly focused (e.g., the Child Welfare League of America), if an insufficient number of roll calls is taken or the outcome points are distant from the group, it is quite likely that the group will choose not to issue ratings. The CWLA did not issue ratings in 1978 because not enough votes were taken in Congress on child nutrition and child health care that year. They told us this in a letter when we asked for their ratings that year.

We think that these arguments also apply to the case when the Democrats control the agenda in one house and the Republicans control the agenda in the opposite house. The mix of issues being voted on did not change markedly when the Republicans took control of the Senate in 1981. What changed was which party won most of the roll calls.

Poole (1982, 1984) has developed a method of metric multidimensional unfolding which finds estimates of the member and group locations— \hat{x}_i and \hat{z}_j —that minimize μ . If the e_{ij} are assumed to be independently and normally distributed with constant variance, then the \hat{x}_i and \hat{z}_j will be maximum likelihood estimators using this method of unfolding.⁴

Because the interest groups publish ratings every year, time can be regarded as a dimension in the spatial model. With time as a dimension, we introduce the possibility that legislators and interest groups may change their spatial positions from year to year. In order to study these movements, we must place the configurations in a common metric or frame of reference.

The configurations recovered from the ratings are unique up to a selection of origin and a rigid rotation. Techniques have been developed by psychometricians for finding a common origin and rotation for a pair of configurations (it is known as the "orthogonal Procrustes" problem: Schönemann, 1966; Schönemann & Carroll, 1970); and for a set of configurations (Kristof & Wingersky, 1971; Berge, 1977). These techniques are not entirely satisfactory for our purposes here. Each year the interest groups issuing ratings and the membership of the House and Senate change. Some legislators serve for only two years, some for 10, and a few were in the Congress for the entire 22-year period covered by our study. Furthermore, some legislators served several years, were defeated and left Congress for some period, and were later elected again. Consequently, we must fit together a set of configurations in which any particular pair of configurations may have many points that are not in common.

Complicating matters further, we must allow for slight contractions or expansions of the space from year to year owing to the changing mix of interest groups that issue ratings. If the number of groups issuing ratings is small and divided into two camps located at opposite ends of the space from each other with most of the legislators falling between the two camps, then "zones of indifference," which we spoke of in connection with Figure 1, may not get whittled down to the extent

⁴Monte Carlo work with the unidimensional version of the unfolding technique has shown it to be very robust when the normal distribution, constant variance assumptions are violated. The procedure does equally well with error generated in accordance with models based on the log normal distribution, the non-central chi-square distribution, and the normal distribution with the variance as a function of the true distances. See Poole (1984). The noncentral chi-square distribution can be used to model the truncation in the ratings.

that they would be if there were more groups. This has the effect of slightly compressing the configuration of legislators.

Accordingly, the model we estimate is:

$$X_o = [\xi W' + Jp_i c']_o + E_o \quad (3)$$

where X_o is the p_i by 22- \times - s matrix. (p_i is the total number of legislators serving in the 22-year period of our study.) For one-dimensional configurations, X_o would be p_i by 22; for two-dimensional configurations, p_i by 44; and so on. ξ is the p_i -by- s matrix of average or "target" coordinates of the legislators in the basic space, W is a 22-by- s matrix of weights, c is a vector of constants of length 22 \times s , Jp_i is a p_i -length vector of ones, and E_o is a p_i -by-22-by- s matrix of error terms. The subscript o indicates that there is missing data. The estimate of ξ , $\hat{\xi}$, can be thought of as the best-fitting average set of coordinates. In effect, the 22 configurations are squeezed together as tightly as possible when they are transformed by \hat{W} and \hat{c} , and the mean configuration around which they are squeezed or targeted is $\hat{\xi}$. Details of the estimation and extensive Monte Carlo analysis can be found in Poole (1983).⁴

The W and c are used to correct the configurations to remove the effects we discussed above. This allows us to study individual change over time. (It does not affect the configurations individually because only linear transformations are applied.) However, this corrective procedure cannot pick up an across-the-board shift of all legislators and interest groups over time. For example, we find below that the primary dimension recovered from the ratings is liberalism v. conservatism as it is commonly understood by journalists and political scientists. The definition of what is liberal and conservative on specific issues can change over time. This is certainly true of foreign policy, which has become increasingly partisan, and of civil rights, which was a regional issue at one time, but ceased to be such by the late 1960s (Asher & Weisberg, 1978; Clausen & Van Horn, 1977; Bullock, 1981; Sinclair, 1981).

As a consequence, when we discuss the movement of individual legislators over time we are in effect describing their movement relative to some overall uniform (but unknown) trend. Because the movement of every legislator is relative to an across-the-board shift from year to year, comparisons of legislators over time are unaffected by this problem. In any case, it bears reiteration that this procedure fails to pick up only uniform shifts of

all legislators and interest groups. If some shift and others do not, this is picked up.⁶

Analysis of the Ratings

Table 1 displays the unfolding results for the combined House and Senate ratings for the 22 years of the study. The measure of fit displayed is the squared Pearson correlation coefficient between the actual interest group ratings and the ratings produced by the recovered configuration from the unfolding. Because the unfolding technique is a metric one, the r -squares, unlike the stress values from non-metric techniques (which only strive to reproduce a weak ordering of the data), are literally the percentage variance explained of the actual ratings.

On the average, one dimension explains approximately 81% of the variance of the 190,000 ratings issued during the 22-year period (mean $r^2 = .812$). This first dimension is a liberal-conservative left-right continuum (Poole, 1981; see also Kritzer, 1978).⁷ The addition of a second and third dimension adds little to the explanatory power of the model. The second dimension adds only 6.1% to the 22-year average, whereas the third dimension adds only 1.4%. However, these figures do hide some significant differences across selected years. Overall, the second dimension is clearly political party. However, the strength of the dimension varies from year to year. It is strongest for the years 1959 to 1960 where it explains an average of 20.4% of the variance.

The nature of the primary dimension can be better delineated by examining the distribution of members in a single year. Figure 2 shows the distribution of the members of Congress across the liberal-conservative dimension in 1979, whereas Figure 3 shows the distribution of the members across the liberal-conservative and the party dimensions.

⁶The overall uniform trend does not have to be linear or monotonic. For example, the uniform shift could accelerate over time then decelerate and reverse direction.

⁷Kritzer (1978) applied factor analysis to the interest group ratings of the 91st and 93rd House of Representatives and found that a single factor accounted for 81.4% of the variance of the 91st House and 74.6% of the 93rd House. "When one focuses on roll calls which interest groups deem to be salient rather than using the more usual shotgun approach (e.g., Clausen, 1973, or MacRae, 1958), a clear unidimensional structure emerges in House roll-call behavior, and this structure can be interpreted as reflecting a unidimensional ideological structure underlying salient congressional action" (p. 496). Below we show that this conclusion can be extended to almost all roll-call voting.

⁴Available upon request.

Table 1. Unfolding Results in Three Dimensions^a

	Dimension			Interest groups	Senators	Representatives	No. of ratings
	One	Two	Three				
1959	.7451	.8813	.8946	8	100	435	4249
1960	.6040	.8755	.8914	8	100	432	4230
1961	.9043	.9074	.9210	11	100	436	5812
1962	.8904	.9008	.9211	10	100	434	5270
1963	.9168	.9217	.9313	11	100	433	5785
1964	.9173	.9336	.9392	11	100	429	5770
1965	.9383	.9455	.9500	11	100	432	5720
1966	.8987	.9000	.9110	9	100	432	5291
1967	.9114	.9328	.9350	10	100	433	5298
1968	.8647	.8835	.8900	9	100	432	5300
1969	.7609	.8537	.8670	12	100	432	6358
1970	.8075	.8671	.8705	11	100	432	5808
1971	.7987	.8622	.8755	16	100	435	8018
1972	.7641	.8040	.8164	15	99	435	7926
1973	.7528	.8273	.8536	20	100	434	10622
1974	.6978	.8122	.8420	19	100	433	9995
1975	.7814	.8590	.8790	23	100	434	12256
1976	.7810	.8520	.8620	22	100	433	11509
1977	.7730	.8470	.8700	30	100	434	15883
1978	.7590	.8410	.8610	26	100	434	13845
1979	.8167	.8529	.8722	37	100	435	19339
1980	.7701	.8258	.8360	28	100	436	14965

^aAll entries are r^2 . The number of senators and representatives fluctuates from year to year because of seat vacancies.

Figure 2.

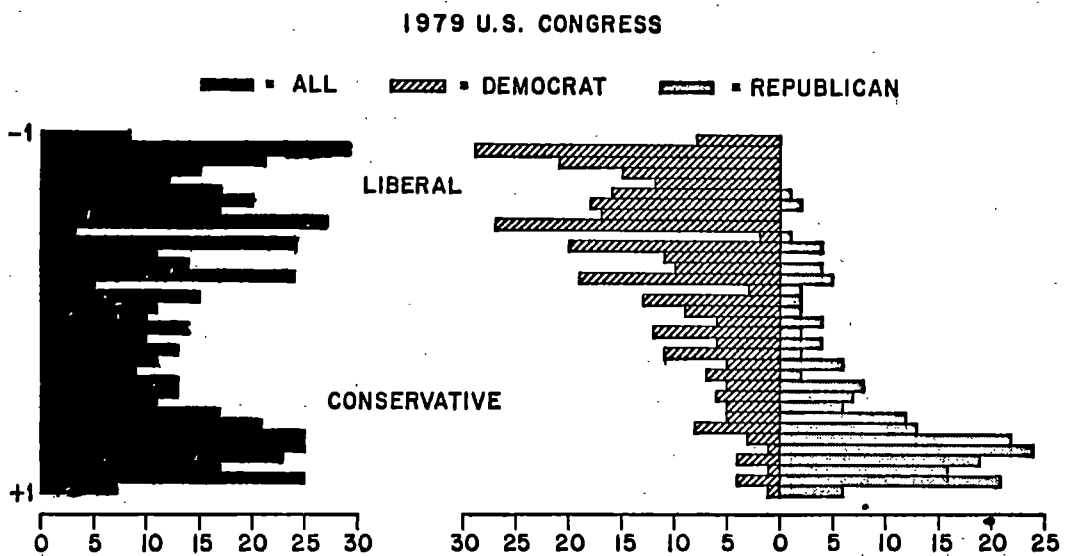


Figure 3. 1979 U.S. Congress

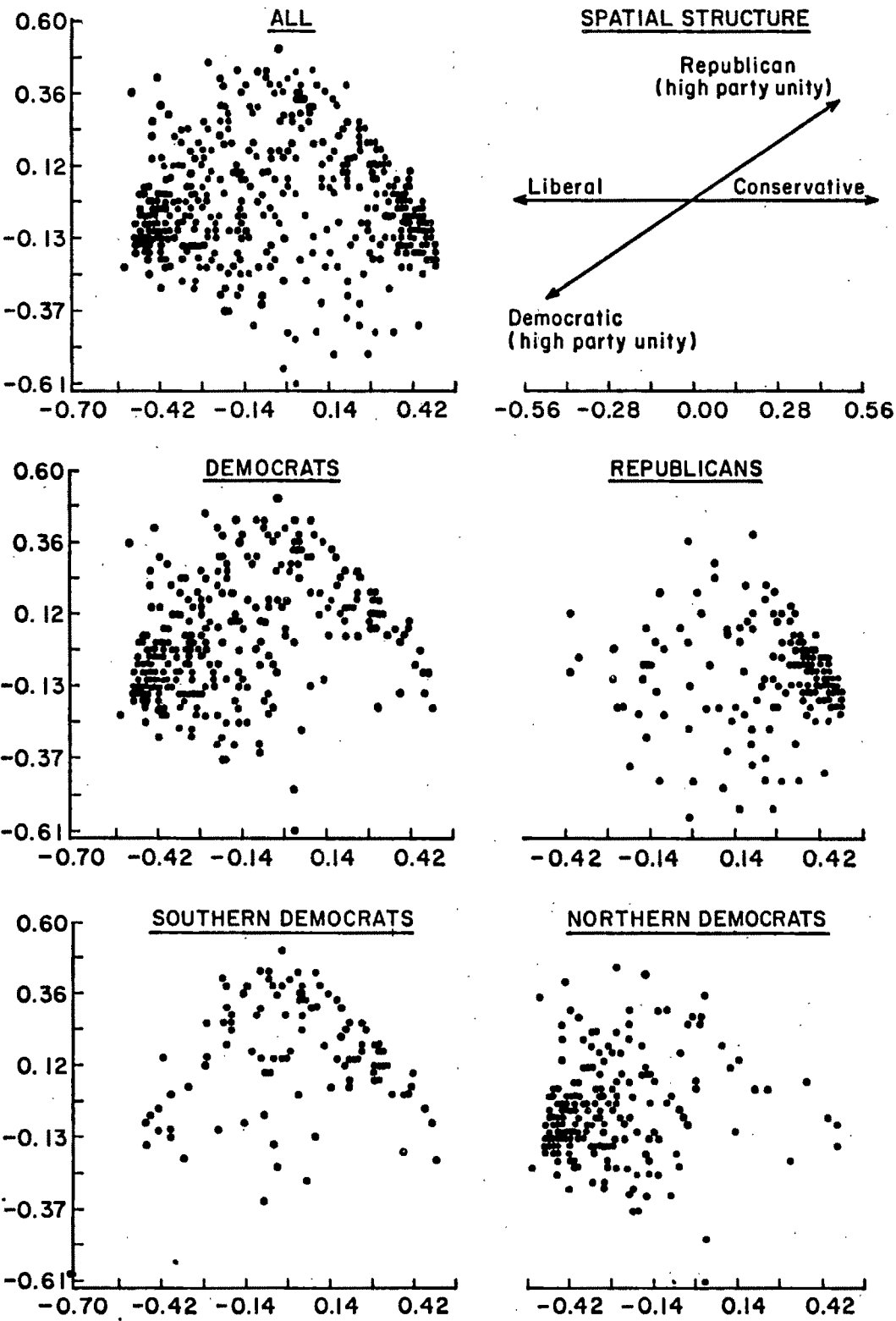


Table 2. Comparability of the Senate and House Interest Group Ratings

	House		Senate	
	1 dimension	2 dimensions	1 dimension	2 dimensions
1980	.9996 ^a	.9938 ^b	.9976	.9886
1979	.9994	.9938	.9984	.9898
1978	.9986	.9980	.9974	.9980
1977	.9992	.9878	.9934	.9728
1976	.9932	.9902	.9968	.9884
1975	.9988	.9934	.9926	.9827
1974	.9942	.9870	.9950	.9491
1973	.9960	.9912	.9950	.9577
1972	.9920	.9942	.9980	.9524
1971	.9876	.9972	.9966	.9940
1970	.9934	.9960	.9942	.9860
1969	.9904	.9948	.9876	.9722
1968	.9968	.9720	.8832	.8619
1967	.9988	.9972	.9393	.9029
1966	.9900	.9845	.9633	.9305
1965	.9994	.9892	.9688	.9746
1964	.9996	.9958	.9616	.8686
1963	.9998	.9882	.9962	.9446
1962	.9974	.9968	.9896	.9618
1961	.9992	.9932	.9992	.9681
1960	.8339	.9922	.9417	.9924
1959	.9266	.9928	.9458	.9930

^aEach entry is the squared Pearson correlation between the unidimensional configurations recovered from the unfoldings of the combined ratings and the ratings of the indicated house of Congress.

^bFor two dimensions each entry is the squared Pearson correlation of the corresponding distances between each of the $p(p-1)/2$ unique pairs of legislators in the configurations.

The histogram on the left of Figure 2 shows the entire Congress; the one on the right separates the Democrats and Republicans. The figure shows a bimodal, ideological dimension. However, there are distinctions between parties: the Democratic party is much less homogeneous than the Republican party. The Republicans are concentrated at center right to far right. The bulk of the Democratic party is concentrated at center left to far left, but substantial numbers of Democrats are located at center right and far right.

In Figure 3, we indicate the approximate two-dimensional structure of the space. As can be seen from the separate plots for the Democrats and Republicans, the party dimension seems to run about 45 degrees off the liberal-conservative dimension. Party unity scores increase with movement along the party dimension as indicated in Figure 3.

To check our assumption that the ratings are comparable between houses, we unfolded the ratings for the Senate and House separately for each year of our study and then compared the resultant one- and two-dimensional coordinates for each house with those recovered from the combined unfoldings. The results are shown in Table 2 which displays the Pearson r -squares between the

configurations from the separate unfoldings and the configurations from the combined Senate-House unfoldings.^a

Table 2 strongly supports our assumption of comparability. Since 1969, the r -squares for both the House and Senate for one dimension are .99 or better. For two dimensions, they are .95 or better. The r -squares for the House tend to be slightly higher than those for the Senate, because there are four times as many Representatives as Senators so that the combined unfolding will more closely resemble the unfolding of only the House members' ratings.

Comparability clearly fails in only two instances—the Senate ratings of 1968 (r -squares of .88 and .86) and the House ratings of 1960 (r -square of .83 for one dimension). In both cases, the number of

^aThe unidimensional r -squares are simply the squared Pearson correlations between the corresponding configurations. For two-dimensional configurations, this is an inappropriate method because of differences in rotation and origin. Instead, we computed the distances between each unique pair of legislators in the corresponding configurations. The r -squares are the squared Pearson correlations between the corresponding vectors of $p(p-1)/2$ distances.

interest groups was small (9 and 8, respectively) so that bias in just two or three groups' ratings will present problems. As the number of interest groups increases, bias, if it exists, becomes much less of a problem. This can be seen in the *r*-squares for the Senate configurations in Table 2: they tend to be higher in recent years when there are far more interest groups issuing ratings.

Tables 1 and 2 viewed in conjunction show that, empirically, the interest group ratings are highly consistent with the spatial model we outlined in the previous section. On average, an 85 rating given by a group to a senator is the same as an 85 rating given to a representative. That this is true over long periods of time enables us to introduce the dimension of time into the spatial model. In our time series analysis below, we include the 1960 and 1968 configurations for completeness. Their inclusion does not appreciably affect our results. In our opinion, the average positions (see Figure 4) of various groups of representatives and senators in the common scaled metric are reliable, but the positions of the individual legislators in

1960 and 1968 must be regarded with suspicion because of the results shown in Table 2.

Between 1959 and 1980, 1,426 individuals served in the House and Senate.⁹ We applied the model stated in equation (3) to the one- and two-dimensional configurations. To obtain accurate estimates of *W* and *c*, we included only those 873 legislators who served six or more years during the period of our study. Accordingly, our *X*₀ matrices were 873 by 22 and 873 by 44, respectively. The overall fit of the model and the fits for each year are shown in Table 3.

For one dimension, the overall *r*-square of the model stated in equation (3) is .939. The magnitude of this *r*-square indicates that the members of Congress are very stable in their location on the liberal-conservative dimension over time. This stability is apparent in the fits for the separate

⁹This number includes duplications—namely, those members of the House of Representatives who later served in the Senate are counted twice. See Table 4.

Table 3. Fit Statistics for Congressional Coordinates

No. of Dimensions	No. Legislators Included	% Missing	Minimum No. of Years	Overall <i>r</i> ²
1	873	47.1	6	.939
2	873	47.1	6	.919

Year	No. Legislators Included	One-Dimension	Two-Dimensions	
		<i>r</i> ²	<i>r</i> ² 1st	<i>r</i> ² 2nd
1959	368	.888	.945	.715
1960	373	.913	.926	.736
1961	426	.934	.973	.410
1962	434	.928	.962	.367
1963	491	.933	.967	.114
1964	492	.940	.967	.370
1965	479	.952	.966	.159
1966	479	.949	.961	.131
1967	512	.934	.948	.258
1968	511	.897	.929	.127
1969	511	.927	.965	.585
1970	509	.930	.960	.726
1971	511	.933	.953	.754
1972	505	.921	.954	.022
1973	501	.962	.953	.789
1974	500	.937	.954	.613
1975	507	.964	.968	.789
1976	502	.962	.966	.758
1977	427	.965	.965	.752
1978	421	.956	.950	.608
1979	351	.945	.939	.683
1980	352	.942	.935	.624
Overall ^a	873	.939	.919	

^aFor both one and two dimensions, 47.1% were missing. Legislators served a minimum of six years.

years, the lowest of which is .888 for 1959.¹⁰ The high correlation between the adjusted yearly configurations (X_0 corrected by \hat{W} and \hat{c}) and the estimated average or target configuration (\hat{E}) supports the arguments of Clausen (1973), Bullock (1981), and Stone (1977; cited by Kuklinski, 1979) that variations in the voting patterns in Congress arise mainly through generational replacement.

The results for the two-dimensional configurations are of lesser quality. Although the overall r -square of .919 is quite satisfactory, it does not carry the same message of stability as does the one-dimensional r -square.

As Table 1 and Figure 3 demonstrate, the second dimension does not account for much of the variance of the two-dimensional configurations. The yearly fits reflect this. The overall fit is high because the first dimension, which accounts for most of the variation in the configurations, is being estimated very well. This is not true of the second dimension. The pattern of fits for the second dimension is consistent with the unfolding results of Table 1. As we discussed earlier, a second dimension is clearly present in 1959 and 1960, is largely absent from 1961 to 1968, and then reappears from 1969 to 1980. With the exception of 1972, the fits for the second dimension follow the same pattern. Because the second dimension does not account for much of the variance in the ratings and demonstrates the instability shown in Table 3, we will not attempt to use it in our time series analysis. We will use the party dimension when we study roll calls within a year, however, because party is an important consideration in many roll call votes. The party dimension is unstable from year to year largely because the interest groups do not choose many party-line votes to construct their ratings. They choose votes on issues that override considerations of party loyalty. In spatial terms, the interest groups tend to be located near the ends of the liberal-conservative dimension. The second dimension does tend to separate the interest groups somewhat, but not anywhere near the extent that it separates the legislators (Poole, 1981).

Figure 4 shows the mean locations of all senators, representatives, and the two parties and

their northern and southern wings for the 22-year period. We applied \hat{W} and \hat{c} to every member for each respective year, so the means shown are for everyone serving and not just those who served at least six years. With the exception of 1960, the Senate as a body is more liberal than the House. (This finding is consistent with the evidence produced by other researchers, for example, Froman (1963, 1967).) The northern Democrats are the most liberal and stable group. The mean location of the northern Democrats hardly moved at all over the 22 years. In contrast, southern Democrats shifted significantly to the right during the years of intense civil rights activity in the Johnson administration (1964-1968). After 1972, however, when civil rights became a less salient issue in the Congress, they shifted back almost to the center of the spectrum, where they had been before 1964.

The congressional Republican party is more homogenous than the congressional Democratic party. In 1959 there were only 10 southern Republicans in Congress. In 1973, this reached a peak of 45 before the Watergate losses in 1974 brought it down to 35. By 1979, southern Republicans had recovered some of their losses and held 41 House and Senate seats. As the number of southern Republicans increased, a gap opened up between northern and southern Republicans. This gap reached its peak in the early 1970s and then declined in the later 1970s. Interestingly, the gap between southern Republicans and southern Democrats was the smallest during the late 1960s, the period of civil rights activism and urban unrest. In fact, the southern Democrats and southern Republicans were closer together than either was to their respective northern counterparts. After 1975, however, the gap between southern Democrats and southern Republicans was nearly as large as that between southern and northern Democrats.

Overall, Congress drifted slightly to the left between 1959 and 1975 (interestingly, the Watergate election of 1974 produced a Congress more liberal than the Congress produced by Lyndon Johnson's 1964 landslide) and after 1976 drifted back to the right, which undoubtedly continued in 1981. The conclusion that Figure 4 points to is that the civil rights, civil disturbance, Vietnam, Watergate era of 1965-1974 was an interruption of normal liberal-conservative patterns. The 1959-1964 and the 1975-1980 periods are quite similar, whereas the 1965-1974 period is unlike either of the other two.

During the 22 years of our study, 38 current or former members of the House were elected to the Senate (see Table 4). The stability of the configurations from year to year suggests that legislators moving from the House to the Senate will

¹⁰Because only those members serving six or more years were included in the estimation of equation (3), this reduced the number of members included in the analysis from the early and late years. That is, to be included in the 1959 row of Table 3, a member would have had to serve six years beginning in 1959. In contrast, to be included in the 1969 column, a member could have served from 1963-1969 or 1969-1975. Finally, to be included in the 1980 column, a member would have had to serve six years ending in 1980 (the six years do not have to be consecutive).

Table 4. Transitions from the House to the Senate: 1959-1980*

	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969
Abourezk											
Armstrong											
Bass	-0.192	-0.347	-0.567	-0.593	-0.449	-0.439	-0.612*	-0.552			
Baucus											0.196
Beall											
Brewster	-0.246	-0.503	-0.413	-0.360	-0.467*	-0.457	-0.637	-0.563	-0.740	-0.457	
Burdick	-0.650	-0.402*	-0.631	-0.696	-0.435	-0.408	-0.701	-0.467	-0.276	-0.299	-0.523
Cochran											
Cohen											
Culver											
Dole			0.873	0.828	0.829	0.842	-0.559	-0.480	-0.686	-0.678	-0.828
Dominick			0.578	0.577	0.627*	0.616	0.736	0.738	0.703	0.681	0.585*
Edmondson			-0.629	-0.627	-0.166*	-0.148	0.543	0.527	0.410	0.357	0.677
Edwards							-0.021	0.248	0.107	0.124	0.438
Goodell	0.517	0.699	0.587	0.562	0.633	0.653	0.609	0.534	0.370	-0.404*	-0.651
Griffin	0.588	0.602	0.524	0.422	0.520	0.596	0.399	0.201*	-0.118	-0.302	0.354
Gurney					0.930	0.845	0.788	0.805	0.848	0.941	0.990*
Hathaway							-0.645	-0.736	-0.681	-0.766	-0.723
Heinz											
Inouye	-0.590	-0.681	-0.632	-0.625	-0.564*	-0.607	-0.653	-0.625	-0.585	-0.564	-0.648
Mathias			0.151	-0.015	-0.185	-0.148	-0.064	-0.097	-0.260	-0.258	-0.437*
Matsunaga					-0.647	-0.676	-0.700	-0.745	-0.681	-0.765	-0.674
McClure									0.761	0.718	0.657
McGovern	-0.580	-0.630			-0.664*	-0.721	-0.735	-0.555	-0.763	-0.786	-0.849
Melcher											-0.353
Metcalf	-0.650	-0.625	-0.580*	-0.604	-0.576	-0.633	-0.559	-0.492	-0.764	-0.574	-0.687
Montoya	-0.598	-0.602	-0.499	-0.521	-0.355	-0.340	-0.431*	-0.511	-0.251	-0.250	-0.455
Pressler											
Pryor									0.142	-0.008	-0.061
Riegle									0.148	-0.156	-0.443
Roth									0.561	0.451	0.317
Sarbanes											
Schweiker			0.341	0.267	0.252	0.208	-0.085	-0.079	0.067	-0.222	-0.551*
Scott									0.778	0.806	0.639
Stafford			0.250	0.131	0.252	0.192	0.114	0.168	0.048	-0.262	-0.293
Tsongas											
Tunney											
Weicker							-0.560	-0.499	-0.576	-0.683	-0.799
											-0.155

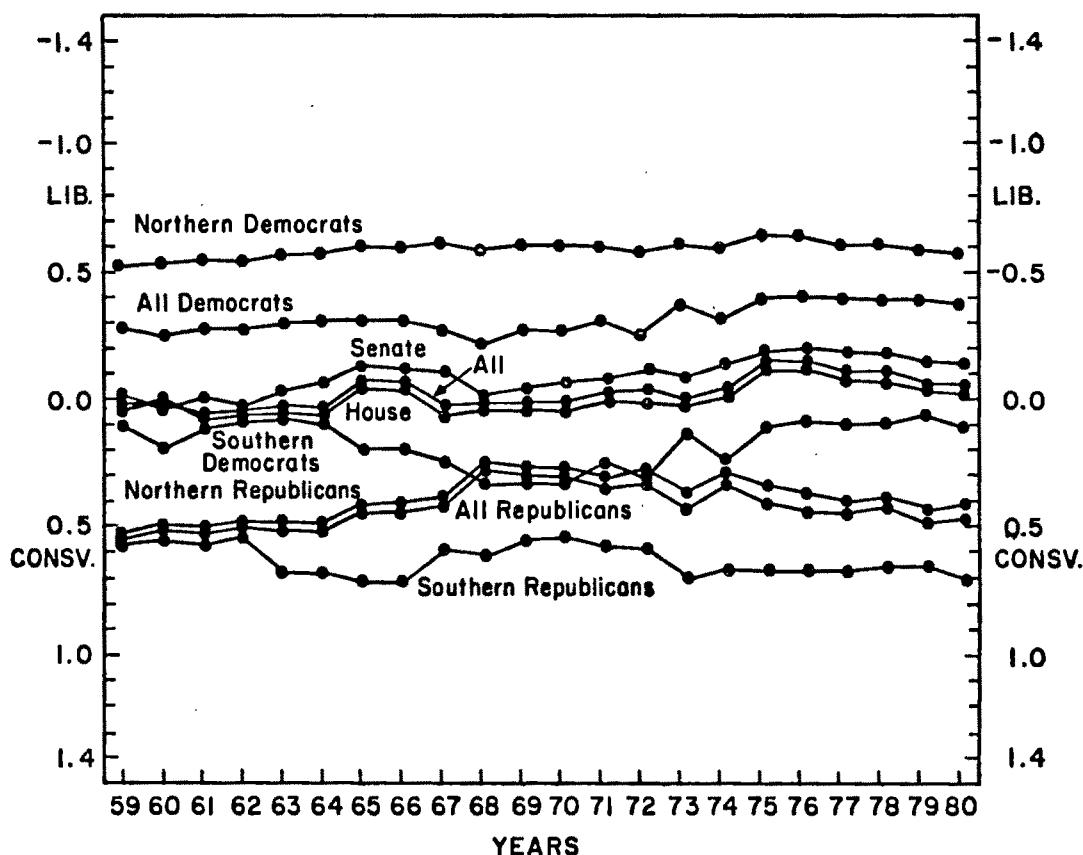
*The first year the individual was in the Senate.

TABLE 4 (continued)

	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980
Abourezk		-0.970	-0.640	-0.734*	-0.802	-0.761	-0.804	-0.891	-0.793		
Armstrong				0.799	0.716	0.702	0.856	0.807	0.789	0.804*	0.815
Bass											
Baucus						-0.632	-0.603	-0.468	-0.504	-0.712*	-0.724
Beall	0.300	0.230*	0.200	0.200	0.284	0.099	-0.033	0.131			
Brewster											
Burdick	-0.590	-0.527	-0.712	-0.596	-0.723	-0.485	-0.582	-0.364	-0.530	-0.517	-0.481
Cochran				0.720	0.643	0.565	0.471	0.701	0.727	0.466*	0.634
Cohen				-0.137	-0.242	-0.124	-0.211	-0.026	-0.179	-0.014*	-0.127
Culver	-0.815	-0.950	-0.819	-0.724	-0.677	-0.878*	-0.853	-0.927	-0.901	-0.879	-0.941
Dole	0.572	0.558	0.571	0.507	0.553	0.368	0.582	0.513	0.357	0.395	0.410
Dominick	0.612	0.624	0.535	0.593	0.691						
Edmondson											
Edwards	0.490	-0.038	0.646*								
Goodell	-0.941			0.631	0.443	0.542	0.645	0.609	0.394		
Griffin	0.437	0.248	0.223	0.593	0.821						
Gurney	0.758	0.609	0.580	-0.783*	-0.861	-0.738	-0.715	-0.881	-0.805	-0.288	-0.212
Hathaway	-0.836	-0.928	-0.714	-0.287	-0.329	-0.312	-0.231	-0.232*	-0.337	-0.577	-0.459
Heinz		-0.347	-0.613	-0.631	-0.700	-0.555	-0.470	-0.622	-0.661	-0.700	-0.788
Inouye	-0.823	-0.536	-0.615	-0.462	-0.747	-0.603	-0.512	-0.552	-0.551	-0.658	-0.800
Mathias	-0.535	-0.455	-0.480	-0.734	-0.770	-0.692	-0.726	-0.723*	-0.704	-0.723	-0.784
Matsunaga	-0.830	-0.624	-0.452	-0.734	-0.770	0.974	0.765	0.802	0.660	0.846	0.784
McClure	0.664	0.627	0.632	0.651*	0.930	0.793	0.765	0.802	0.660	-0.991	-0.881
McGovern	-0.842	-0.768	-0.807	-0.734	-0.826	-0.549	-0.804	-0.882	-0.768	-0.382	-0.360
Melcher	-0.394	-0.609	-0.580	-0.537	-0.328	-0.793	-0.464	-0.435*	-0.421		
Metcalf	-0.813	-0.665	-0.705	-0.617	-0.760	-0.776	-0.804	-0.859			
Montoya	-0.524	-0.417	-0.382	-0.411	-0.236	-0.047	-0.141				
Pressler						-0.072	-0.063	0.357	0.129	0.061*	0.142
Pryor	-0.142	-0.588	-0.446							-0.129	-0.116
Riegle	-0.372	-0.700	-0.836	-0.745	-0.853	-0.761	-0.651	-0.835*	-0.842	-0.934	-0.968
Roth	0.403	0.378*	0.340	0.426	0.361	0.298	0.454	0.525	0.677	0.462	0.542
Sarbanes		-0.686	-0.852	-0.850	-0.887	-0.889	-0.882	-0.959*	-0.877	-0.869	-0.955
Schweiker	-0.501	-0.456	-0.629	-0.569	-0.645	-0.817	-0.732	0.184	0.226	0.351	0.404
Scott	0.719	0.764	0.742	0.936*	1.000	0.835	1.002	0.789	0.771		
Stafford	-0.285	-0.242*	-0.228	-0.299	-0.354	-0.516	-0.374	-0.276	-0.417	-0.441	-0.348
Tsongas						-0.871	-0.859	-0.940	-0.778	-0.864*	-0.880
Turney	-0.808	-0.770*	-0.839	-0.663	-0.767	-0.542	-0.539				
Weicker	-0.197	0.132*	0.012	0.016	-0.393	-0.325	-0.311	-0.234	-0.465	-0.461	-0.685

*The first year the individual was in the Senate.

Figure 4. Change in Voting Blocs over Time



not alter their spatial position. On the other hand, the Senate is more liberal than the House. Senators represent whole states and must cater to a more diverse constituency (Froman, 1963), which suggests that legislators moving to the Senate will, if they shift at all, shift left.

Table 4 is largely consistent with this interpretation. For the most part, a legislator's first few years in the Senate are spatially very close to his or her last few years in the House. Edmondson (D Okla.) and Edwards (D La.), who shifted abruptly to the right, and Griffin (R Mich.), who shifted sharply to the left over a four-year period, are the exceptions. Other legislators, Mathias (R Md.), Riegle (D Mich.), Weicker (R Conn.), and Stafford (R Vt.), for example, shifted their positions, but over much longer periods of time.

In a class by themselves are Richard Schweiker (R Pa.) and Charles Goodell (R N.Y.). During Schweiker's eight years in the House (1961-1968) he drifted slowly from center-right to center. After his election to the Senate, Schweiker shifted to the center-left and continued to drift left until

his selection as Ronald Reagan's running mate in 1976. The trip to the mountain top with Reagan had a profound effect upon Schweiker—it produced the largest single one-year shift in the 22 years of our data. By the time Schweiker retired in 1980, he was slightly to the right of where he was when he began his career in 1961. In contrast, Goodell was much more stable than Schweiker during his years in the House (1959-1967 in our data). He was appointed to the Senate by Governor Nelson Rockefeller in 1968 to serve out the remainder of Robert Kennedy's Senate term. Goodell, who had been a moderately conservative upstate New York congressman, by 1970 had become, in Spiro Agnew's view, a radical liberal. In fact, Agnew's characterization was not all that far off. In 1970 Goodell was more liberal than either Edward Kennedy or George McGovern. Goodell's three-year shift is the largest in our data.

Legislators who served in both the House and the Senate do not of course have a monopoly on long-term spatial movement. One of the most in-

interesting examples is John Anderson (R Ill.), who ran for the presidency as an independent in 1980. Anderson appears to have gone through three phases during his 20-year career in the House. From 1961 to 1966 he was more conservative than Gerald Ford, the Republican minority leader.¹¹ Between 1967 and 1968, he abruptly shifted to the center of the spectrum where he remained until his presidential campaign of 1979-1980, when he abruptly shifted to center left. That Anderson, a moderate to moderate conservative for the bulk of his career, should attract so much support from liberals in 1980, is eloquent testimony to the depth of the dissatisfaction with President Carter.

In conclusion, it is clear from the discussion above that the interest group ratings are highly structured. On average, a single liberal-conservative dimension accounts for 81% of the variance of the 190,000 interest-group ratings issued from 1959 to 1980. Furthermore, the interest groups and the legislators are very stable on the liberal-conservative dimension over time. We now turn to the question of the usefulness of the one- and two-dimensional spatial configurations of legislators derived from the ratings for roll-call vote analyses.

Analysis of the Roll-Call Votes

The spatial model we described in the second section assumes that each roll-call vote has two outcome locations in the evaluative space. The interest groups and the legislators react to these policy outcomes in different ways. The interest groups are not members of Congress. They can indulge in the luxury of "voting" on only those matters of great importance to them. In spatial terms, the interest groups are free to "vote" on only those roll-calls with outcomes near them in the space; otherwise, they abstain. Legislators have no such freedom. In a real-world voting situation, a member of Congress cannot afford to indulge in the luxury of not voting or abstaining too often. Consequently, we assume that legislators vote for the outcome closest to them in the space, even though in some situations it may be a choice between two distant outcomes. Therefore, in a perfect world, there will be a point (in one dimension) or cutting line (in two dimensions)

such that all legislators to the left of the point/line will vote for the "left" outcome and all members to the right of the point/line will vote for the "right" outcome.

Because this cutting point/line construct is inherent in the model of the interest group ratings (and given the success of the spatial model of the ratings), it is practically certain that the roll calls chosen by the interest groups will produce clearly defined cutting points/lines in the legislator configurations. The more interesting question is how well the roll calls not chosen by the interest groups will do. If both sets of roll calls produce equally defined cutting points/lines in the configurations, then this is good evidence that the configurations recovered from the ratings are not biased. By "biased" we mean the following. Suppose the interest groups were all on a hyperplane through the space of the legislators. They would therefore tend to select roll calls with outcomes on the hyperplane to construct their ratings, and this would result in the recovery of a distorted version of the true configuration of legislators.

To locate the cutting point/line for a roll call, we place it within the pattern of yea and nay voting such that it minimizes error relative to an ideal pattern. For example, suppose we are working with a single dimension and we observe:

YYYYY ··· YYNYYY|NNYNNNN ··· NNNNN

Placing the cutting point as shown minimizes the error with respect to a pattern in which everyone to the left of the cutting point votes yea and everyone to the right votes nay. In this example, three errors are made. For an empirical example, consider Table 5. It displays the members of the Senate in the order of their recovery on the liberal-conservative dimension in 1972 and 1977 along with how they voted on the Jackson amendment to the SALT I treaty in 1972 and how they voted on four SALT related motions in 1977.¹²

¹²Jackson (D Wash.) amendment—as amended by voice votes to state that continued modernization of U.S. nuclear forces was required for a prudent nuclear posture but expressing the hope that such actions would become less necessary in the future—Request that any future permanent treaty on offensive nuclear arms "not limit the United States to levels on intercontinental strategic forces inferior to" those of the Soviet Union but be based rather on "the principle of equality"; endorse the maintenance of a vigorous research, development, and modernization program and provide that failure to negotiate a permanent treaty limiting offensive arms would "jeopardize supreme national interests" of the United States and would be grounds for abrogating the U.S.-Soviet treaty limiting defensive nuclear weapons (1972 CQ Almanac, p. 62-S, CQ vote

¹¹Interestingly, George Bush, who served in the House from 1967-1970, was also more conservative than Ford. In 1980, Jesse Helms (one of the most conservative members of Congress), the Moral Majority, and their allies opposed putting Bush on the ticket with President Reagan. Yet during his years in Congress, Bush's voting record was more conservative than ex-President Gerald Ford's—who almost ended up on the ticket.

Table 5. Voting on Strategic Arms Limitations Motions

SALT I Treaty (1972) (Jackson Amendment)		Related Votes (1977)				
			Warnke	Warnke	Helms	Church
Muskie	N	Kennedy	Y	Y	N	Y
Kennedy	N	Sarbanes	Y	Y	N	Y
Nelson	N	Metzenbaum	Y	Y	N	Y
Mondale	N	Clark	Y	Y	N	Y
Hart	N	Culver	Y	Y	N	Y
Cranston	N	Pell	Y	Y	N	Y
Hughes	N	Williams	Y	Y	N	Y
Williams	N	Abourezk	Y	Y	Y	N
Harris	N	McGovern	Y	Y	Y	Y
Tunney	N	Hathaway	Y	Y	N	Y
Eagleton	N	Case	Y	Y	N	Y
Case	N	Brooke	Y	Y	N	Y
McGovern	N	Metcalf	Y	Y		
Ribicoff	N	Riegle	Y	Y	N	Y
Hartke	N	Nelson	Y	Y	N	Y
Gravel	N	Haskell	Y	Y	N	Y
Stevenson	N	Anderson	Y	Y	N	Y
Proxmire	N	Javits	Y	Y	N	Y
Humphrey	N	Cranston	Y	Y	N	Y
Bayh	N	Ribicoff	Y	Y	N	Y
Pell	N	Bayh	Y	Y	N	Y
Javits	N	Humphrey	Y	Y	N	Y
Church	N	Muskie	Y	Y	N	Y
Symington	N	Durkin	Y	Y	N	Y
Mansfield	N	Biden	Y	Y	N	Y
Moss	N	Matsunaga	Y	Y	N	Y
Brooke	N	Leahy	Y	Y	N	Y
Burdick	N	Hart	Y	Y	N	Y
Metcalf	N	Jackson	N	Y	N	Y
Pastore	Y	Inouye	Y	Y	N	Y
Schweiker	N	Moynihan	N	Y	N	Y
Inouye	Y	Eagleton	Y	Y	N	Y
Magnuson	Y	Church	Y	Y	N	Y
Percy	Y	Magnuson	N	Y	N	Y
Mathias	N	McIntyre	Y	Y	N	Y
McIntyre	Y	Gravel	Y	Y	N	Y
Hatfield	N	Mathias	Y	Y	N	Y
Montoya	Y	Glenn	Y	Y	N	Y
Randolph	Y	Bumpers	Y	Y	N	Y
Fulbright	N	Stevenson	Y	Y	N	Y
Jackson	Y	Proxmire	Y	Y	N	Y
McGee	Y	Sasser	Y	Y	N	Y
Stafford	N	Melcher	Y	Y	N	Y
Stevens	Y	Huddleston	N	Y	N	Y
Chiles	Y	Burdick	Y	Y	N	Y
Boggs	Y	Ford	Y	Y	N	Y
Pearson	Y	Stafford	Y	Y	N	N
Anderson	Y	Percy	Y	Y	N	N
Hollings	Y	Byrd	Y	Y	N	Y
Byrd	Y	Weicker	N	N		
Bible	Y	Deconcini	Y	Y	N	Y
Bentsen	Y	Heinz	N	Y	N	N
Aiken	N	Randolph	Y	Y	N	Y
Cannon	Y	Hatfield	Y	Y	N	Y
Weicker	N	Chafee	Y	Y	N	Y
Scott	Y	Sparkman	Y	Y	N	Y
Smith	N	Packwood	N	Y	N	N
Packwood	Y	Bentsen	Y	Y	N	Y

TABLE 5 (continued)

SALT I Treaty (1972) (Jackson Amendment)		Related Votes (1977)				
			Warnke	Warnke	Helms	Church
Spong	Y	Hollings	Y	Y	N	Y
Cooper	N	Pearson	Y	Y	N	Y
Saxbe	Y	McClellan	Y	N		
Beall	Y	Stone	N	Y	N	Y
Cook	Y	Cannon	N	Y	N	Y
Griffin		Chiles	N	Y	N	Y
Long	Y	Long	Y	Y	N	Y
Taft	Y	Johnston	Y	Y	N	Y
Roth	Y	Danforth	N	N	N	N
Bellmon	Y	Schweiker	N	N	Y	N
Fong	Y	Talmadge	N	Y	N	N
Gambrell	Y	Stevens	N	Y	N	N
McClellan	Y	Morgan	Y	Y	Y	Y
Miller	Y	Baker	N	N	N	N
Sparkman	Y	Nunn	N	Y	N	Y
Young	Y	Eastland	Y	Y	N	Y
Jordan	Y	Zorinsky	N	N	Y	N
Talmadge	Y	Bellmon	N	N	N	N
Dominick	Y	Young	Y	Y	N	N
Allott	Y	Stennis	N	N	N	Y
Jordan	Y	Dole	N	N	Y	N
Dole	Y	Domenici	N	N	Y	N
Gurney	Y	Thurmond	N	N	Y	N
Baker	Y	Roth	N	Y	N	N
Allen	Y	Lugar	N	N	Y	N
Buckley	Y	Allen	N	N	Y	N
Byrd	Y	Griffin	N	N	Y	N
Edwards	Y	Schmitt	N	N	N	N
Hruska	Y	Byrd	N	N	N	N
Ervin	Y	Bartlett	N	N	Y	N
Bennett	Y	Laxalt	N	N	N	Y
Stennis	Y	Tower	N	N	N	N
Eastland	Y	Hayakawa	N	N	N	Y
Curtis	Y	Hansen	N	N	Y	N
Cotton	Y	Goldwater	N	N	N	N
Brock	Y	Wallop	N	N	Y	N
Tower	Y	Curtis	N	N	Y	N
Thurmond	Y	Scott	N	N	Y	N
Fannin	Y	Hatch	N	N	Y	N
Hansen	Y	McClure	N	N	Y	N
Goldwater	Y	Helms	N	N	Y	N
		Garn	N	N	Y	N
% Predicted		% Predicted	88.0	93.0	88.7	89.7
2 Party		2 Party	77.0	81.0	80.4	84.5
3 Party		3 Party	77.0	81.0	80.4	84.5
Pre 2 Party		Pre 2 & 3	.478	.632	.423	.335
Pre 3 Party			.510			

The cutting lines shown in the table fit well; 90 of 99 senators on the Jackson amendment, 88 and 93 of 100 senators on the two Warnke votes, 86 of 97 senators on the Helms motion, and 87 of 97 senators on the Church motion are consistent with the spatial model. Viewed in isolation, these figures are quite impressive. However, as Weisberg (1978) has pointed out, simply "predicting" that a member will vote with the majority of his or her party (the two-party model) or, better yet, "predicting" that a member will vote with a majority of his or her party where the Democratic party is split into its northern liberal and southern conservative wings (the three-party model), will yield very accurate results. Weisberg found that the two-party model correctly accounted for 82.4% of House votes from 1957-1974; the three-party model correctly accounted for 84.8%. Table 5 shows that the liberal-conservative dimension outperforms both the two- and three-party models on these SALT related votes. As a basis of comparison, in Table 5 we also show lambda, the statistic recommended by Weisberg to measure "predictive" improvement. Lambda measures the proportional reduction in error of one model over another. For example, on the vote to confirm Paul Warnke as director of the Arms Control and Disarmament Agency, the liberal-conservative dimension correctly accounts for 93 of 100 senators, whereas the two- and three-party models correctly account for only 81 senators. The proportional reduction in error (PRE) is, therefore, $(93 - 81)/(100 - 81) = .632$, or 63.2%. All the lambda values in Table 5 are above .3, which indicates that voting on Strategic Arms Limitation—arguably the most important question of our age—is substantially ideological.¹³

#401). The vote took place on September 14, 1972. Confirmation of President Carter's nomination of Paul C. Warnke to head the U.S. Delegation to the Strategic Arms Limitation Talks (SALT) with the Soviet Union (1977 *CQ Almanac*, p. 8-S, CQ vote #41). Confirmation of President Carter's nomination of Paul C. Warnke to be Director of the Arms Control and Disarmament Agency (ACDA) (1977 *CQ Almanac*, p. 8-S, CQ vote #42). Both Warnke votes were on March 9, 1977. Helms (R N.C.) motion to table, and thus kill, the resolution that would express congressional support for President Carter's decision to adhere to the arms ceilings in the 1972 U.S.-Soviet interim agreement on strategic weapons (SALT I) after that agreement expired on October 3, 1977. Church (D Idaho) motion to table, and thus kill, the McClure (R Idaho) amendment to stipulate that the resolution would not prohibit the United States from development of any nuclear weapon system (1977 *CQ Almanac*, p. 74-D, CQ votes #513 and #514). Both votes took place on October 3, 1977.

¹³Three especially glaring errors in Table 4 are the votes of Abourezk and McGovern on the Helms and

Table 6 shows the percentage of votes cast in the 11 congresses in our study that are consistent with the one- and two-dimensional spatial model, the two-party model, and the three-party model. With the exception of the 88th Senate, in both Houses and all Congresses the one- and two-dimensional spatial models outperform the two- and three-party models for all roll-call votes and for all three categories of nonunanimous votes (minority not less than 5%, 20%, and 40% of the total vote cast, respectively). The three- and two-party lambdas are low in the first three Congresses, but are substantial otherwise. Overall, the party dimension only accounts for an additional 1.5% of House voting and 2.3% of Senate voting. However, on closer votes, the party dimension plays a more important role. On roll calls with at least 40% in the minority, party accounts for an additional 2.6% of House voting and 2.7% of Senate voting. Consistent with the interest-group ratings, the party dimension is strongest for the 86th Congress. The results for the 92nd through 94th Congresses are consistent with Schneider's (1979, p. 147) argument that a single liberal-conservative dimension structured congressional voting during this period.

Table 7 offers some evidence bearing on the bias question we raised above. In the 96th Congress, there is little or no difference between the set of votes chosen by the interest groups and the set not chosen in terms of the percentage of the vote consistent with the three models. This result holds even for the subset of roll calls that are chosen by five or more groups. The roll calls chosen by the interest groups from the 96th Congress are a representative sample of the total set of roll calls.

We put quotation marks around the word predict above because, in reality, none of these models is predicting anything because they are applied ex post. Suppose, however, that a random sample of legislators were polled before a roll call and they reported how they were going to vote. For true predictive purposes, the spatial model is the most parsimonious because only one parameter (the cutting point/line) is estimated from the sample, whereas two and three parameters (the majority preference of each subgrouping) are estimated for the two- and three-party models respectively.¹⁴

Church motions. Their votes, contrary to President Carter's position, are almost certainly due to their pique at President Carter's handling of the natural gas pricing issue. A number of key votes on natural gas (Abourezk had led a filibuster which had been shut off through questionable tactics) took place on October 1 and October 3.

¹⁴Of course, if you had a true random sample, the best prediction would be the division found in the sam-

Table 6. Consistency of Roll Calls with Spatial and Party Models

Congress	Number Roll Calls	Spatial Model		Party Model		Two Party Lambda		Three Party Lambda	
		1 Dim	2 Dim	Two	Three	1 Dim	2 Dim	1 Dim	2 Dim
HOUSE									
All Votes									
86	180	84.2	88.6	80.9	84.2	.17	.40	.00	.28
87	240	87.7	89.8	84.3	85.9	.22	.35	.13	.28
88	232	88.2	89.3	85.0	86.4	.21	.29	.13	.21
89	394	89.8	90.9	84.6	87.1	.34	.41	.21	.30
90	478	88.5	89.8	83.4	86.0	.30	.39	.17	.27
91	443	87.7	89.6	82.3	85.0	.31	.41	.18	.31
92	649	86.7	88.6	80.7	83.5	.31	.41	.18	.31
93	1078	85.4	87.2	81.0	82.6	.23	.33	.16	.27
94	1273	86.4	87.7	81.3	83.3	.28	.34	.19	.26
95	1540	86.9	88.1	82.7	84.1	.24	.31	.17	.25
96	1284	86.8	88.1	82.9	84.1	.23	.31	.17	.25
Total	7791	86.9	88.4	82.3	84.2	.26	.35	.17	.27
Nonunanimous votes 5% minority									
86	168	82.1	87.2	78.3	82.1	.17	.41	.00	.28
87	225	85.0	87.6	80.9	82.8	.22	.35	.13	.28
88	219	86.1	87.4	82.3	84.0	.22	.29	.13	.21
89	288	86.7	88.1	79.7	83.0	.34	.42	.22	.30
90	411	84.9	86.6	78.2	81.7	.31	.39	.18	.27
91	317	83.7	86.1	76.1	79.9	.32	.42	.19	.31
92	482	82.7	85.2	74.9	78.5	.31	.41	.20	.31
93	850	83.7	84.3	76.5	78.5	.31	.33	.24	.27
94	1003	83.3	84.9	76.9	79.4	.28	.35	.19	.27
95	1107	82.6	84.1	76.8	78.8	.25	.31	.18	.25
96	971	83.3	85.0	78.2	79.8	.23	.31	.17	.25
Total	6041	83.6	85.2	77.4	79.8	.27	.35	.19	.27
Nonunanimous votes 20% minority									
86	138	81.4	86.8	77.0	81.4	.18	.42	.00	.29
87	154	83.7	86.9	79.2	81.6	.22	.37	.12	.29
88	155	85.0	86.4	80.6	82.7	.23	.30	.13	.21
89	230	85.7	87.3	77.7	81.6	.36	.43	.22	.31
90	270	83.3	85.3	74.9	79.5	.33	.42	.18	.28
91	208	80.1	83.2	70.3	75.8	.33	.43	.18	.30
92	369	80.3	83.4	71.0	75.8	.32	.43	.19	.31
93	613	79.9	82.0	72.1	74.9	.28	.36	.20	.28
94	758	81.3	83.1	73.6	76.9	.29	.36	.19	.27
95	813	79.8	81.6	72.7	75.4	.26	.33	.18	.25
96	746	81.3	83.2	75.5	77.5	.24	.32	.17	.25
Total	4454	80.8	83.3	74.0	77.2	.26	.36	.16	.27
Nonunanimous votes 40% minority									
86	57	79.7	85.3	74.7	79.3	.20	.42	.02	.29
87	82	84.6	87.4	81.4	83.8	.18	.33	.05	.22
88	87	86.7	88.3	83.0	84.8	.22	.31	.12	.23
89	105	87.7	89.0	78.4	83.8	.43	.49	.25	.32
90	123	84.4	87.0	76.6	81.6	.34	.44	.15	.29
91	101	78.7	82.5	68.6	75.3	.32	.44	.14	.29
92	187	79.2	82.9	70.9	76.2	.28	.41	.12	.28
93	273	77.9	81.8	71.9	75.3	.21	.35	.11	.26
94	316	80.6	82.5	71.0	77.0	.33	.40	.16	.24
95	375	79.2	81.2	71.4	75.4	.27	.34	.15	.24
96	346	80.7	82.7	74.9	77.3	.23	.31	.15	.25
Total	2052	80.8	83.3	73.5	77.7	.28	.37	.14	.25

TABLE 6 (continued)

Congress	Number Roll Calls	Spatial Model		Party Model		Two Party Lambda		Three Party Lambda	
		1 Dim	2 Dim	Two	Three	1 Dim	2 Dim	1 Dim	2 Dim
SENATE									
All votes									
86	422	84.9	89.6	80.0	83.1	.25	.48	.11	.39
87	428	84.8	87.0	79.3	82.1	.27	.37	.15	.27
88	534	82.5	86.7	77.1	83.0	.24	.42	-.03	.22
89	497	85.5	86.8	77.9	80.9	.35	.41	.24	.31
90	596	83.9	86.6	78.7	81.4	.24	.37	.13	.28
91	666	86.0	88.7	78.5	81.8	.35	.48	.23	.38
92	955	86.3	89.4	79.2	82.4	.34	.49	.22	.40
93	1138	86.1	88.1	79.1	81.3	.34	.43	.26	.36
94	1311	87.1	88.8	78.7	81.4	.39	.47	.31	.40
95	1156	85.1	86.8	77.6	79.6	.34	.41	.27	.35
96	1054	84.2	87.0	79.1	80.7	.25	.38	.19	.33
Total	8807	85.4	87.7	78.6	81.4	.32	.43	.22	.34
Nonunanimous votes 5% minority									
86	358	82.0	87.7	75.9	79.7	.25	.49	.12	.39
87	398	83.6	86.0	75.9	80.7	.27	.37	.15	.27
88	507	81.3	83.6	75.4	81.8	.24	.33	-.03	.10
89	434	83.6	85.0	74.8	78.3	.35	.43	.25	.34
90	487	80.7	83.9	74.2	77.6	.25	.38	.14	.28
91	534	82.9	86.2	73.4	77.5	.36	.48	.24	.38
92	759	83.3	87.0	74.4	78.4	.35	.48	.23	.40
93	949	83.8	86.0	75.4	78.0	.34	.43	.26	.37
94	1106	85.1	87.0	75.2	78.3	.40	.48	.31	.40
95	993	83.2	84.9	74.2	76.6	.35	.42	.28	.36
96	888	81.6	84.8	75.5	77.3	.25	.38	.19	.33
Total	7413	83.3	85.7	75.0	78.2	.33	.43	.23	.34
Nonunanimous votes 20% minority									
86	284	80.5	86.6	73.3	77.5	.27	.50	.13	.40
87	336	83.0	85.4	74.9	79.5	.29	.39	.17	.29
88	437	80.0	82.5	73.6	80.8	.24	.34	-.04	.09
89	361	82.6	84.0	72.6	76.7	.37	.42	.26	.32
90	383	78.6	82.1	70.5	74.8	.27	.38	.15	.29
91	433	81.4	85.0	70.3	75.2	.38	.50	.25	.40
92	612	81.9	86.0	71.2	76.2	.37	.51	.24	.41
93	745	82.1	84.5	72.0	75.3	.36	.45	.27	.37
94	885	83.6	85.7	72.2	76.1	.41	.49	.32	.40
95	805	81.6	83.4	71.3	74.2	.36	.42	.29	.36
96	720	79.6	83.2	72.6	74.9	.26	.39	.19	.33
Total	6001	81.5	84.2	72.1	76.1	.34	.43	.23	.34
Nonunanimous votes 40% minority									
86	123	79.5	85.0	69.5	74.6	.33	.51	.20	.41
87	147	83.5	85.3	74.0	79.8	.37	.44	.18	.27
88	146	78.6	81.7	71.1	78.4	.26	.37	.01	.15
89	140	83.8	85.3	70.1	76.5	.46	.51	.31	.37
90	155	77.3	81.1	67.7	73.4	.30	.42	.15	.29
91	201	81.2	84.9	68.1	74.4	.41	.53	.27	.41
92	285	81.6	86.3	70.5	76.3	.37	.53	.22	.42
93	295	80.2	83.0	69.9	75.0	.34	.43	.21	.32
94	344	82.2	84.2	70.4	77.1	.40	.47	.22	.31
95	303	79.5	81.5	68.1	73.2	.36	.42	.23	.31
96	331	77.4	82.2	71.3	74.5	.21	.38	.12	.30
Total	2470	80.9	83.6	70.0	75.5	.36	.45	.22	.33

Parsimony, however, is not the important difference between the models. The spatial model is based on a simple theory of voting which can incorporate the essential portions of the other two models (strength of party unity and the conservative coalition of Republicans and southern Democrats versus the northern Democrats) within its framework.¹⁵ In our opinion, it is a straightforward model "with verisimilitude to the process being studied" (Weisberg, 1978, p. 557).

The House lambdas for all votes in Table 6 are about the same magnitude as those shown by Weisberg (1978, p. 566) for the Matthews and Stimson (1975) simulation which was based on a cue-taking theory. We think the similarity of our results with those of Matthews and Stimson in terms of "prediction" stems from the finding of many researchers that legislators tend to select cue givers on the basis of similarity of policy attitudes (e.g., Kingdon, 1973, p. 76). Legislators are much more likely to share friendships with, seek advice from, and adopt the positions of fellow party members. The probability of common voting increases further if the members share similar views and are from the same state delegation. Norpoth (1976) examined the sources of party cohesion in roll-call voting and found four key elements:

- (1) representatives choose informants from the ranks of their own party groups; (2) representatives as well as informants share policy attitudes; (3) informants reach their own voting decisions on the basis of shared attitudes; and (4) representatives adopt the decisions of their informants as guides for their own behavior. (p. 1171; see also Cherryholmes & Shapiro, 1969, pp. 63-84)

In spatial terms, representatives who choose in-

ple! This overstates somewhat the case for the spatial model. The legislator configuration used for prediction would have to be estimated from the previous year's roll calls so it would not include new members. Furthermore, even though the legislator configuration is estimated from the previous year's roll calls, from the standpoint of starting the predictive experiment from scratch, it seems fairer to count the legislator locations as parameters along with the cutting points/lines. In recent years when there have been more than 500 roll calls per session, the one-dimensional spatial model would still have fewer parameters.

¹⁵As Figure 4 shows, southern Democrats are to the right of northern Democrats, and Republicans are to the right of the southern Democrats. The one-dimensional spatial model will therefore not capture votes on which a coalition of northern Democrats and Republicans form. For example, the civil rights votes in the late 1950s and early 1960s fit this pattern (Sinclair, 1981). However, as Figure 3 shows, the two-dimensional spatial model can capture this phenomenon to an extent.

formants on the basis of shared attitudes or party are quite likely to be close to such informants in the basic space.

The spatial model will not capture other types of cues—for example, voting with the administration, voting with the committee chairman or ranking minority member, voting with the state party delegation when three-fourths of it vote together—as well as it should capture the policy-related cues. A theory of voting that combined the spatial model and the "non-spatial" cues could very well achieve "predictive" rates in excess of 90% and could satisfy the criteria laid down by Weisberg (1978) for an organizing theory of roll-call voting. A proposal along these lines has been made by Daniels (1983).

The Relationship of the Evaluative Dimensions to Various Issue Areas

Table 6 demonstrates that congressional voting in the aggregate is highly consistent with a uni-dimensional spatial model. However, the percentages in Table 6 are averages; they disguise differences between issue areas. For example, Table 5 suggests that voting on strategic arms limitation is highly ideological. In contrast, as we will show momentarily, voting on agricultural issues is not very consistent with the spatial model.

In order to determine what issues are most closely associated with the liberal-conservative evaluative dimension (we will turn to the party unity dimension momentarily), we sorted all the nonunanimous (minority not less than 10%) votes in the 1959-1978 period by how consistent they were with the dimension and by the five general policy categories identified by Clausen (1973). To measure the degree of fit of a roll call to the dimension, we used the majority lambda for the vote rather than the percentage predicted correctly. For example, suppose the vote is 281 to 149 and the cutting point on the dimension is placed such that 379 or 88.1% are accounted for correctly. The majority lambda or proportional reduction in error is: $PRE = (379 - 281) / (430 - 281) = .658$. This PRE measure allows us to disregard vote margins in our analysis. The best-fitting votes were designated as those with PRE values of .70 or higher. The results are shown in Table 8.

The votes that are most closely associated with the dimension are drawn primarily from the government management and social welfare policy areas. These two highly related policy areas can be uniquely associated with the policies developed during the New Deal realignment (cf. Clausen, 1973, p. 47; Clausen & Cheney, 1970, p. 141). On average, 69% of the best-fitting roll calls in the Senate and 72.4% of the roll calls in the House

Table 7. Nonunanimous Votes Chosen by Interest Groups for the 96th Congress

		% Cast Spatial		% Cast		Two Party Lambda		Three Party Lambda	
		1	2	Two Party	Three Party	1	2	1	2
		Dimension	Dimension	Party	Party	Dimension	Dimension	Dimension	Dimension
HOUSE									
Chosen by:									
Interest groups		312	84.3	85.8	78.9	80.2	.26	.33	.21
5 or more interest groups		40	84.4	85.9	78.7	79.9	.27	.34	.27
Not chosen by interest groups		659	83.0	84.7	78.0	79.7	.23	.31	.16
All		971	83.3	85.0	78.2	79.2	.23	.31	.17
SENATE									
Chosen by:									
Interest groups		332	81.3	84.5	74.5	76.3	.27	.41	.21
5 or more interest groups		28	81.3	84.3	74.9	76.3	.26	.38	.21
Not chosen by interest groups		556	81.7	84.9	75.9	77.7	.24	.37	.18
All		888	81.6	84.8	75.5	77.3	.25	.38	.19

Table 8. Distribution of Best-fitting Votes (%)^a

Policy Area	Congress											Total
	86	87	88	89	90	91	92	93	94	95		
Senate												
Government management	23.1	18.6	21.6	11.0	14.3	6.9	19.1	35.1	51.4	53.4	29.9	
Social welfare	74.4	59.3	59.5	76.9	53.6	43.1	18.1	29.7	17.4	25.0	39.1	
Agricultural policy	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	
Civil liberties	0.0	1.7	8.1	7.7	25.0	12.1	14.9	15.3	10.4	8.0	10.4	
Foreign policy	0.0	20.3	5.4	3.3	3.6	29.3	43.6	18.0	10.4	12.5	16.3	
Miscellaneous	2.6	0.0	5.4	1.1	3.6	8.6	4.3	1.8	10.4	1.1	4.3	
N	39	59	37	91	28	58	94	111	144	88	749	
House												
Government management	47.1	41.2	33.3	12.3	40.0	16.7	9.8	33.3	66.7	39.6	35.3	
Social welfare	41.2	47.1	25.9	47.7	31.4	50.0	51.2	40.0	21.8	32.1	37.1	
Agricultural policy	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.2	
Civil liberties	11.8	0.0	0.0	10.7	0.0	20.8	12.2	6.7	2.6	11.3	7.5	
Foreign policy	0.0	5.9	33.3	12.3	17.1	4.2	22.0	11.1	2.6	15.1	12.2	
Miscellaneous	0.0	5.9	7.4	16.9	11.4	8.3	4.9	8.9	5.1	1.9	7.7	
N	17	17	27	65	35	24	41	45	78	53	402	

^aPRE > .70.

fall into the government-management and social-welfare categories. The lowest percentage in the House is 59.2% for the 88th Congress, whereas the highest is 88.5% in the 94th Congress. The comparable figures in the Senate are 37.2% for the 92nd Congress and 97.5% in the 86th. All of the House figures and nine of the ten Senate figures are above 50%. The dimension is clearly associated with the controversies surrounding government intervention.

An examination of the content of the two categories reinforces this conclusion. The content of the social welfare domain changes very little over the ten Congresses. The best-fitting votes drawn from the social welfare area in the 86th Congress involved labor relations, public housing, urban renewal, minimum wage legislation, food stamps, aid to education, unemployment compensation, social security, and medical care for the elderly. The votes in the 95th Congress concerned the minimum wage, the Department of Housing and Urban Development appropriations, labor law reform, food stamps, the funding of the Legal Services Corporation, social security, antirecession assistance, appropriations for the Comprehensive Education and Training Act, and unemployment compensation. Given this consistency of content across time, the stability of voting position registered by continuing members that we discussed earlier is not surprising.

The same cannot be said for the government management domain—it shows a dramatic shift in content. The best-fitting votes drawn from this area in the 86th Congress focused on area redevelopment, interest rates on savings bonds, the public debt ceiling, tax reform, and airport construction. By contrast, the 95th Congress was concerned with gas and oil price deregulation, the activities of the Occupational Safety and Health Administration, federal campaign spending, budget balancing, mine safety, community reinvestment, pollution control, strip mining, and general energy policy. Yet, the relative voting positions of the continuing members remained very stable. Clearly, a key source of stability in the legislative system is a tendency to classify new issues in terms of old alignments.¹⁶

¹⁶Although the positions of legislators vis-à-vis each other remained relatively stable over time—which implies that new issues get redefined in terms of old alignments—we cannot tell from our technique whether or not changes in the substantive content of the liberal-conservative dimension have produced a uniform shift to the left/right by all legislators and interest groups over time. In any case, even if such a shift occurred, we think that it is of lesser significance than our finding of the power of the existing liberal-conservative alignment to absorb new issues.

The best-fitting foreign policy votes dealt with controversial treaties, Vietnam, the procurement of weapon systems, aid to both communist and right-wing regimes, and foreign aid under Democratic presidents. Civil liberties votes show a temporal as well as a content change. The best-fitting votes early in the study period involve anti-communism and criminal justice. By the end of the period, civil rights moves into the best-fitting category. Finally, conspicuous by their absence are agricultural policy votes. Only one vote in the House fit the liberal-conservative dimension at a PRE level of .7.

Given the results shown in Table 6, it is not surprising that the addition of the second dimension does not greatly increase the consistency of the votes in the various issue areas with the spatial model. There is, however, one interesting exception—agriculture.

Table 9 lists in the same format as Tables 7 and 8 several representative (in terms of their consistency with the spatial model) issue areas drawn from voting in the House from 1969 to 1980. As we indicated above, agricultural votes are not very consistent with the liberal-conservative dimension. However, the addition of the second dimension has a significant impact, no doubt because a coalition of midwestern Democrats, most southern Democrats, and midwestern Republicans passed much of the agricultural legislation in the 1970s. The spatial structure shown in Figure 3 would capture this to an extent. Even so, the two-dimensional spatial model does not really do much better than the three-party model.

In contrast to agriculture, food stamps are almost purely a liberal-conservative issue. About 90% of the voting is consistent with the dimension. The addition of the second dimension has almost no effect. Finally, voting on the national defense budget and on busing are typical of many other issue areas. In line with Table 6, the second dimension adds about 3% to the fits. Voting on the defense budget is more consistent with the party models than is busing, but the spatial model does very well in both issue areas.¹⁷

¹⁷Our results are largely consistent with a recent Senate voting study by Smith (1981), who found that, using Clausen's categories, voting in the governmental management and social welfare areas were very ideological from the 86th Congress onward. Civil liberties voting was very ideological after the 90th Congress, and defense and foreign policy voting were moderately ideological after the 91st Congress. The policy area that was decidedly not ideological was agriculture. He concludes that "the similarity of voting alignments did increase during the period, and, as Schneider suggested, the ideological patterning of policy positions became more visible in several policy areas by the 1970s. Simply put,

Table 9. Consistency of Issue Areas with Spatial and Party Models of the House of Representatives

		Spatial Model		Party Model		Two-Party Lambda		Three-Party Lambda	
Congress	Number Roll Calls	1 Dim	2 Dim	Two	Three	1 Dim	2 Dim	1 Dim	2 Dim
Agriculture									
91	10	67.7	76.8	63.5	69.8	.11	.36	-.07	.23
92	18	72.3	80.9	74.1	78.2	-.07	.26	-.27	.12
93	34	71.3	80.7	73.2	76.6	-.07	.28	-.23	.17
94	25	75.5	78.6	72.9	76.0	.09	.21	-.02	.11
95	41	73.2	77.4	69.9	72.5	.11	.25	.02	.18
96	17	77.7	79.9	73.5	75.8	.16	.24	.08	.17
Food Stamps									
91	3	90.6	91.3	76.3	84.7	.61	.64	.39	.43
92	2	91.2	91.3	78.7	82.5	.59	.59	.50	.50
93	8	87.9	89.4	79.0	81.8	.43	.49	.34	.42
94	6	87.9	88.9	79.7	81.9	.40	.45	.33	.39
95	12	85.9	87.0	78.8	80.9	.33	.39	.26	.32
96	16	90.5	91.4	82.5	82.9	.46	.51	.44	.50
Defense Budget									
91	19	91.3	93.0	80.5	85.1	.56	.64	.42	.53
92	37	88.9	90.4	75.3	80.0	.55	.61	.45	.52
93	63	83.2	85.1	73.9	77.3	.36	.43	.26	.34
94	68	81.7	84.4	73.7	77.2	.30	.41	.20	.32
95	69	83.7	85.7	75.5	78.8	.33	.42	.23	.33
96	59	83.7	86.0	75.2	78.3	.34	.43	.25	.35
Busing-Desegregation									
91	8	89.2	91.2	69.4	78.0	.65	.71	.51	.60
92	18	81.4	83.9	66.5	75.6	.44	.52	.24	.34
93	17	81.4	84.1	65.9	74.1	.46	.53	.28	.39
94	4	79.2	82.2	65.7	71.6	.39	.48	.27	.37
95	6	82.2	83.1	65.4	73.1	.49	.51	.34	.37
96	12	85.2	86.0	72.7	75.6	.46	.49	.39	.43

Conclusion

Our empirical analysis has shown that the liberal-conservative and party evaluative dimensions used by the interest groups to construct their ratings are consistent with much of the roll-call voting in Congress. On average, the liberal-conservative dimension explains 81% of the variance of the ratings and in conjunction with a simple two-outcome spatial theory of voting, successfully accounts for 86.9% of the voting in the House and 85.4% of the voting in the Senate during the period of our study. The addition of the party dimension adds 1.5% and 2.3% respectively to these figures.

senators' policy positions relative to each other were more stable across policy areas in the 1970s, and that stability was paralleled by a greater match between those policy positions and the standards of evaluation constructed by contemporary ideologue" (p. 794).

The method of roll-call analysis that we used in our empirical analysis has many advantages. The most important advantage is that recovering the spatial configuration of legislators from a multi-dimensional unfolding of a 535-by-30 matrix of ratings (535 legislators rated by 30 interest groups—e.g., 1977) is much easier than recovering a configuration from a factor analysis of a 100-by-100 or a 435-by-435 matrix of associations. Furthermore, the spatial model of the ratings results in the placement of the members of the House and Senate in a common configuration. We checked the model by separately unfolding the House and Senate ratings for each year. In only two instances were the configurations different enough to cast doubt on the assumption of comparability. Although we have not exploited it here, having the House and Senate members in the same spatial configuration makes cross-chamber comparisons of roll-call voting within and between issue domains possible.

Comparisons across time are also possible with this methodology. As we demonstrated above, the

liberal-conservative dimension is very stable over time. However, if the interest groups and the legislators uniformly drifted to the left (or right) over time, this would not be uncovered by the unfolding and matrix-fitting procedures.

Our methodology has the further advantage that it does not require a researcher to make any judgments about what roll calls to include in the analysis (e.g., in the computation of the association measures for a factor analysis) and the item directions associated with roll-calls. In effect, the interest groups do it for the researcher. Because the spatial configuration of legislators is what the interest groups "see" in their evaluative space, the roll calls can be fitted to the configuration and can be compared with one another in terms of the evaluative dimensions of the interest groups. This allows a researcher to sort the roll calls not only by issue area but also by how well they are explained by the perceptual space of the interest groups. Voting on strategic arms, for example, is highly ideological.

Although our evidence suggests that the interest groups select a representative sample of roll calls to construct their ratings, the instability of the second dimension recovered from the ratings points to an inherent limitation of their usefulness in spatial analysis. The interest groups are not dispersed enough over the second dimension for it to be recovered as accurately as the first dimension. This has been a less serious problem in recent years when the number of interest groups issuing ratings has been large. Nevertheless, the estimation of the second dimension must be improved.

The "predictive" success of the spatial model is about the same as the voting model of Matthews and Stimson (1975). We think that the spatial model largely captures party and policy-related cue-taking, which suggests that a voting model that incorporated spatial and nonspatial cue-taking could account for more than 90% of Congressional voting. We hope that what we have done here will help to stimulate the development of such new voting theory.

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PACs, Contributions, and Roll Calls: An Organizational Perspective

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This study uses information about the organizational arrangements of five major political action committees to develop an explanation for the extent to which PAC contributions are capable of influencing congressional voting decisions. The explanation claims that the processes by which PACs raise and allocate money must be understood before the impact of money on roll call decisions can be appreciated. In contrast to some previous studies, this analysis demonstrates with marked clarity the limited nature of PAC influence.

Concern about the effects of campaign contributions on congressional voting has stimulated a growing collection of studies on contributions and roll calls. Scholarly interest in this issue is a natural outgrowth of representatives' increased dependence on money for electoral survival. From 1974 to 1980, political action committee (PAC) expenditures increased by 500%, and average total contributions to congressional candidates tripled (Jacobson, 1983). During just the 1981-1982 election cycle, 2665 nonparty PACs contributed over \$87 million to federal candidates (Malbin & Skladony, 1984).

Because of the prominent role of money in congressional elections, a relationship between PAC contributions and congressional voting is often claimed.¹ The validity of such assertions, however, has been difficult to establish. More often than not, these claims have not been confirmed through rigorous empirical analysis, and even when they have been, researchers have failed to identify any theoretical forces that might con-

strain or limit PAC influence. Unfortunately, because such theoretical considerations are absent, our understanding about PACs and their ability to influence roll calls is extremely tenuous.

In the research reported here, I begin to examine organizational arrangements in the PACs of five national associations that affect the ability of these PACs to influence roll call votes. Although organizational arrangements are thought to be important considerations in understanding interest groups generally (e.g., Berry, 1977; Truman, 1951; Wilson, 1973), the consequences of organizational arrangements in PACs have not been explored.² As a result, an important constraint on their activities and influence has been overlooked.

My objective in this article is to point out a fundamental paradox about an organizational arrangement common to many political action committees. The contradiction is that the organizational arrangement *most* conducive to raising money from individual contributors is also the organizational arrangement *least* conducive to influencing congressional voting. Paradoxically, the factors that allow some PACs to become very rich are the very same factors that undercut their potential influence. As a consequence, organizational arrangements are capable of limiting the ability of PACs to influence roll calls through money. This argument is assessed in light of empirical evidence drawn from a qualitative study of PAC organizations and a quantitative analysis of contribution patterns and roll calls.³

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¹Representative Downey, Democrat of New York, for example, claims, "You can't buy a congressman for \$5000. But you can buy his vote. It's done on a regular basis" (Isaacson, 1982, p. 20). Barber Conable, Republican of New York, suggests that "These new PACs not only buy incumbents, but affect legislation as well. It's the same crummy business as judges putting the arm on lawyers who appear before them to finance their next campaign" (Green, 1982, p. 18). Additional support for these positions can be found in Drew (1982).

²Organizational arrangements have not been ignored altogether (e.g., Eismeier & Pollock, 1984; Sabato, 1984; Sorauf, 1982), but the importance of organization for congressional decision making has not received attention.

³Naturally, roll call votes are only a rough indicator of the overall influence that contributions might have

PAC Contributions and Roll Calls

Empirical evidence about the influence of PAC contributions on congressional voting is filled with ambiguity and apparent contradiction. On the positive side, Silberman and Durden (1976) found that an increase in total labor contributions in 1972 led to a significant increase in congressmen's support for the minimum wage in the 93rd Congress. Chappell (1982), too, discovered a significant positive connection between contributions and votes: a connection between contributions from Rockwell International and votes on the B1 bomber. In a study using aggregate data, Kau and Rubin (1982) reported that total labor contributions in 1978 were significantly related to votes on such issues as the debt limit, the windfall profits tax, and wage and price controls in 1979. Frendreis and Waterman (1983) uncovered a significant relationship between contributions from the trucking industry and voting in the Senate on trucking deregulation, and Brown (1983) concluded that contributions from the National Automobile Dealers Association and the American Medical Association had "substantial" effects on votes pertaining to those two groups in the 97th Congress.

In contrast, however, Chappell's (1981) work indicates that the effects of campaign contributions from several maritime unions on votes in the 96th Congress were "unavoidably ambiguous." More conclusively, Welch (1982) has asserted that "contributions by dairy PACs were *not* a major influence" (p. 491) on votes for dairy price supports in 1975. Finally, in an analysis of the effects of PAC contributions on House votes for the Chrysler loan guarantee and the windfall profits tax, Yiannakis (1983) has concluded that the influence of money was "very limited."

This diversity in findings is not particularly surprising, because different researchers have analyzed different groups and different votes. Unfortunately, because of the excessive costs involved, a representative sample of PACs and roll calls has not been collected and analyzed, and as a result, the meanings of the existing findings are difficult to interpret.⁴ Are there really significant discrepancies in the various findings? Which findings are the aberrations—those that ascribe a positive and significant influence to PAC con-

tributions or those that do not? How substantial is a "substantial" effect? Can even larger effects be expected?

The strategy of this research is to examine just five PACs, but five PACs that appear to have considerable influence on roll call decisions. Because this sample is both so small and skewed, it will not be possible to make generalizations about the influence of all PACs, or even a "typical" PAC. Yet an analysis of some of the most powerful PACs will permit generalizations about the upper bounds of PAC influence on roll call decisions. Given the present state of knowledge, an attempt to specify the range of possible empirical results, along with an explanation of why the range is as restricted as it is, represents a clear step forward in generalizing about PACs and roll calls.

Data on Five PACs

The five PACs included in this study are: The American Medical Political Action Committee (AMPAC), affiliated with the American Medical Association; the Dealers Election Action Committee (DEAC), affiliated with the National Automobile Dealers Association; BANKPAC of the American Bankers Association; the Realtors Political Action Committee (RPAC) of the National Association of Realtors; and the Associated General Contractors Political Action Committee (AGCPAC), affiliated with the Associated General Contractors.

The focus is on the PACs of national associations because many observers think they are among the most active and successful of all PACs in influencing roll call voting. Several factors contribute to this reputation. First, the national associations are generally well known for their strong lobbying organizations, and these organizations are alleged to work closely with the PACs (Malbin, 1979).⁵ Second, the PACs of national associations tend to be among the largest PACs in terms of total dollars contributed to congressional candidates. As of 1982, RPAC was the largest of the association PACs; AMPAC ranked second; BANKPAC and DEAC ranked fourth and fifth, respectively; and AGCPAC was seventh largest among the association PACs. Moreover, RPAC, AMPAC, and DEAC were among the ten largest PACs, regardless of type (Cohen, 1983). If

on legislative outcomes. Contributions may have considerable effects on other aspects of the legislative process, such as decisions to report bills out of committee, or strategic considerations in scheduling.

⁴Although Sabato (1984) has surveyed a random sample of 399 PACs, his data do not include roll call votes.

⁵Even though many of the labor unions also have strong lobbying organizations, the labor PACs appear to be more interested in using their campaign contributions to influence election outcomes than the voting decisions of incumbent members of Congress (e.g., Budde, 1980).

one assumes that congressmen will be more sensitive to large contributions than small ones, then large PACs are more likely to affect voting decisions.

A third reason for the strong reputation of the association PACs is the excessive publicity they have received from the news media. One of the most publicized cases of influence in the 97th Congress involved the House vote to overturn the FTC rule regulating used-car dealers. Campaign contributions from the National Automobile Dealers Association were thought to be instrumental in that vote (UPI, 1982). The alleged connection between AMPAC contributions and the House's decision to exempt professionals, physicians in particular, from FTC regulation also received attention in 1982 (Barone, 1983). Finally, campaign contributions from the American Bankers Association were thought to be a "major factor" in the effort to repeal the law requiring banks to withhold taxes on interest and dividend payments (Noble, 1983).

Last, the national associations tend to lobby a lot of bills, so that at least one important bill for each group is likely to reach the House floor for a vote in any given session. Consequently, the possibility of a connection between money and votes is greater. Of the five PACs selected for this study, the Associated General Contractors is one of the more active groups. Although smaller and less well-known than some of the other groups, the AGC has traditionally played a leading role in lobbying major pieces of legislation (Ornstein & Elder, 1978). During the 97th Congress, the AGC lobbied extensively for passage of the Surface Transportation Assistance Act. That act involved several roll calls before final passage.

In summary, all five PACs selected for study here are either large PACs, PACs that were quite active in the 97th Congress, or PACs that had reputations for influencing legislation with their contributions. These five PACs certainly are not the only ones that meet these criteria; evidence of the effects of campaign contributions on voting might be found by studying other PACs. But if such evidence is to be found, it is likely to be found among these five.

In relation to the entire universe of PACs, the five PACs of this study fall under the rubric of "connected" PACs. A connected PAC, stated simply, is one that is affiliated with, and which coexists with, some extant organization, and this extant organization is commonly called the PAC's "parent" (Sorauf, 1982). Typical parents are trade and professional associations, membership organizations (such as the National Rifle Association), corporations, and labor unions. The vast majority of all nonparty PACs—more than 80%—are connected PACs. In contrast, un-

nected PACs have no sponsoring organizations. Examples are the National Conservative Political Action Committee; National Committee for an Effective Congress, and Life Amendment PAC. Unconnected PACs generally, but not always, fall under the rubric of "ideological" PACs, and ideological PACs are generally, but not always, unconnected PACs (Latus, 1984).

Two types of data were collected for each of the five PACs. One type is from open-ended personal interviews; the other is quantitative data on actual contributions and roll calls. The interview data were collected in order to help characterize the organizational structures of the PACs and to provide first-hand information about fundraising practices and allocation strategies.⁶

Fourteen officials were formally interviewed from the five PACs. Most of those interviewed were located in Washington, but several were in other offices of their committees (e.g., at the regional and state levels). In four of the five, the principal interview was conducted with the executive director, or the official in charge of the day-to-day operations of the PAC. In the fifth, the principal interview was conducted with a consultant to the PAC who was instrumental in its founding. For four of the PACs, lower-level Washington officials were interviewed in addition to the executive director. These formal interviews were then supplemented with a number of informal discussions with various officials in the PAC community in Washington.

The quantitative data on campaign contributions were provided by the Federal Election Commission. The data set includes all nonindependent contributions to all House candidates for the 1979-1980 and 1981-1982 election cycles. Excluded are candidates who received money but subsequently were defeated in a primary, retired, died, resigned, or ran for some other office. These data were combined with data on roll calls in the U.S. House of Representatives from the first and second sessions of the 97th Congress.

Organization, Fundraising, and Allocation Strategies

Students of political action committees and campaign finance have theorized that PAC officials use one of two basic strategies when attempting to influence congressional outcomes through campaign contributions (Welch, 1980). Under one strategy (call it Type I), PACs attempt to influence elections. This strategy gives priority

⁶For a fuller discussion of both types of data, see Wright (1983, pp. 13-21).

to candidates in the closest contests, and the objective is to keep representatives in office, or to place new candidates in office, who by nature are most sympathetic to the PAC's policy goals. Under the other strategy (Type II), PACs attempt to influence the behavior of incumbent representatives. Theoretically, this strategy gives priority to candidates who are likely to win, regardless of their initial policy positions, and to incumbents who are influential within the House. The objective is to persuade influential congressmen who are not always in agreement with the PAC's policy goals to become more agreeable and to maintain the support of already agreeable congressmen.

Although the goal of each strategy is to influence legislation, the means of accomplishing this goal and the assumptions underlying each strategy are quite different. Under the first strategy, congressmen's policy positions are assumed to be known, but fixed. As a result, voting decisions are not thought to be susceptible to manipulation or persuasion through campaign contributions, and thus the emphasis is on altering the membership of Congress rather than altering members' policy positions.

Under the second strategy, however, congressmen's policy positions are assumed to be variable, and congressmen are thought to be willing to exchange votes for campaign contributions.⁷ PACs, therefore, pursue policy objectives by contributing to likely winners, who presumably will have flexibility in their policy positions, and to influential incumbents, who presumably are most capable of affecting the fate of proposed legislation.⁸

Both strategies assume that candidates need money, but the source of the need is different for each strategy. The Type I strategy presumes that the basis for financial need is immediate electoral competition, that is, a very close race. Implicit in the Type II strategy, however, is the notion that need derives from other considerations. Among these are the desire to establish or maintain a large war chest in order to deter future challenges, the desire to win by an exceptionally large margin in

order to demonstrate viability for an attempt at a senatorial or gubernatorial seat, and the desire to make contributions to one's colleagues in order to solidify or expand one's influence within the House.⁹

Whether a PAC uses a Type I or Type II allocation strategy depends, in part, on the organizational resources available to the PAC. Theoretically, only those PACs that have the capacity to link contributions to congressional action can effectively employ a Type II strategy. Because Washington lobbyists are essential in establishing these links, a Type II strategy is much more viable for PACs associated with professional lobbying organizations—that is, connected PACs—than for unconnected, or ideological, PACs. In fact, research by Welch (1978, 1980) indicates convincingly that ideological PACs are much more likely to use a Type I than a Type II allocation strategy.

Because the PACs of national associations are affiliated with established lobbying organizations, contributions and lobbyists are often thought to be inextricably linked in the association PACs. Malbin (1979), for example, has suggested that the PACs of national associations are "special interest" rather than "general interest" groups. The hallmark of special interest groups is that they allocate their money "defensively," being concerned more with influencing legislative behavior than with influencing electoral outcomes. One reason the PACs of associations behave this way, says Malbin, is that national associations "tend to be headquartered in Washington, where lobbyists and PAC staffs work closely together" (p. 33). Similar views have been expressed by Fred Wertheimer, President of Common Cause, who states flatly that "PAC money is tied to lobbying" (Hucker, 1978), and by Germond and Witcover (1983) who claim that, "It's left to a few professional lobbyists to steer that [PAC] money in the 'right' direction."

A popular belief about PAC contributions (particularly association PACs), then, is that they are controlled, or at least shaped to a considerable degree, by Washington lobbyists. Surprisingly, however, the analysis of decision-making patterns within the five PACs of this study yielded *very little support for this position*. Instead of allocation decisions being controlled by Washington lobbyists, the evidence suggests overwhelmingly that PACs' allocations are dominated by local inputs—recommendations of active members of the PACs at the state, congressional district, and

⁷Congressmen are predicted to alter their votes, however, only up to the point where the gain in electoral support through additional contributions just offsets the losses in support that result from moving away from voters' preferred policy positions. For a mathematical formulation, see Ben-Zion and Eytan (1974).

⁸In reality, PACs probably pursue some mix between the two strategies. Unfortunately, though, attempts to model mathematically behavior motivated by both approaches have been unsuccessful (Welch, 1980, p. 101). The two pure strategies, then, should be regarded as heuristic tools, not as perfect depictions of the real world.

⁹These three rationalizations for money were described in the interviews with the PAC officials.

county levels.¹⁰ Interviews with PAC officials in Washington indicated that when local recommendations are made concerning the amount of money that the national office should allocate to different federal candidates, some *80 to 90% of these recommendations are approved without modification*. Within AMPAC, for example, 80% of all local recommendations are approved. For RPAC, the approval rate is 92%; and for DEAC, 95 to 98%.

Furthermore, there is very little evidence that PAC staffs and lobbying staffs work closely together. Historically, lobbying has been an activity of the parent organizations, not the PACs; and thus the two staffs have developed separately. Of the five PACs studied here, only in the case of the AGC does a single individual oversee both the lobbying activities and the PAC activities. Even physical distance often separates the PAC officials from the lobbyists. The NADA maintains an office for its lobbyists in downtown Washington, but the PAC operations are located in the organization's headquarters in McLean, Virginia. The headquarters for AMPAC have been located in Washington only since 1982; before 1982, the AMPAC offices were located in Chicago.

So, in light of the conventional view about PAC contributions and lobbyists, these findings are indeed surprising, and, as with most unexpected results, they raise important questions. Why is it that allocation decisions are dominated so strongly by local officials rather than Washington lobbyists? Who are these individuals at the local levels whose recommendations carry so much weight? Answers to both of these questions require a short digression into the process by which political action committees raise money for distribution to political candidates.

Organization and Fundraising

A continual and pressing problem for political action committees, as for any interest group, is the establishment and maintenance of a large contingent of active, dues-paying members (e.g., O'Brien, 1975; Olson, 1965/1971). Under federal law, corporations, labor unions, and other membership organizations are allowed to draw on their general treasury funds (e.g., funds from the parent organization, such as the AMA) to raise money for their PACs, but they are prohibited from drawing on their general treasury to make

campaign contributions to federal candidates. National associations, as well as other organizations, must establish a "separate, segregated fund" (i.e., a political action committee) and make campaign contributions from this separate fund rather than from the general treasury. As a result, a separate membership must be established for the PAC, and this membership is itself a subset of the membership of the parent organization.

The fact that the PAC draws its members from the parent association puts the PAC at a severe disadvantage, relative to the parent, in providing selective material incentives to attract contributors or members. Members of the parent association can enjoy all of the selective incentives provided by the parent without ever contributing a dime to the political action committee, and because PACs are nascent political organizations, generally having been formed some 50 or 60 years after their parents, they lag far behind their parents in the provision of material benefits to prospective members. The American Medical Association, for example, was founded in 1846, but AMPAC was not added until 1961; the National Automobile Dealers Association was founded in 1917, but DEAC was not established until 1975; and the National Association of Realtors was formed in 1908, but RPAC was not founded until 1969. The parent associations, therefore, have had a great deal of "lead time" in testing and adopting those incentives that are most efficient for encouraging membership. In effect, then, the parents have established a virtual monopoly on material incentives, posing a formidable problem for the PACs in their fundraising efforts.

The way the PACs have managed to overcome this obstacle is to take advantage of their basic organizational structures. A common characteristic of the five PACs of this study is that the organizations with which they are affiliated are federated, or decentralized, organizations. Because the PACs were formed so much later than their parents, the organizational lines of the PACs tend to run parallel to the organizational lines of their parent groups.¹¹ Under the aegis of each of the five national PACs of this study are state-level PACs which, in turn, comprise officials from congressional districts, counties, and, where appropriate, individual corporations.¹²

The federal, or decentralized, structure of the

¹¹For a more detailed description of organizational arrangements, see Wright (1983, pp. 50-56).

¹²Member banks of the ABA generally, but not always, have their own political action committees. However, individual corporations of the AGC generally do not have their own PACs, although there are exceptions.

¹⁰Handler and Mulkern (1982) make a comparable finding with respect to allocation decisions of corporate PACs. In 14 of 70 PACs surveyed, allocation decisions were said to be made by "nonspecialist amateurs."

association PAC provides an extensive grassroots network through which contributions can be raised. Each national PAC has, in effect, a dedicated core of local activists who bear much of the responsibility for soliciting individual contributions. These local activists are neither professional lobbyists nor full-time, salaried officials of the PAC, but rather political amateurs, such as professional bankers, physicians, and realtors, who happen to have a strong interest in politics and who take an active role in the local PAC organization.

Because these activists operate at the local level through small constituent groups, solidary incentives (e.g., Salisbury, 1969) can be used effectively to stimulate membership. Instead of offering selective material incentives (e.g., Olson, 1965/1971), the basic approach to fundraising is to make persuasive appeals to an individual's sense of *citizen or professional* duty—the opportunity and obligation to join with other members of one's profession in order to create and sustain a political environment that is favorable to the maintenance and growth of the profession or industry. One PAC official characterized this approach as “an appeal to professional responsibility,” and he stated that “it's how you market that approach that determines how successful you are going to be.”

Persuasive appeals are made in a variety of ways: through direct mailings, presentations and speeches at small gatherings of staff and employees, phone calls, and personal visits. Because persuasive appeals of this sort are most effective when made in person, or in small group settings, the basic fundraising approach of the association PACs is ideally suited to a decentralized organizational structure. In fact, it is doubtful whether such an approach could be implemented successfully in any arrangement other than a decentralized one.

But in addition to persuasive appeals, the PACs also emphasize the effectiveness and legitimacy of contributing money to the PAC. PACs, it is argued, can and do influence election outcomes, and PACs frequently publish figures that emphasize the percentage of winning candidates they have supported. PAC officials also are quite sensitive to the perceived legitimacy of PACs, and they devote considerable effort—through speeches, cooperation with the news media, written editorials, and similar means—to stimulate contributions by portraying PACs in a positive light.¹³

The notion that large groups can be organized

on the basis of factors other than selective material incentives is certainly consistent with the theoretical work of other scholars. Even Olson (1965/1971) has pointed out that federal groups may be able to bring about collective action through social incentives in the small constituent groups, and Moe (1980) has argued that

[It] would be a serious mistake to “understand” organizational politics by automatically pointing to whatever organizational structures exist for the production and distribution of economic selective incentives. Organizations can do much more than this to attract members, and attention must clearly be given to other means of generating support: e.g., the ideological appeals of leaders; peer and leader emphasis on responsibility or fairness; the structuring of meetings, committees or social activities; the organizational generation of social pressure; leader efforts to convince individuals that their contributions make a difference; and the role of newsletters and informal communications networks in shaping member perceptions and values.

Implications for Allocation Strategies

The great dependence on local activists of the PAC for raising money significantly enhances their influence over decisions about how to allocate money to political candidates. National officials of the PAC are acutely aware of the need to respect local recommendations when allocation decisions are made. To do otherwise would reduce the incentives of local officials to raise money. In an outline of guidelines for establishing an effective state PAC organization, one national PAC makes the following suggestion.

Although [statewide] contribution decisions must finally be made by the state PAC, proper recognition of the role of leaders of local PAC support groups and others should be given. *This is the key to a harmonious PAC program, and an important stimulus to membership development.* (emphasis added)

The national executive director of another PAC explained that:

If we have someone who raised 10,000 dollars at the local level, but wants to support someone who we don't think is especially deserving, we will usually go along with him. If we didn't, he might not raise that kind of money for us the next time around.

And an official from still a different PAC summed it up this way: “They [the local officials] raise the money; they ought to be the ones who spend it.”

¹³Additional discussion of the fundraising methods of the association PACs can be found in Wright (1984).

These observations suggest that, contrary to conventional wisdom, the best allocation strategy for the association PACs is a Type I rather than a Type II strategy. The dominance of local inputs emphasizes electoral considerations rather than lobbying considerations, because local PAC officials are much more likely to be familiar with electoral politics of geographic constituencies than with the mechanics of influencing legislation in the Congress. In addition, because allocation decisions are shaped largely by local PAC officials, congressmen who desire campaign contributions are better advised to cultivate favorable relationships with prominent local PAC officials than with Washington lobbyists. This arrangement tends to undercut substantially the value of money as a bargaining tool for professional lobbyists, thereby undermining the effectiveness of a Type II allocation strategy.

Contribution Patterns in Five PACs: Empirical Analysis

The fact that allocation decisions are dominated by local officials leads to testable propositions about allocation strategies and the impact of campaign contributions on congressmen's voting decisions. Specifically, the weak influence of Washington lobbyists relative to the strong influence of local PAC officials suggests that PACs' campaign contributions will be used to elect ideologically sympathetic candidates in close races rather than alter the policy positions of not-so-sympathetic candidates in safe races. As a consequence, campaign contributions are not likely to carry substantial weight in congressmen's roll call voting decisions. These two propositions are examined in this section and the next with the use of data on campaign contributions during the 1979-1980 and 1981-1982 election cycles, along with roll calls from the House during the 97th Congress.

In the analysis that follows, inferences about PACs' allocation strategies are made from information about the characteristics of the candidates to whom the PACs contribute. Under a Type I strategy, as defined previously, priority for contributions is given to congressmen who have the "correct" ideology and to those who face tough electoral competition. Under a Type II strategy, priority for contributions is given to likely winners who are influential within Congress. Theoretically, then, these three factors—ideology, institutional influence, and electoral security—can be used to distinguish between allocation strategies.

The measurement of institutional influence is accomplished through a cluster of variables indicating committee assignments and leadership positions within the party. The relationship be-

tween this cluster of variables and a Type II allocation strategy has been recognized by Green (1979).

Special interest investors, who would not dream of pouring their money into dud stocks, are equally careful when it comes to choosing their legislative portfolios. A freshman legislator with a seat on a dull committee won't cost much, but won't yield much return either. The logical result is that the money goes to the men who rule Congress—the members of key committees, the party leaders of each of the houses, and the committee chairmen (pp. 10-11).

The committee variables included in the present analysis are: AGRICULTURE, APPROPRIATIONS, BANKING, COMMERCE, JUDICIARY, and WAYS AND MEANS. These six committee variables were selected on the basis of the interviews with the PAC officials, who were asked explicitly whether or not members of certain committees were targeted routinely for contributions. Officials of BANKPAC mentioned Banking, Ways and Means, Agriculture, Commerce, Judiciary, and Appropriations, in that order of priority. An official of the realtors' PAC indicated that Ways and Means is sometimes important, and Commerce was mentioned by an official of DEAC. Neither AMPAC nor AGCPAC claimed to give special consideration to members of any particular committee. In addition to membership on these committees, institutional influence is also measured by whether or not a representative is a party leader (REP LEADER, DEM LEADER) or a committee or subcommittee chairman (CHAIRMAN). Complete definitions of these and other variables are given in the Appendix.

The ideology factor is operationalized through another cluster of variables—the congressman's party affiliation (PARTY) and his score on the Americans for Democratic Action index of voting in 1979 (ADA79). And the electoral competition factor is measured by the closeness of the candidate's race and the electoral experience of his or her challenger (NEED).¹⁴ In addition, the NEED variable involves the interaction between ideology and competition, because ideology should be a more important factor under a Type I strategy when competition is strong.

¹⁴A congressman was determined to have had strong competition if he or she received less than 60% of the vote in a two-way race in 1978, or if the challenger in 1980 had held any elective office before the 1980 congressional election. Information on the experience of challengers was taken from "The 1980 Elections," Congressional Quarterly *Weekly Report*, Supplement, October 11, 1980, pp. 2965-3092.

The importance of ideology, institutional influence, and electoral security for predicting allocation patterns should vary depending on whether a PAC uses a Type I or a Type II allocation strategy. Under a Type I strategy, larger campaign contributions should be associated with greater electoral need and more conservative ADA scores. If a Type I strategy is dominant, the coefficients for PARTY and NEED should be large and positive, and the coefficient for ADA79 should be large and negative (because high ADA79 scores denote liberalism). Also, the coefficients for the institutional influence variables should all be negative. Under a Type II strategy, larger contributions should be associated with institutional influence, less conservative ideological positions, and less competitive races. Although the coefficient for ADA79 will not necessarily be positive, it should be small if a Type II strategy is dominant. Similarly, the NEED coefficient should be small, but not necessarily negative, and all of the coefficients for institutional influence should be large and positive.

These hypotheses were tested by regressing the total contributions (minus independent expenditures) in actual dollars for each of the five PACs during the 1979-1980 election cycle on all twelve of the explanatory variables. The results of the regression analyses appear in Table 1.

Generally, the signs of the coefficients in Table 1 are more consistent for the ideology and need variables than for the committee and leadership variables. The sign of the NEED coefficient is always positive, indicating that all five PACs gave more money to candidates who generally agreed with them ideologically and who faced tough elec-

tion fights. Similarly, the sign of the ADA coefficient is always negative, which indicates a clear preference among all five PACs for conservative candidates. Also, with the exception of DEAC, all five PACs gave more money to Republican than to Democratic candidates. The coefficients for the committee and leadership variables, however, do not exhibit such clear patterns. As expected, BANKPAC favored members of the Banking and Ways and Means Committees, and DEAC clearly favored members of the Commerce Committee. But otherwise, there is little clear evidence that committee and leadership positions elicited larger contributions.¹⁵

Overall, then, the results of the regression analyses tend to provide greater support for a Type I allocation strategy than for a Type II strategy. The general pattern is for larger contributions to go to sympathetic candidates who need money rather than to candidates who are on influential committees or in positions of party

¹⁵Tests of statistical significance have been omitted here because there is no reason to believe that they would be meaningful. Significance tests are meaningful only when observations have been selected randomly from a larger population or when it can be assumed that the universe from which they have been selected is normally distributed. The universe of interest here is all Congresses from 1974 to 1980, because 1974 marked the first year that the FEC required full disclosure of campaign expenditures. But the observations of this study have not been selected randomly from this universe, and the Congresses since 1974 are certainly not normally distributed with respect to relevant variables, because they have all been controlled by Democrats.

Table 1. Regression Results for Contributions to Incumbents

Variable	AGCPAC	AMPAC	BANKPAC	DEAC	RPAC
Intercept	245.1	2354.1	864.5	2163.1	3222.8
AGRICULTURE	-51.2	-118.9	409.5	-148.8	555.8
APPROPRIATIONS	-154.0	-365.3	17.2	12.5	-624.2
BANKING	-109.6	-405.8	1281.4	200.8	589.4
COMMERCE	-139.4	172.1	157.8	977.6	-40.7
JUDICIARY	-139.2	580.8	-196.2	86.0	73.7
WAYS AND MEANS	11.8	-225.8	963.6	372.1	-292.4
CHAIRMAN	70.8	-608.2	580.6	575.3	-401.8
REP LEADER	11.6	-475.5	562.4	369.6	195.2
DEM LEADER	178.8	-176.6	-102.2	631.5	-432.8
PARTY	121.5	196.7	13.9	-219.2	84.3
ADA	-2.0	-23.2	-7.9	-22.8	-33.6
NEED	423.6	1739.9	209.8	930.0	1153.0
R SQUARE	.17	.33	.23	.24	.41

N = 385

Table 2. Predicted Contributions to Different Types of Congressmen

	Congressman A (conservative, non-leader, competitive)	Congressman B (moderate/liberal, leader, safe)
AGCPAC	\$ 740	\$ 223
AMPAC	3709	0
BANKPAC	891	2981
DEAC	2304	2857
RPAC	3619	0
Mean	\$2253	\$1212

leadership. Only the coefficients for the ideology and need variables, not the influence variables, are consistently strong and in the right direction. These are precisely the results one would expect when allocation decisions are dominated by local inputs.

The results in Table 1 can be carried a step further by using the regression coefficients to make predictions of the actual dollar amounts allocated under each type of strategy. Such an exercise yields a more concrete picture of allocation patterns—a picture in terms of dollar amounts rather than OLS coefficients.

The prediction of actual dollar amounts that follows is based on a conception of two hypothetical kinds of congressmen. One kind, Congressman A, is intended to represent the ideal target for a Type I allocation; the other, Congressman B, the ideal target for a Type II allocation. Let Congressman A be a rank-and-file, conservative, Republican congressman from a competitive electoral district. Let Congressman B be a member of a strategic committee (Ways and Means and Banking for the ABA, Commerce for the auto dealers), a committee or subcommittee chairman, a Democratic House leader, a liberal, and electorally secure. Then, a PAC that uses a Type I allocation strategy should give larger contributions to Congressman A, and a PAC that uses a Type II strategy should give larger contributions to Congressman B.

The entries in Table 2 are the dollar amounts that each PAC, on the basis of its allocation strategy, is predicted to give to each kind of congressman.¹⁶ Of the five PACs, only BANKPAC

clearly places highest priority on a Type II allocation strategy.¹⁷ Even though DEAC is predicted to give slightly more money to Congressman B, the PAC is also predicted to give a substantial contribution to Congressman A. Each of the other three PACs, however, is predicted to give a substantially larger contribution to Congressman A than to a Congressman B, and on average across all five PACs, Congressman A is predicted to receive almost twice as large a campaign contribution as Congressman B. These results are quite consistent with the hypothesis that allocation decisions are a function of local discretion in the allocation process.

Campaign Contributions and Roll Call Votes

Two necessary (but not sufficient) conditions for PAC contributions to influence roll call votes are that PACs allocate money with the intent of influencing roll calls, and congressmen are aware of that intent. The evidence presented thus far, however, suggests quite clearly that neither of these conditions will be satisfied. The evidence indicates that money is given with the intent of influencing election outcomes, not specific roll calls, and that congressmen and their staffs must cultivate relationships with local PAC officials, not Washington lobbyists. As a result, a strong

¹⁶The entries under Congressman B for AMPAC and RPAC have been entered as "0" rather than the negative values (-370 and -216) that actually were predicted. The reason for the negative values is that OLS is an inappropriate functional form when the dependent variable is truncated normal—that is, when there is a lower bound (i.e., zero contributions) with a large concentration of values, but no such concentration at the upper bound. Tobit analysis (Tobin, 1958), which

provides an appropriate functional form, was also used to estimate coefficients for the independent variables. However, the results did not differ in any systematic ways from the OLS results, so the more familiar OLS results have been presented here.

¹⁷BANKPAC also has the most centralized structure of the five PACs for making allocation decisions. The Washington office sends out a computer listing to all state PAC organizations with recommended contributions to all candidates. These recommendations are based primarily on the congressman's committee assignment, and local PAC officials usually, but not always, follow these recommendations.

causal relationship between PAC contributions and roll calls is unlikely.

To assess the influence of contributions on roll call decisions in the 97th Congress, the most important—or one of the most important—roll calls for each of the five PACs was selected for analysis. Information on important roll calls was obtained through communication with PAC and association officials and through a review of the lobbying priorities of each group as reported in the *Congressional Quarterly Weekly Reports*.

The most important vote for the NADA in the 97th Congress was the vote to overturn the Federal Trade Commission's rule that required used-car dealers to inform customers of known defects in used automobiles. On May 6, 1982, the House adopted a concurrent resolution (286-133) to disapprove the FTC rule.

A crucial vote for the Associated General Contractors was a vote on the rule governing floor consideration of the Transportation Assistance Act (HR6211). A closed rule was assigned to a Ways and Means tax package to be amended to the act, and the AGC supported the closed rule from fear that an open rule might destroy all prospects of passing a highway bill in any form. The House adopted a resolution approving the closed rule on December 6, 1982, by a 197-194 vote.

Votes on the Gramm-Latta budget proposal were important to the National Association of Realtors.¹⁸ The realtors favored as deep a slowdown in the growth of spending as possible and opposed a rule governing consideration of Gramm-Latta-II that would have encouraged lower spending cuts. The rule was overturned by a 210-217 vote (HR3982) on June 25, 1981.

For the AMA, the critical vote was on an amendment to exempt professionals—physicians in particular—from FTC regulation. The AMA succeeded in getting the amendment (HR6995) passed in the House by a 245-155 vote margin in December, 1982, but the amendment subsequently failed in the Senate.

Finally, an important vote to the bankers (although not as important as in the next Congress) was a vote concerning the withholding of taxes on interest income from bank accounts. The ABA opposed a closed rule for consideration of the 1983 tax bill in hopes of amending the bill to exclude the withholding provision. On August 19,

1982, the bankers ended up on the losing side of a 220-210 vote on the rule (HR4961).

Each congressman's votes on these issues was recorded, and various predictor variables, including PAC contributions, were collected in order to examine the impact of campaign contributions on roll call votes, with other factors held constant. The full array of predictor variables fall into four general categories: constituency demographics (URBAN, BLUE COLLAR, INCOME, REAGAN), region (EAST, MIDWEST, WEST, SOUTH), ideology (ADA79, ADA81, PARTY), and contributions. Full definitions of these variables are given in the appendix.

The use of these particular independent variables to predict roll calls can be justified on several grounds. The inclusion of the demographic, or constituency, variables is justified in that demographic characteristics serve as rough indicators of some of the basic needs and concerns of constituents. Fenno (1978), for example, reports that congressmen frequently discuss demographic factors when talking about their constituencies. Even though the basic needs and concerns of a constituency are much more complex and heterogeneous than allowed for by demographic variables, the absence of better measures helps justify their inclusion (Clausen, 1973).

Because the demographic factors are rather firmly fixed, whatever needs or preferences they represent can be assumed to be longstanding. In order to capture some of the more immediate or short-term policy concerns of voters in the district, the variable for support of Reagan in 1980, REAGAN, has been included. Presumably, this variable represents constituency support for the policies of the president or the president's party. Even though it is true that some voters cast their ballots devoid of any policy considerations, congressmen may perceive that the presidential vote percentage is an indicator of voters' policy preferences. If so, congressmen may be reluctant to vote against a bill supported by the president if the president appears to have considerable support in the district, and empirical studies indicate that this is sometimes the case (Erikson & Wright, 1980; Kau & Rubin, 1982).

The region variables have been included to account for differences in outlook and behavior resulting from geographic and environmental constraints. The South, Midwest, East, and West have differed historically with regard to agricultural, environmental, and industrial concerns. Because of the geographic basis of representation in American politics, a congressman's first responsibility is usually to his district, not to the nation as a whole, and as a result, the geographic differences among districts should be

¹⁸Unfortunately, this issue is not as specific to the realtors as one might wish. It is a more general bill and, therefore, does not provide as strong a test of the effects of contributions as the bills selected for the other groups. For a discussion of the realtors' position, see Fair (1981).

reflected in congressmen's voting decisions (Jackson, 1974).

The ADA variables have been included as general measures of a congressman's ideological orientation. ADA79 is used to predict votes in the first session of the 97th Congress, and ADA81 is used to predict votes in the second session. The ADA score is a composite measure that reflects numerous voting decisions on different types of issues. Because the ADA score used here is always from the session preceding the vote being analyzed, it provides the most recent snapshot of the congressman's voting behavior. This snapshot captures not only a bit of the congressman's own personal ideological leaning, but also reflects the shared ideology of the congressman and his constituents. Because most congressmen have lived a good portion of their lives in the districts they represent, the beliefs of constituents and the beliefs of the congressman are likely to be similar owing to the common roots of both, and the ADA score is presumed to reflect these beliefs.

The contribution variables indicate the actual dollars contributed by each PAC to each congressman. Ideally, these variables should be weighted by their importance to the congressman, because all congressmen are not likely to attach the same significance to each dollar received. But because the factors that make a contribution particularly important are likely to be highly idiosyncratic across all congressmen, no weighting has been attempted in the following analysis. For some congressmen, an early contribution might be important in that it helps stimulate subsequent contributions; but for others, a late contribution might be more important, especially if the race is

close. In some cases, a contribution might stand out if it is one of just a few contributions; but in other cases, if only a few contributions are received, none of them may have any significance.

Each congressman's vote on each of the five roll calls was predicted on the basis of the explanatory variables through a logistic estimation technique. The results of the logit analysis appear in Table 3.¹⁹ The entries in Table 3 are logit coefficients which indicate the change in the log odds of voting "yea" given a unit change in an independent variable, the other independent variables remaining constant. The preferred position was "Yea" for all groups but BANKPAC and RPAC.

With the exception of the sign for AGC, all the signs of the coefficients indicate that contributions increased the probability of voting in the direction preferred by the contributing group. One explanation for the reversal of the sign for the AGCPAC coefficient is that the AGC gave

¹⁹Since the votes for AGCPAC and AMPAC occurred after the 1982 election, during the lame-duck session, 1982 contribution data were used. The Ns for the DEAC and BANKPAC equations include all members of the 97th Congress who were elected in 1980 and voted on the resolutions. The N for the RPAC equation includes only members of both the 96th and 97th Congresses who voted on the motion. Since this vote occurred relatively early in the first session of the 97th, there were not enough votes to construct an ADA score for the congressmen first elected in 1980. Hence they were excluded from the analysis. The Ns for the AGC and AMA equations include all members of the 97th Congress who were elected in 1980 with the exclusion of 58 lame duck congressmen.

Table 3. Logit Results for Contributions and Roll Calls

Variable	Roll Call Vote for				
	AGCPAC	AMPAC	BANKPAC	DEAC	RPAC
CONSTANT	-5.934	1.423	-2.2992	2.219	8.947
URBAN	0.034	-0.13	0.014	-0.050	-0.009
BLUE COLLAR	0.049	0.034	0.030	0.0006	-0.038
INCOME	0.00019	-0.00002	0.0001	0.00023	-0.0003
REAGAN	-0.006	0.040	0.006	0.066	-0.078
EAST	0.741	-1.326	-0.634	-1.366	-1.746
MIDWEST	-0.139	-1.579	0.031	-0.330	0.480
WEST	0.652	-0.583	0.292	0.531	-1.982
SOUTH	0.567	-0.615	0.524	0.897	-2.204
ADA	0.007	-0.054	0.002	-0.060	0.100
PARTY	-0.838	-0.182	-0.796	-0.507	-7.189
CONTRIBUTION	-0.00023	0.00027	-0.00004	0.00024	-0.00006
N	348	358	419	416	347
-2 log L	369.41	226.4	537.4	211.69	82.96

most of its money to conservative Republicans who happened to oppose the rule because of their objections to any tax increases at the time. This finding, however, merely underscores the fact that the congressmen who received money from the AGC voted on the basis of their ideological and partisan convictions, not on the basis of contributions.

With a little manipulation, the logit coefficients can be combined with values of the independent variables to yield predicted probabilities of voting with a contributing group's position. Two probabilities of voting with the contributing group were computed for each congressman. One probability was based on the actual contribution received by the congressman, together with his or her actual values on the remaining independent variables. The other probability was based on a theoretical contribution of zero dollars, with no change in the other independent variables. The difference between these two probabilities indicates the theoretical increase in the probability of voting with the contributing group associated with a campaign contribution.

The mean increases in the differences between these two probabilities are reported in Table 4. Each entry in the table indicates the average (over all congressmen in the analysis) change in the predicted probability of voting with the contributing group's preferred position as a result of a campaign contribution. For example, if the auto dealers had made no campaign contributions in 1979-1980, then, on average, each congressman's probability of voting to overturn the FTC regulation on used cars would have been lowered by .019 (e.g., from a .6 probability of supporting the NADA to a probability of support of .581). Clearly, the changes reported in Table 4 are rather paltry. The mean of the means across all five groups is a mere .0096. Moreover, in none of the cases were the effects of contributions great enough to change the voting outcome.²⁰

The actual results in Table 4 are quite consistent with the anticipated results, which were that the relationships between campaign contributions and roll calls would be weak. This is clearly the case. In none of the five cases examined were campaign contributions an important enough force to change the legislative outcomes from what they would have been without any contributions.

Even more important, though, to the extent that the PACs under investigation here are among the most influential PACs, the effects exhibited

Table 4. Mean Change in Probability of Voting with Contributing Group due to Difference between Zero Contributions and Actual Contributions

Political Action Committee	Change in Probability
AGCPAC	-.022
AMPAC	.038
BANKPAC	.007
DEAC	.019
RPAC	.006
Mean	.0096

by their contributions should be among the largest effects that campaign contributions can exert on roll call voting decisions. Results that scholars have labelled "substantial" or "significant," therefore, are substantial and significant only in the sense that they characterize the upper limits of PAC influence. But these upper limits pale in comparison to the unbounded influence that many scholars, journalists, and politicians have imputed to PACs.

Finally, as I suggested earlier, there is a sound theoretical reason for not expecting campaign contributions to exert effects on roll call voting any larger than those reported in Tables 3 and 4. Very simply, the organizational arrangements in which PACs raise and allocate money impose strong constraints on the ability of PACs to influence congressional voting through their campaign contributions. Thus, any time the fundraising operation and the locus of decision-making are structured as they are in the PACs of this study, contributions are not likely to carry much weight in congressional voting decisions.

Conclusions

The major finding of this research is that the ability of PACs to use their campaign contributions to influence congressional voting is severely constrained by the organizational arrangements through which money is raised. Paradoxically, the organizational arrangement that allows PACs to raise and allocate large sums of money also restricts their ability to influence roll calls. Because money must be raised at a local, grassroots level, local PAC officials, not Washington lobbyists, are primarily responsible for making allocation decisions. Consequently, congressmen who desire contributions must cultivate favorable relationships with local officials, and this arrangement tends to undercut the value of contributions as a bargaining tool for professional lobbyists.

This finding is significant in two respects. First, in contrast to previous analyses, this analysis has

²⁰This was determined by counting the number of congressmen on each roll call whose probability of support changed from less than .5 to greater or equal to .5 because of the contribution received.

demonstrated with marked clarity the limited nature of PAC influence. Of the numerous variables that influence the voting behavior of congressmen, the campaign contributions of PACs appear to take substantial effect only infrequently. Only when other cues, such as party, are weak can PAC contributions be expected to be important. The results presented here cannot support a claim that PACs never have, or never will, determine voting outcomes, but they do indicate the probable rarity of such influence.

Second, I have formulated an explanation for the limited nature of the influence of PAC contributions. This explanation adds a very substantial degree of confidence to the empirical results. Moreover, it moves the analysis in the direction of a more general understanding of the relationship between PAC campaign contributions and roll calls. Future efforts to understand roll calls in terms of campaign contributions should benefit substantially from a consideration of the organizational arrangements of PACs.

Additional tests of the explanation presented here need to be conducted. Not only is it important to investigate the frequency with which the organizational structures of the five PACs studied

here characterize other PACs, but it is also important to explore the implications of quite different organizational arrangements—for example, the organization of “institutional” groups (Salisbury, 1984) or the differences between nascent and mature organizations (Hayes, 1981). Evidence of systematic and predictable differences across different organizational arrangements would put the explanation on even firmer ground.

Finally, evidence of the relative inability of PACs to determine congressional voting should not be construed as evidence that PACs in no way matter. The growth of PACs may have altered the way congressmen allocate their scarce resources of staff and time. Today, congressmen must spend a great deal of time courting past and potential contributors to their campaigns. If such efforts drain members’ resources and energies so thoroughly that they can no longer devote themselves properly to important political issues and to formulating sound public policy, then prevailing methods of campaign finance might be considered highly inappropriate. The purpose of this article, however, has been neither to endorse PACs nor to indict them, but only to improve understanding of them.

Appendix

Variables Used for Predicting Contributions (Table 1)

Institutional Influence

AGRICULTURE	1 if the incumbent was a member of the Agriculture Committee in the 96th Congress; 0 otherwise.
APPROPRIATIONS	1 if the incumbent was a member of the Appropriations Committee; 0 otherwise.
BANKING	1 if the incumbent was a member of the Banking, Finance, and Urban Affairs Committee; 0 otherwise.
COMMERCE	1 if the incumbent was a member of the Interstate and Foreign Commerce Committee; 0 otherwise.
JUDICIARY	1 if the incumbent was a member of the Judiciary Committee; 0 otherwise.
WAYS AND MEANS	1 if the incumbent was a member of the Ways and Means Committee; 0 otherwise.
REP LEADER	1 if the incumbent was a floor leader, chairman or secretary of the party caucus or conference, a whip, deputy whip, or assistant whip; 0 otherwise.
DEM LEADER	same as for REP LEADER.
CHAIRMAN	1 if the incumbent chaired a standing or select committee; 0 otherwise.

Ideology

PARTY	1 if the incumbent is a Republican; 0 if a Democrat.
ADA79	the incumbent's score on the Americans for Democratic Action index in 1979; a score of 0 indicates conservatism; a score of 100 indicates liberalism.

Electoral Need

NEED	1 if the incumbent was ideologically sympathetic to the PAC's policy goals (ADA79 less than or equal to 50) and faced strong electoral competition; 0 if the incumbent was not sympathetic or did not face strong competition.
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APPENDIX (continued)

Variables Used for Predicting Roll Calls (Table 3)

Constituency Demographics

URBAN	the percentage of residents in a district living in urban areas (as defined by the Bureau of the Census).
BLUE COLLAR	the percentage of blue collar workers in a district.
INCOME	the median income of a district.
REAGAN	the percentage of the presidential vote in a district in 1980 for Ronald Reagan.

Region

EAST	1 if a district is in Maine, New Hampshire, Vermont, Connecticut, Rhode Island, Massachusetts, New York, Pennsylvania, New Jersey, or Delaware; 0 otherwise.
MIDWEST	1 if Ohio, Michigan, Indiana, Illinois, Wisconsin, Minnesota, Iowa, North Dakota, South Dakota, Nebraska, Kansas; 0 otherwise.
WEST	1 if Montana, Wyoming, Colorado, New Mexico, Utah, Idaho, Nevada, Arizona, California, Oregon, Washington, Alaska, Hawaii; 0 otherwise.
SOUTH	1 if Virginia, North Carolina, South Carolina, Georgia, Florida, Alabama, Louisiana, Mississippi, Texas, Arkansas; 0 otherwise.
(BORDER)	(Missouri, Kentucky, Tennessee, Oklahoma, Maryland, West Virginia).

Ideology

ADA79	the congressman's rating from the Americans for Democratic Action during the second session of the 96th Congress.
ADA81	the congressman's ADA rating from the first session of the 97th Congress.
PARTY	1 if a Republican; 0 if a Democrat.

Contributions

AGCPAC	total contributions from AGCPAC during the 1981-1982 election cycle.
AMPAC	total contributions from AMPAC during the 1981-1982 election cycle.
BANKPAC	total contributions from BANKPAC during the 1979-1980 election cycle.
DEAC	total contributions from DEAC during the 1979-1980 election cycle.
RPAC	total contributions from RPAC during the 1979-1980 election cycle.

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Woman Suffrage in the Progressive Era: Patterns of Opposition and Support in Referenda Voting, 1910-1918

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Sources of opposition and support for woman suffrage are analyzed with the use of the responses of male voters to constitutional referenda held in six key states during the Progressive era. Traditional axes of opposition and support for suffrage are examined, establishing that stable sources of suffrage support originate most often from Protestant and northern European constituencies (with the exception of Germans), whereas southern Europeans and Catholics (except for Germans) generally show no consistent patterns. Opposition to suffrage is most constant from Germans—both Catholic and Protestant—and from urban constituencies. A structural model indicating the greater importance of prohibition as an intervening variable compared to partisanship or turnout at the grass-roots level of voting behavior explicates the sources of direct and indirect support for suffrage while it also demonstrates the influence of educational commitment in determining suffrage voting patterns. Except in the West, opposition to suffrage was intense and greater at the grass-roots level than among legislative elites. The ultimate success of the federal amendment is discussed in the context of state referenda, the changed political climate after American entry into World War I, and the innovative efforts of state legislatures to grant “presidential” suffrage, thereby circumventing what proved to be the difficult referenda route.

We begin with a question: In what decade have increased use of state referenda, national debate over the rights of women, the political rise of the western states, and intense divisions over proper roles for women been an important feature of American politics? Obviously, the past decade is one answer, but an equally appropriate answer is

1910 to 1920. In that period both woman suffrage and a host of reform issues moved from the periphery of politics, becoming in some cases—such as woman suffrage and prohibition—amendments to the federal Constitution. Woman suffrage now appears uncontroversial, inevitable, and simply part of the larger question of women's rights, whereas prohibition's legacy is principally that of an unfortunate political aberration. At the time, however, both were achieved by major political campaigns involving elected political elites in state and national legislatures as well as the mass electorate empowered to vote on state constitutional referenda. Recent defeat of the Equal Rights Amendment significantly links the era of woman suffrage referenda to the politics of our own times. As a noted historian put it, “the most dramatic ramification for scholarship” of the ERA is “the esteem in which the [successful] woman's suffrage movement is held,” including the effectiveness of such leaders as Carrie Chapman Catt and Alice Paul (Pleck, 1982, p. 3).

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Woman Suffrage: Historical Overview

Before examining the neglected research opportunities at the mass electoral level, it may be useful to review the struggle for woman suffrage which formally began in 1848 with the Seneca Falls con-

vention. Given the American federal system, full woman suffrage could be obtained either by federal constitutional amendment or piecemeal by state constitutional amendments. Early women's rights leaders viewed the ties between the suffrage and abolition movements as *guaranteeing* enfranchisement of both blacks and women after the Civil War (Flexner, 1975).¹ These expectations were soon dashed by Republican political leaders, who found it virtually impossible to maneuver even black suffrage (for which there was both an ideological and a partisan advantage) past a generally hostile white male electorate and consequently judged there was no hope at all for woman suffrage.²

In 1869 the woman's suffrage movement divided into the National organization favoring a federal amendment via congressional lobbying, and the American organization, committed to state-by-state change via state constitutional amendment. Owing to insurmountable political obstacles at the congressional level, the strategy of the latter initially prevailed. When the two movements joined forces in 1890 to form the National American Woman Suffrage Association, an era ensued of literally hundreds of state campaigns. In the words of one of the most important veterans of this period:

To get the word "male" in effect out of the Constitution cost the women of the country fifty-two years of pauseless campaign. . . . During that time they were forced to conduct 56 campaigns of referenda to male voters; 480 campaigns to get [state] legislatures to submit suffrage amendments to voters; 47 campaigns to get state constitutional conventions to write woman suffrage into state constitutions; 277 campaigns to get state party conventions to include woman suffrage planks; 30 campaigns to get presidential party conventions to adopt woman suffrage planks in party platforms, and 19 campaigns with 19 successive Congresses. (Catt & Shuler, 1926, p. 107)

From 1910 to 1920 efforts were made to hold referenda on woman suffrage in most non-

southern states.³ In some, the state legislature or governor blocked the move, but in a majority the issue ultimately was put to the voters (all male). Although there were early successes in the West, in all other regions the going proved much more difficult.⁴ In fact, Kansas became the only state east of the Rocky Mountains to approve full woman suffrage before American entry into World War I in 1917. By contrast, prohibition gained strength first in the southern and border states, and after 1912 scored victories in the West.⁵

An important legal difference between prohibition and state campaigns for full woman suffrage was that the latter could *not* be won by local option at the town or county level. Yet one important loophole enabled women in a given state to

¹Women in the South were slower to mobilize in support of suffrage. However, southern white leaders also had a morbid fear of any precedent of federal involvement in regard to the electorate. See Kousser (1974) for the definitive account of the turn-of-the-century disfranchisement. Such support for suffrage as there was in the South generally was limited to the loophole of permitting women to vote in the primaries (usually decisive, and open only to whites), or later favoring state passage of a full suffrage provision in hope of avoiding the federal amendment.

²The territorial legislature of Washington had twice enacted legislation (1883 to 1886) to grant the vote to women, but both acts were voided on technicalities by the antisuffrage, Democratic-controlled state Supreme Court. (See the scathing discussion by Ostrogorski (1893, pp. 66-69).) Finally, in 1910 the state of Washington inaugurated a surprisingly successful round of suffrage referenda in the West with an almost two-to-one victory. California's Hiram Johnson, elected Governor in 1910, made woman suffrage part of his comprehensive 1911 reform package, and it was narrowly approved. In 1912 Oregon voters reversed themselves, approving suffrage narrowly. Ten of the remaining 11 western states fell into line by 1914 (Duniway, 1971; Larsen, 1971). The lone exception in the West was New Mexico, where a constitutional amendment had to be approved by three-quarters of the legislature, then by three-quarters of the voters, including two-thirds of all those voting in each county (Scott & Scott, 1982, pp. 132-136).

³Until American entry into World War I, the existence of statewide prohibition was of little assistance to the suffrage campaign, because it was concentrated either in the South (which was well-nigh hopeless for suffrage), or in the West (where suffrage had been achieved). Rather, the agitation for prohibition made the political situation more difficult for suffrage (Bordin, 1981; Epstein, 1981). On the other hand, in the West the existence of woman suffrage may well have helped the prohibition movement since Washington, Oregon, and Arizona each adopted prohibition either at the first or the second election after adopting woman suffrage.

¹Flexner (1975) provides an excellent general account of the suffrage movement in the United States. For the most detailed account, see Anthony et al. (1881-1922).

²Dykstra and Hahn (1968) establish that white electorates after the Civil War consistently voted down state referenda extending the franchise to blacks. DuBois (1978) presents a superb analysis of the difficulties of the woman suffrage movement in relation to black suffrage in the context of Kansas referenda in the Reconstruction era.

Table 1. Pattern of State Approval of Full Woman Suffrage, Presidential Suffrage, and Statewide Prohibition, 1900-1920

	Woman Suffrage		Prohibition
	Constitutional Referenda: Full Suffrage	Statutory Action: Presidential Suffrage	Constitutional Referenda or Statutory Action
Pre-1900	Wyoming, Idaho, Colorado, Utah		Maine, Kansas, North Dakota
1907			Oklahoma, Georgia
1908			Mississippi, North Carolina
1909			Tennessee
1910	Washington		
1911	California		
1912	Oregon, Kansas, Arizona		West Virginia
1913		Illinois	
1914	Nevada, Montana		Washington, Oregon, Arizona, Colorado, Virginia
1915			Idaho, Arkansas, Iowa, Alabama, South Carolina
1916			Michigan, Montana, Nebraska, South Dakota
1917	New York	Michigan, ^a Ohio, ^b North Dakota, Rhode Island, Nebraska, Indiana ^c	New Mexico, Utah, New Hampshire, Indiana
1918	Michigan, South Dakota, Oklahoma		Wyoming, Texas, Nevada, Ohio, Florida
1919		Maine, Indiana, Tennessee, Iowa, Missouri, Ohio, Minnesota, Wisconsin	Kentucky
1920		Kentucky	

^aMichigan referendum of 1918 superseded legislation of 1917.

^bOhio legislation of 1917 was repealed by popular initiative, but was reenacted in 1919.

^cThe Indiana law of 1917 was reenacted in 1919, owing to court challenge.

obtain by legislative action (rather than constitutional referendum) the right to vote for presidential electors, as provided by Article II, Section 2, of the federal Constitution, which states (in part):

Each state shall appoint, *in such manner as the Legislature thereof may direct*, a Number of Electors, equal to the whole Number of Senators and Representatives to which the State may be entitled in the Congress' (*italics added*)

This possibility came to the fore in 1913 in the state of Illinois where a vigorous campaign in the legislature for a partial suffrage bill succeeded despite intense opposition from the liquor and wet interest (Catt & Shuler, 1926, pp. 189-195). The Illinois law demonstrated powerfully a new type

of suffrage campaign, opening an alternative route that did not require direct popular approval. Furthermore, the impact of this partial woman suffrage was brought to bear on the single office of most concern to the political parties, the presidency, thereby contributing to the eventual inclusion of the woman suffrage issue on the political agendas that determine national party politics.⁶

Table 1 summarizes the pattern of state ap-

⁶As of mid-1920 and before ratification of the federal amendment, the presidential-suffrage states accounted for 173 electoral votes (excluding Michigan, which adopted full suffrage a year after the legislature ap-

proval of full woman suffrage, presidential suffrage, and statewide prohibition from 1900 to 1920 and illustrates the early victories in the West for full woman suffrage and the increased importance of Presidential suffrage in the later years.⁷ As is well known, the battle eventually shifted to Congress, which—with the firm support of President Wilson—narrowly passed the Nineteenth Amendment enfranchising women (Morgan, 1972).⁸ With the ratification of this amendment, America became *not* the first, but a belated twenty-seventh nation to include women in the electorate (Paulson, 1973).⁹

For political science the movements for prohibition and for woman suffrage raise fascinating questions about the structure of Progressive era “belief systems” (how tightly various issues were related); differences in response between office-holding elites (both state and congressional) compared with the mass (male) electorate; strategic allocation of campaign resources and the power of momentum; and the existence of such period-defined effects as those of American entry into World War I. A reanalysis of the neglected state referenda voting provides new perspectives on all of these questions. We shall concentrate our analysis on the task of determining who supported and who opposed each movement, how these groupings were related to traditional ties of Democratic or Republican partisanship, and how they varied over time and across regions.

proved the presidential vote) and full suffrage states (including the two admitted from territorial status with woman suffrage) accounted for 137 electoral votes. Thus, at the time of ratification of the national suffrage amendment, women already had either total suffrage or the presidential vote in states with 310 of the then 531 electoral votes. This total does not include Arkansas and Texas, which by 1920 had granted women the right to vote in primaries only.

⁷The greater willingness of state legislators compared to referenda voters to grant suffrage to women illustrates one of the distinctions noted by Stouffer (1955) between elites and the rank and file.

⁸Recent scholarship based on Woodrow Wilson's papers establishes that he played a much more supportive and active role in the passage and ratification of the Nineteenth Amendment than previous analyses have suggested (Lunardini & Knock, 1980-1981).

⁹Before the United States Nineteenth Amendment, women had the right to vote in the following nations: Australia, Austria, Belgium (municipal), British East Africa, Burma (municipal), Canada, Czechoslovakia, Denmark, Estonia, Finland, Germany, Great Britain, Holland, Hungary, Iceland, Isle of Man, Latvia, Lithuania, Luxembourg, New Zealand, Norway, Poland, Roumania (municipal), Rhodesia, Russia, Sweden (Catt & Shuler, 1926, p. viii).

Woman Suffrage: Axes of Opposition and Support

Much has been written about the leadership and ideas of the suffrage movement, and a good deal has been learned about financial and organizational activities of leading opponents of woman suffrage, particularly the liquor lobby and the German-American Alliance (Cashman, 1981; Kraditor, 1965). However, assumptions about patterns of opposition and support in the *mass* (all-male) electorates are asserted, but rarely tested (Rossi, 1982, pp. 2-3).

The following sources are often said to contribute to opposition to suffrage: 1) organized liquor and brewing interests plus retail saloon operators; 2) German-Americans, who saw prohibition as an assault on their *Kultur* and way of life; 3) Roman Catholic hierarchy and Irish Catholics, especially in the Northeast, where women had been granted the municipal or school ballot partly in an attempt to check growing Catholic influence; 4) big-city political machines, which often had close ties with saloons and the wet interests and presumably would lose influence with a broader (and perhaps purer) electorate; 5) immigrants from Italy and southern Europe, who represented more traditional family patterns; 6) the illiterate and poorly educated, who were more open to machine manipulation; and 7) textile and manufacturing interests, which depended on cheap supplies of women's labor.

Support for woman suffrage, on the other hand, is most often cited as stemming from the following sources: 1) those favoring prohibition, because of the close nineteenth-century alliance between the suffrage and temperance movements; 2) native-born Protestants (outside the South), who were of a northern European background; 3) immigrants of English, Scandinavian, or other northern European backgrounds; 4) Mormons in the West, and Jews in New York City; 5) reform and progressive elements who favored broader popular participation in general, and who saw women as potential allies in particular; 6) the better educated and middle class, who were more open to abstract arguments and who favored a broader role for their own wives and daughters (as in earlier campaigns for women's property rights); and 7) the younger generation and those in the West.

This list suggests axes of opposition and support for suffrage along the following dimensions: opposition derives from those who are southern European, Catholic, immigrant, less educated, urban, and do not favor prohibition, whereas support for suffrage is associated with those who are northern European, Protestant, well educated, rural and favor prohibition. However, not

only has this array of sources of presumed opposition to and support for woman suffrage been largely untested, but axes of opposition and support contain contradictions best resolved by data analysis. For example, some northern European groups, such as the Germans, include high proportions of both Catholic and Protestant denominations. This raises the question: how does national origin *interact* with religious affiliation to produce a position on the woman suffrage issue?

Even more complex is the alliance between prohibition and woman suffrage voting. It is well known that the overlapping leadership between the temperance-prohibition and woman suffrage organizations inextricably linked these social movements, creating the widespread assumption that a woman's vote was (or would be) a dry vote (Bordin, 1981, pp. 118-119; Paulson, 1973, pp. 113-114; Sinclair, 1965, pp. xxii-xxiii).¹⁰ Temperance and prohibition peaked as political issues before woman suffrage; those mobilized to prevent prohibition saw the woman suffrage issue solely in terms of the instrumental consequence it held for adding dry votes to the electorate (Bordin, 1981, pp. 156-157). Appeals were made repeatedly to (male) voters opposed to prohibition to vote against woman suffrage, for the sake of the prohibition issue:

The main purpose of the political operations of the [liquor] industry was to prevent Prohibition.

¹⁰For example, the pioneer suffragist, Susan B. Anthony, was first involved in organizing a temperance association for women in response to their exclusion from men's temperance societies in the early 1840s, and in 1852 another prominent suffrage leader, Elizabeth Cady Stanton, was elected president of the Woman's State Temperance Society in New York (Boole, 1929, p. 36; Paulson, 1973, p. 70). The national Prohibition Party platform formally endorsed woman suffrage as early as 1872 (Paulson, 1973, pp. 113-114). In 1888 Anthony and Elizabeth Cady Stanton commemorated the fortieth anniversary of the Seneca Falls convention by sponsoring a conference highlighting the close affiliation between the then international suffrage and temperance associations (Paulson, 1973, p. 118). In general, leadership overlapped between the Women's Christian Temperance Union and the organized suffrage movement in the last quarter of the nineteenth century, extending in some cases to overt cooperation between temperance and suffrage organizations in suffrage referendum campaigns (Bordin, 1981, p. 121). However, some suffrage leaders (such as Oregon campaigner, Abigail Scott Duniway) vehemently opposed the informal alliance between the prohibitionists and women's movements because they viewed this connection as responsible for arousing the hostility and opposition of liquor interests, thereby creating the single most serious threat to successful passage of suffrage referenda (Paulson, 1973, p. 140).

tion, the manner of implementing this purpose involved it in fighting allied issues and, generally, financing the opposition to the roots of Prohibitionist strength. . . . The fact that women were clearly prominent among Prohibitionists earned for Suffrage the enmity of the liquor industry. . . . Carl Bauer, president of the Michigan German-American Alliance, had circularised his organization on the eve of a Suffrage referendum. "If the suffrage would be laid into the hands of the native born American women, only the results which surely will follow can easily be predicted. Narrow-mindedness will triumph everywhere, fanaticism will flourish, prohibitionists easily set up for dictators in the State of Michigan." . . . Oscar Schmidt, a Milwaukee brewer . . . commented [on defeat of a suffrage referendum in Nebraska]: "If they [woman suffrage] had carried the election, the state would have been dry." (Morgan, 1972, pp. 159-162)

The presumed alliance between the woman suffrage movement and the prohibition movement cost the suffrage movement dearly, as the suffrage cause automatically inherited the enemies of prohibition, most notably, the brewing interests and those immigrant and religious groups most vehemently opposed to prohibition (Morgan, 1972, pp. 140-141, 156; Paulson, 1973, pp. 137, 140). Brewers' organizations zealously sought to influence voting outcomes in state referenda on both prohibition and suffrage by means of the foreign-language press, registration of immigrants opposed to prohibition, and political use of the German-American Alliance—activities that vitally strengthened the identification of prohibition as a critical intervening variable determining voting behavior on woman suffrage (Morgan, 1972, pp. 158-161).

Yet the alliance of the prohibition and suffrage issues at the level of leadership and interest group activities must not be allowed to mask equally fundamental differences—differences that could be critical sources of quite distinct voting patterns at the grass-roots level. Ideologically, prohibition and suffrage stem from very different democratic concepts (Paulson, 1973). Temperance and prohibition were a response to a social problem—drunkenness and alcoholism—viewed as threatening a democratic conception of community and society.¹¹ Temperance—the movement in which

¹¹Some analyses of the prohibition era stress urban-rural cleavages as the source of conflict (Cashman, 1981); others emphasize status anxieties entailed by discontinuities in cultural values of nativist and immigrant groups (Gusfield, 1963). However, the existence of a real social problem of alcohol abuse as a component of the prohibition movement is reflected in the following indexes: by 1830 the annual per capita consumption of

women were most actively involved—advocated voluntary abstinence or self-control, with the primary goal the restoration of the family unit threatened by increasing drinking problems. The temperance movement was referred to as “the maternal struggle,” and its slogan was home-protection, a strategic familial use of a familiar tariff legislation term (Annual Leaflet, 1898; Boole, 1929, p. 46; Bordin, 1981, pp. 3, 58, 158).¹² Prohibition substituted social control for self-control, advocating the use of government to enforce principles and habits of personal abstinence. Both temperance and prohibition, however, affirmed traditional democratic values of responsible citizenship and the conventional status quo. For this reason they were mass movements enlisting thousands of members in their organizations as well as attracting legions of impassioned voters.

By contrast, woman suffrage was part of a feminist movement viewed as radical and based on arguments of rights and principles necessitating such a new vision of women and women's relationship to society that some place it with transformations as major as the Reformation (DuBois, 1975, p. 69; Paulson, 1973, pp. 182-184).¹³ In the name of the equality asserted by the Declaration of Independence, suffrage leaders

protested the “contradiction between [democratic] principle and practice” involved in the disenfranchisement of women (Paulson, 1973, p. 85). As such, enfranchisement was viewed as the key to a more complete liberation entitling women's admittance to any and all activities conventionally categorized as the public sphere. This proposal, that women could and should step outside the private sphere of family and domestic roles to participate as equal individuals with men in the public spheres of work and politics, was a radical proposition—especially in the nineteenth century and even in early decades of the twentieth—promising (or foreboding) drastic alterations in traditional relationships between the sexes and in primary social institutions, such as the family (DuBois, 1975). The radical premise of the woman suffrage movement explains why it was temperance and prohibition—with emphasis on women's traditional role as caretaker of the home—and not suffrage, that drew a broader base of support from which to influence the suffrage issue rather than vice versa (DuBois, 1975, pp. 67-68).

When the fundamentally different nature of the prohibition and suffrage movements is considered, it becomes problematic whether voting patterns on these two issues will be related as directly as assumed from historians' accounts of the activities of the liquor industry in opposing suffrage referenda. Additional complexities in the alliance between prohibition and woman suffrage stem from recognition that there were some groups such as manufacturing interests which had a positive position on prohibition (in order to curtail the financial losses attendant from alcoholism, abstenteeism from work, and job-related injuries resulting from drunkenness), but an equally negative position on woman suffrage (in order to prevent child labor legislation which it was assumed women would support if they had the vote).¹⁴

With these considerations in mind, we analyze electoral data from suffrage, prohibition, and typical partisan elections along with demographic and religious data for six states: Ohio and Michigan in the Midwest, California and Oregon in the West, and New York and Pennsylvania in the East. The repeated Progressive era suffrage cam-

distilled spirits was three times higher than today (Rorabaugh, 1979); by 1909 there was one saloon for every 300 people, which meant there were more saloons than schools, churches, libraries, hospitals, theaters, or parks and by 1914 more than a third of the federal government's revenue came from the liquor trade (Cashman, 1981, p. 3).

¹²Members of the WCTU viewed themselves as “home” women with a focus on the welfare of children (Boole, 1929, p. 46). Perhaps their most successful political campaign was for “compulsory temperance education in the public schools” based on a plan of “graded temperance lessons for hygiene and physiology” to be taken and passed by teachers and students alike. By 1903 all but the state of Georgia had passed such legislation, grandly testifying to the WCTU's success (Bordin, 1981, p. 137).

¹³Certainly until the 1900s the suffrage movement was more advanced ideologically on issues of women's rights than most women were prepared to embrace (Bordin, 1981, pp. 156-157). However, to some degree this began to change in certain states, such as New York, where by 1915 the National American Woman Suffrage Association was able to enroll half a million women in support of the suffrage issue and could claim a national membership between 70,000 and 100,000 compared to only 13,000 paid members in 1895 (Bordin, 1981, p. 151). However, over the vast majority of years during which suffrage was an issue, it did not appeal to large numbers (in contrast to the WCTU), but rather was viewed as on the fringe of politically warranted goals if not actual respectability.

¹⁴Between 1911 and 1920 more than three-fourths of the states adopted workingmen's compensation laws. Consequently, manufacturing interests campaigned for “safety through sobriety” and increasingly favored prohibition (Cashman, 1981, pp. 5-6). However, business interests usually opposed woman suffrage, because enfranchised women would be likely to take positions on labor legislation—such as child labor laws—antithetical to manufacturing interests (Flexner, 1975, pp. 309-313).

paigns in the Midwest—three each in Ohio and Michigan—permit us to develop and to test a model for suffrage voting and to trace the changing relationship of the suffrage issue to prohibition and partisan voting as well as to turnout for suffrage referenda. The use of suffrage data from the West and East tests our model for regional differences.

Woman Suffrage: Model Specification and Data

We are interested in clarifying the sources of opposition and support for suffrage, particularly whether basic background characteristics such as nationality, religious heritage, and place of residence have a direct effect upon support for suffrage or an indirect effect by means of intervening attitudinal variables, such as attitude toward prohibition and partisan orientation. To test for direct and indirect effects of basic background (predetermined) variables, a path-type, structural model is specified in Figure 1. This model illustrates how the total effects of the background (predetermined) variables are derived from direct effects of background (predetermined) variables and intervening (attitudinal) variables as well as by means of indirect effects of background variables via intervening variables. The final dependent variable of interest is the percentage voting yes on suffrage referenda.

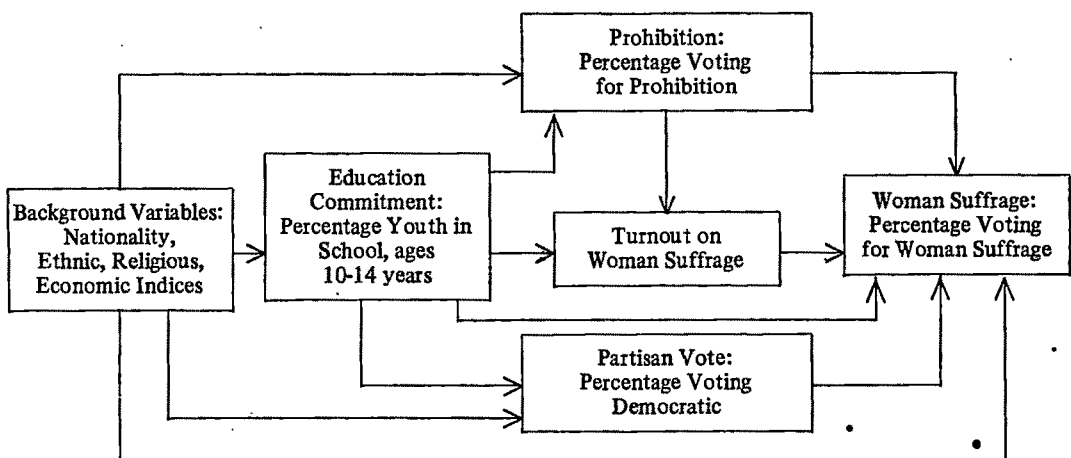
The variables are examined in terms of a causal ordering which runs from the various background (exogenous) variables through the intervening variables to the final dependent variable. Figure 1 hypothesizes that suffrage support is affected by (but does not affect) support for prohibition.

Attitude toward education (indexed by the percentage of youths ages 10-14 in school) functions as an intervening variable likely to affect each of the other three intervening attitudinal variables: traditional voter partisanship, degree of support for prohibition, and level of turnout for suffrage. In this model the background (predetermined) variables can directly effect each of the intervening variables as well as the final dependent variable: support for suffrage.

The advantage of a structural model, such as that in Figure 1, lies in the clarification of the different paths by which causal influence flows from background (predetermined) variables through intervening variables to the final dependent variable. As Alwin and Hauser (1975) point out, the total effect of a background variable can be derived by the path analysis method of multiplying the standardized coefficients along the various paths and summing the result. However, an alternative procedure, which does not require standardized coefficients, is to calculate a reduced-form equation, which predicts the final dependent variable solely from the set of background (predetermined) variables, thereby omitting all intervening variables. Subsequent equations can establish the role of the various intervening variables and test for possible direct and indirect effects that link the predetermined variables and the final dependent variable of interest. As Alwin and Hauser put it:

[the estimation of] successive reduced-form equations in the interpretation of recursive path models substitutes for the more cumbersome computation of direct and indirect effects from the coefficients of each structural equation in a recursive model. (Alwin & Hauser, 1975, p. 37)

Figure 1. Structural Model: Support for Woman Suffrage, 1910-1918



Using this approach we test the model on county level referenda voting returns for six states: Oregon, California, Ohio, Michigan, Pennsylvania, and New York.¹⁵ The county vote on various referenda and partisan elections was taken from the reports of the relevant Secretary of State for each year indicated. The specific suffrage, prohibition, and partisan votes used in our analysis are listed below in Appendix 1.¹⁶

The background variables are drawn from either the 1910 *Census of Population* or the 1916 *Census of Religion* and are listed in Appendix 2.¹⁷ First- and second-generation immigrants for foreign-born categories are combined. We have retained major religious denominations in lieu of composite categories.¹⁸ To avoid heteroscedasticity, all variables have been weighted by dividing them by the square root of the county population. The total data set for the six states examined consists of 392 counties.

We initially tested the explanatory power of large, categorical groupings of ethnic and

religious variables corresponding to the axes of opposition and support designated above as well as ethnic, religious, and economic variables taken separately as independent variables regressed upon voting variables (see Appendix 3). The census data for 1910 and 1920 monitor most of the post-Reconstruction new immigration accurately, but can provide only an indirect indicator for earlier immigration. This is particularly important in the case of the Irish and Germans. These immigration streams were of massive size in the pre-Civil War period (extending back to pre-Revolutionary days in the case of German settlements in Pennsylvania). Moreover, both nationality groups were divided into Catholic and Protestant groups. In the Midwest Irish Protestants, some of whom came to the United States via Canada, are almost as numerous in the twentieth century as Irish Catholics. To be able to test for differences between German Catholics and German Protestants we developed interaction terms.¹⁹ Similar interaction terms are used for the percentage of Irish, because it proved necessary to distinguish such Protestant Irish from those with a Catholic affiliation. (See Appendix 2 for a complete list of all background variables.)

The four major intervening variables in the model are the percentage supporting prohibition (calculated on basis of total vote cast), percentage turnout on suffrage (calculated on basis of total voting-age population), educational commitment (percentage pupils age 10-14 enrolled in school), and percentage voting democratic (calculated on basis of total vote cast).²⁰ There are strong *a priori* reasons for considering the role of each of these possible intervening variables in the overall causal structure effecting support for suffrage.²¹ Despite Progressive inroads, this was still an era of intense partisanship at both elite and mass levels.²² Although prohibition and especially suffrage were kept at the periphery of issues endorsed by the

¹⁵We are, of course, well aware of the problems of so-called ecological correlation. We present no correlations or other standardized measures. Economists, who regularly depend upon aggregate data, have developed an extensive literature on possible "aggregation bias," dating back to Theil (1954). For useful introductions to the problem, see Hannan (1971), Langbein and Lichtman (1978), Gillespie and Zinnes (1982), and Hanuschek and Jackson (1977).

¹⁶The Michigan 1912 prohibition measure is the local option vote held in 35 counties in 1912 and in closely adjacent years for most of the remaining counties. For the few counties not holding a local option vote, we have generated a predicted vote. The 1917 suffrage vote in Ohio was for the repeal of the legislature's grant of presidential-only suffrage.

¹⁷The *Census of Religion* does have some problems as a data source, such as different denominational practices of "dating" membership from birth versus from other points in the life cycle as well as the selective manner in which some returns were recorded. However, the resulting inaccuracies do not seriously preclude using this data resource as a means for obtaining information on the relative size of religious groupings.

¹⁸The Midwest has been the focus for analyses emphasizing ethnocultural differences, especially those based on differing religious orientations, such as liturgicals and pietists; see Jensen (1971) and Kleppner (1970). We did not find this dichotomy helpful for our post-1910 analysis for two reasons: by this time Protestant groups had evolved and subdivided—there were 23 Lutheran groups by 1900—making classification difficult, and also, the Pietist-Liturgical typology does not capture aspects of church organization and attitude toward government and society (Vander Meer, 1981). Kleppner himself has argued (1982, p. 71) that ethno-religious conflict was of only quite limited significance for the so-called "fourth party system" (1890s to 1930).

¹⁹Friedrich (1982) makes a strong case for including—rather than excluding—multiplicative (interaction) terms in regression analysis.

²⁰Although the measurement of turnout involves the "ratio variable" controversy, a most recent exposition of the topic argues the case for its inclusion in the model (Long, 1980).

²¹For elections held in 1912 we explored the effect of percentage voting for Progressive Party candidate, Theodore Roosevelt, but this possibility of a fifth intervening variable did not prove particularly useful in analyzing the mass electorate.

²²See Lichtman (1979) for the most extensive analysis of the determinants of partisanship using aggregate data. Although primarily concerned with the 1928 election, Lichtman also traces patterns of partisanship as far back as 1916.

traditional two-party system, it is nevertheless reasonable to investigate possible partisan influences when suffrage votes came up at a general election necessarily involving intense partisan activities (which was typically the case). As discussed above, prohibition, whether at state, county, or national level, was widely regarded as being inextricably linked to woman suffrage and an important determinant of suffrage voting choice owing to the lobbying activities of wet interest groups (Timberlake, 1963).²³

Voter turnout on suffrage referenda should reflect the relative success of supporters or opponents in getting out the vote and making a persuasive case (not to mention buying votes and political taxidermy or ballot-stuffing).²⁴ Counties also differ substantially in their commitment to whether children aged 10-14 are in school—whether public or parochial—rather than dropping out to join the labor force.²⁵ We expected that a strong commitment to education for the young would be associated with support for prohibition, higher turnout, and support for woman suffrage. As we soon shall see, this is precisely the case.²⁶

Data Analysis

The Midwest

The Midwest was by far the fiercest regional battleground for both prohibition and for woman suffrage. Repeated referenda votes on both issues provide the analyst with the unusual opportunity to estimate a model on one set of data and then test it against a separate data set. We develop our model initially in the context of the 1913 suffrage vote in Michigan pooled with the 1914 vote in

Ohio. This analysis then will be contrasted with earlier suffrage voting (1912 in both states) and with later suffrage votes (1917 in Ohio and 1918 in Michigan).

We explored a variety of specifications both for Ohio and Michigan separately as well as for the two states pooled into a single data set.²⁷ (See Appendix 3.) The 18 variables indicated in Table 2 emerged as our preferred set of background (predetermined) variables. The five equations presented in the columns of Table 2 indicate the unstandardized coefficients of the 18 background characteristics in relation to each of the four intervening variables—educational commitment, Democratic partisanship (1916 election), support for prohibition, and turnout on the suffrage vote—as well as the final dependent variable: support for woman suffrage.

Looking at the background variables in relation to suffrage and prohibition voting, we see general verification that sources of support for suffrage stem from Protestant, northern European, and rural elements, such as percentage native born, number of farms per county, percentage English, Scandinavian, Presbyterian, and Episcopal. The only significant negative coefficient is for percentage Methodist, perhaps reflecting opposition to suffrage among those with southern antecedents. Although prohibition voting is similar to suffrage voting, it reflects the greater importance of economic variables such as population density and percentage manufacturing wage earners and also registers a negative coefficient for percentage Italian. Expected opposition to suffrage and prohibition from German and Irish groups is not evident in these equations with background variables alone; however, the large number of significant background variables related to Democratic vote, suffrage turnout, and education (equations 3, 4, and 5), suggests that a more complete pattern will emerge when these latter intervening variables are included in the full structural equations analyzing suffrage support.

Looking at the social determinants of the suffrage vote, prohibition vote, and partisan vote, we note the highest R^2 is for prohibition, followed by suffrage, and the lowest for percentage Democratic, indicating the somewhat greater extent to which the response to suffrage and prohibition reflected stable constituencies. Furthermore, suffrage in particular is marked by many more positive sources of support compared to negative, in-

²³Many of the early suffrage leaders had become politically active via the WCTU, and wet interests generally assumed that women as voters would substantially strengthen the dry cause (Flexner, 1975, pp. 306-309).

²⁴We do not have complete county-level data on the degree to which the suffrage campaigns may have succeeded in activating women. One presumes that highly committed women were quite successful in converting brothers, husbands, or fathers. After all, the anti-suffrage case rested, ultimately, on the argument that most women really did not want the vote.

²⁵The proportion-in-school variable indexes commitment to education and compulsory attendance. As such, it certainly is related causally to various background characteristics. Hence, it cannot appropriately be a purely background (predetermined) variable itself.

²⁶Indeed, the four intervening variables taken alone (without any of the background variables) typically explain over 90% of the variance in referenda votes for suffrage.

²⁷We also tested a variety of dummy variables, both to index individual states and to test for possible sub-state cultural differences for areas such as the Western Reserve in Ohio and the southern-oriented Ohio River counties in Ohio, but these did not prove significant.

Table 2. Unstandardized Regression Coefficients for Suffrage Support, Prohibition Support, Suffrage Turnout, Democratic Vote and Educational Commitment in Ohio and Michigan, 1913-1914 (Reduced form equations)

Background (Predetermined) Variables	Suffrage Vote ^a Eq. (1)	Prohibition Vote ^b Eq. (2)	Democratic Vote ^c Eq. (3)	Turnout Suffrage ^a Eq. (4)	Education Commitment ^d Eq. (5)
English ^e	2.873***	2.753***	-0.141	1.591***	3.489***
Scandinavian	0.945***	0.914***	0.214	0.375*	1.543***
Dutch	-0.223	0.396	-0.128	-0.372	0.735
Italian	-0.822	-1.400*	-0.029	-1.630**	-1.466*
Congregational	0.435	-0.849	-0.234	-0.900	-1.312**
Methodist	-0.681**	-0.620**	-0.588**	-0.279	-1.360***
Episcopal	5.211***	6.556***	2.649*	3.641**	9.210***
Presbyterian	1.271**	1.343**	0.587	0.952*	1.211*
Native born	0.277***	0.315***	0.412***	0.371***	0.558***
Black males	0.351	0.092	-0.200	-0.574	0.912
German X Catholic	-0.004	0.002	-0.004	0.015*	0.022**
German X Protestant	-0.008	-0.013	0.018**	0.012	0.003
Irish X Catholic	0.091	0.074	0.169***	0.116*	0.126*
Irish X Protestant	-0.050	-0.016	-0.110*	0.039	-0.052
Population Density ^f	3.004	4.750*	7.197***	4.906*	9.758***
Farm ^g	170.413***	253.301***	48.555**	144.484***	294.448***
Manufacturing ^h	0.365	0.462*	0.347*	-0.192	0.521*
Constant	-0.058	-0.069*	-0.057*	-0.037	-0.093*
Adjusted R ²	0.892	0.931	0.852	0.872	0.954
N	171	171	171	171	171

^a 1913 Michigan pooled with 1914 Ohio.

^b 1916 Michigan pooled with 1915 Ohio.

^c 1916 Michigan pooled with 1916 Ohio.

^d Pupils in school, age 10-14.

^e County percent.

^f Log of population per square mile.

^g Farm: number of farms per population.

^h Manufacturing: percentage wage earners.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

dicating reservoirs of strength not directly evident from the popular vote itself where majorities rejected the issue. (In Michigan only 39% of the electorate voted "yes" on suffrage in 1913, and in Ohio 47.3% voted "yes" in 1914.)

In Table 3 we indicate the effects of the intervening variables operating in our structural model, thereby differentiating between the direct and indirect effects of the background variables (see Figure 1). An examination of the structural equations in Table 3 shows that at least in this election the principal paths determining suffrage support are through attitude toward education and degree of support for prohibition rather than through traditional partisan ties or differences in suffrage turnout.²⁸ We would expect the prohibi-

tion coefficient to be positive in relation to suffrage support, given the alliance between the anti-prohibition wets and the antisuffrage forces (as discussed above). However, education emerges as an equally significant and positive influence on suffrage support. Furthermore, when the education variable is added to the predetermined variables as a regressor on the voting variables (equations 1 and 2), the number of predetermined variables that are statistically significant is reduced almost by half for both suffrage and prohibition, indicating that many of the background variables listed in Table 2 affect the voting variables indirectly through intervening variables, such as education.

²⁸Kleppner (1982) attributes relatively little importance to education measured as the number of residents

aged 6-20 in school. However, the high age bracket used tends to capture college towns.

The powerful opposition of the German community to prohibition is evident in the highly significant and negative coefficients for the German Catholic and German Protestant terms in equation 2, Table 3. This German opposition to prohibition produces an indirect effect on suffrage voting, because prohibition is an intervening variable strongly affecting suffrage support (equation 1). In addition, German opposition is a further influence on suffrage as indicated by the direct and

negative impact of the German Catholic term on suffrage support (equation 1).

Although population density and German nationality are common negative influences on suffrage and prohibition support, it is equally interesting to note the distinctly different sources of support for prohibition and for suffrage. For example, percentage Methodist and percentage Presbyterian are significant and positively related to prohibition support, but are statistically insignificant

Table 3. Unstandardized Regression Coefficients for Suffrage Support, Prohibition Support, Democratic Vote, and Suffrage Turnout in Ohio and Michigan, 1913-1915

	Suffrage Vote ^a Eq. (1)	Prohibition Vote ^b Eq. (2)	Democratic Vote ^c Eq. (3)	Turnout Suffrage ^a Eq. (4)
Background (Predetermined) Variables				
English ^d	0.276	0.283	-1.193***	0.198
Scandinavian	-0.039	-0.178	-0.243	-0.401*
Dutch	-0.657	-0.124	-0.330	-0.763
Italian	0.383	-0.359	0.404	-1.171*
Congregational	1.356***	-0.080	0.167	-0.278
Methodist	0.119	0.342*	-0.139	0.502*
Episcopal	-1.115	0.036	-0.103	-0.412
Baptist	-0.263	0.192	-0.891*	-0.034
Presbyterian	0.239	0.485*	0.175	0.672
Native born	-0.062	-0.080*	0.247***	0.082
Black males	-0.186	-0.554	-0.422	-1.270**
German X Catholic	-0.014**	-0.014***	-0.011*	-0.002
German X Protestant	-0.003	-0.015***	0.018**	0.003
Irish X Catholic	0.015	-0.016	0.131**	0.052
Irish X Protestant	-0.027	0.021	-0.094*	0.073
Population density ^e	-2.736*	-2.157*	4.160**	-0.543
Farm ^f	-46.465*	26.872	-40.849	28.480
Manufacturing ^g	-0.010	0.093	0.187	-0.374*
Intervening Variables				
Education commitment ^h	0.408***	0.708***	0.300***	0.815***
Prohibition support ^b	0.395***	—	—	-0.527***
Democratic vote ⁱ	-0.064	—	—	—
Suffrage turnout ^a	0.041	—	—	—
Constant	6.909	-0.033	-0.028	0.002
Adjusted R ²	0.958	0.979	0.876	0.910
N	171	171	171	171

^a1913 Michigan pooled with 1914 Ohio.

^b1916 Michigan pooled with 1915 Ohio.

^c1916 Michigan pooled with 1916 Ohio.

^dCounty percent.

^eLog of population per square mile.

^fFarm: number of farms per population.

^gManufacturing: percent wage earners.

^hPupils in school, age 10-14.

ⁱ1916 Michigan and Ohio vote pooled.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

nificant in relation to suffrage support, whereas the reverse is true for percentage Congregational. This finding accords with historical accounts that emphasize common elements of direct opposition to prohibition and the efforts of antiprohibition interest groups getting out an antisuffrage vote. The fact that sources of direct support for prohibition and suffrage do not show as much commonality indicates the presence of diverse constituencies.

Although turnout on suffrage is not significantly related to suffrage support, it is interesting to note in equation (4), Table 3, that prohibition support is negatively related to suffrage turnout. This suggests that the antiprohibition interests were more effective in generating turnout on suffrage referenda compared to those supporting prohibition. However, the fact that turnout for suffrage is not statistically related to support for suffrage indicates that even if antiprohibitionists got more voters to the polls on the suffrage issue, the voters did *not* vote more strongly against suffrage than would otherwise be expected. This finding supports the proposition that examination of referenda voting patterns uncovers complexities in the relationship between the prohibition and suffrage issues at the grass roots level not apparent from historical accounts of the confounding of these issues at the level of organizational elites and interest group politics.

In the case of suffrage and prohibition we have the unusual advantage of being able to test our model against additional referenda elections in the Midwest. Thus, in Table 4 we compare the coefficients for the pooled Michigan and Ohio 1912 suffrage referenda with those we have already presented for the second suffrage test (1913 in Michigan pooled with 1914 in Ohio) and with the coefficients for the third suffrage round in these states (1917 in Ohio pooled with 1918 in Michigan). As a further test, we analyze the change in percentage support for suffrage between these successive referenda.

There is, however, one marked difference in the data which affects the pattern of intervening variables in the case of the 1912 votes (Warner, 1964, pp. 312-353). In both states the liquor question at this election was one of county local option rather than statewide prohibition (and in Michigan the local option question was only on the ballot in years just before or shortly after 1912). As presented in Table 4, equation (1), the local-option measure of prohibition sentiment does not prove significant as a predictor of the 1912 suffrage vote.²⁹ Apparently, local option campaigns

did not generate the same intensity as proposals for statewide prohibition.

There is substantial consistency over time between the background variables and support for suffrage. Except for equation (1), support for suffrage depends heavily on attitude to prohibition and on education but is not significantly related to level of turnout or percentage Democratic of the partisan vote. Additional direct effects of opposition to suffrage are evident in the negative coefficients for percentage Dutch, percentage black males, and for the two German terms. The one consistent source of additional direct support for suffrage is percentage Congregational.

In the last two columns of Table 4 we test our full set of variables against the change in level of support for suffrage between successive suffrage referenda. Given the relative stability of the coefficients for the pattern of support, we would expect few significant coefficients among the background variables, and this is the case. The overall explanatory power is substantially less, and neither education nor prohibition support is significantly related to the change in the pattern of suffrage support. Changes in turnout, however, do appear to be very significantly related to changes in the level of suffrage support, even though turnout itself does not directly affect the outcome of each suffrage referenda (see equations 2 and 3). This finding again indicates the complex relationship between forces that affected turnout for suffrage and the consequent effect such turnout had for the success or failure of the suffrage issue.

A comparable analysis of the stability of patterns in prohibition voting over time is not possible for Michigan and Ohio jointly. During this period, Michigan had scattered local option contests, but only one state referendum vote on prohibition—the 1916 prohibition referendum that passed (Englemann, 1979). This outcome had been prophesized by Michigan Governor Osborn in response to the infamous stolen suffrage referendum of 1912, where an apparent suffrage victory had been turned into a narrow defeat by means of devious ballot-counting procedures apparently launched by opponents to prohibition fearing the dry consequences of women obtaining the right to vote:

If the liquor interests defeat the [suffrage] amendment by fraud, proved or suspected, the people of Michigan will retaliate in my opinion

²⁹However, if one substitutes the earliest subsequent statewide prohibition vote (1914 in Ohio and 1916 in

Michigan), support for prohibition does prove highly significant in relation to support for suffrage, whereas the list of significant background variables remains virtually unchanged.

Table 4. Unstandardized Regression Coefficients for Suffrage Support and Change in Support between Successive Referenda in Michigan and Ohio, 1912-1918 (Structural equations)

	Suffrage Vote			Suffrage Vote Change	
	1912 ^a Eq. (1)	1913-1914 ^b Eq. (2)	1917-1918 ^c Eq. (3)	1912-1914 ^d Eq. (4)	1913-1917 ^e Eq. (5)
Background (Predetermined) Variables					
English ^f	0.592	0.276	-0.585	-0.111	-0.718*
Scandinavian	0.018	-0.039	-0.107	-0.094	0.059
Dutch	-1.446***	-0.657	-1.340**	0.816**	-0.526
Italian	0.237	0.383	0.235	-0.106	0.246
Congregational	0.930**	1.356***	0.740*	0.472*	-0.601**
Methodist	-.304	0.119	-0.295	0.479**	-0.441
Episcopal	-1.259	-1.115	-0.365	0.138	0.297
Baptist	0.098	-0.263	-0.076	-0.372	0.086
Presbyterian	0.412	0.239	-0.436	-0.006	-0.771*
Native born	-0.109*	-0.062	-0.104	0.026	0.025
Black males	-0.831*	-0.186	-1.172**	0.474	-0.461
German X Catholic	-0.023***	-0.014*	-0.007	0.005	0.004
German X Protestant	-0.013**	-0.003	-0.016**	0.004	-0.007
Irish X Catholic	0.008	0.015	-0.042	0.015	-0.045
Irish X Protestant	-0.032	-0.027	0.060	-0.001	0.074*
Population density ^g	3.932**	-2.736*	-0.516	-7.362***	0.774
Farm ^h	4.755	-46.465**	-15.634	-46.685**	25.317
Manufacturing ⁱ	0.149	-0.010	-0.008	-0.067	0.162
Intervening Variables					
Education commitment ^j	0.581***	0.408***	0.531***	0.076***	0.031
Prohibition support ^k	0.056	0.395***	0.503***	-0.005	0.087
Democratic vote ^l	-0.269***	-0.064	-0.150	0.153**	-0.074
Suffrage turnout ^m	0.096*	0.041	-0.101	0.140***	-0.375***
Constant	-0.032	6.909	0.013	0.035*	0.014
Adjusted R ²	0.961	0.958	0.964	0.440	0.676
N	171	171	171	171	88

^aMichigan and Ohio pooled.^b1913 Michigan pooled with 1914 Ohio.^c1918 Michigan pooled with 1917 Ohio.^d1913-1914 vote compared to 1912 vote.^e1917-1918 vote compared to 1913-1914 vote.^fCounty percent.^gLog of population per square mile.^hFarm: number per population.ⁱManufacturing: percent wage earners.^jPupils in school, age 10-14.^kEquations 1-3: prohibition vote nearest in time to suffrage vote; Equations (4) and (5) change in prohibition vote between successive referenda.^l1916 Michigan and Ohio pooled.^mEquations (4) and (5): change in suffrage turnout between successive referenda.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

by adopting statewide prohibition; the question seems to be largely one as to whether the liquor interests own and control and run Michigan. (quoted in Catt & Shuler, 1926, p. 181)

In Ohio, however, the frequency of prohibition referenda allows us to assess the stability of prohibition sentiment in that state from 1915 to 1917 and from 1917 to 1918. For the latter pair of votes we regressed change in prohibition strength on our standard set of 18 background variables plus education. All coefficients were found to be insignificant, and the adjusted R^2 was 0.001. Between 1915 and 1917, however, the United States had entered the war against Germany. Here we found change in the percentage for prohibition significantly related to the percentage English and percentage native born, but the adjusted R^2 was still only 0.121. Thus, aside from the limited changes triggered by American involvement in World War I, the successive prohibition votes exhibit even greater stability than that displayed by the successive suffrage referenda.³⁰

Overall our analysis of suffrage referenda in the Midwest underscores the importance of our structural model (Figure 1) in identifying sources of support and opposition to suffrage. Of particular interest is the finding that support for prohibition and educational commitment are extremely powerful as influences on suffrage support in contrast to partisan vote and suffrage turnout, which are insignificant. Direct negative sources of opposition are typically the same for suffrage and prohibition, which suggest the success of anti-prohibition forces in confounding the suffrage issue with the prohibition issue. However, direct positive sources of support for suffrage and prohibition are diverse, indicating distinct constituencies for these two issues at the grass-roots level of voting behavior.

The West and East

We have seen in Ohio a situation of intense opposition to suffrage linked to a generation of conflict over prohibition in a state that was the cradle of the Women's Christian Temperance Union and the birthplace of the Anti-Saloon League (Stegh, 1975). In the West the 1910 approval of suffrage in Washington is the prototype of the opposite context: a suffrage campaign in the absence of large-scale organized opposition. The result in Washington is a low degree of cleavage, reflected by our 18 background variables producing no statistically significant coefficients when run

against support for suffrage (and an insultingly negative adjusted R^2).³¹

The story was strikingly different in neighboring Oregon, however, where suffrage was caught up in the prohibition issue, and repeatedly voted down (Kessler, 1980; Moynihan, 1979). Suffrage finally passed in 1912, after Oregon's neighboring states had adopted it. Finally, there is the case of California, where an extensive 1896 suffrage campaign had faced strong last-minute opposition in the San Francisco Bay area. It was defeated by the heavy antisuffrage margins in San Francisco and Alameda (Oakland) counties generated by wet interests opposed to woman suffrage (Catt & Shuler, 1926, pp. 123-124).

By 1911 California had local option by individual cities and towns (rather than by county), thus freeing the woman suffrage issue somewhat from linkage with prohibition (Ostrander, 1957). The suffrage issue also had the advantage of coming up as part of a popular "reform" package backed by newly elected governor Hiram Johnson. Most of northern California other than the Bay area was highly supportive of suffrage.³² Although the statewide vote was in doubt for several days, suffrage ultimately carried the state (winning in all but eight counties), despite losing in San Francisco County by almost 14,000 votes and in Alameda County (Oakland) by more than 2,000.³³

In Table 5 equations (1) and (2) examine the pooled data for the 1911 suffrage vote in California and the 1912 suffrage vote in Oregon. The first equation (without intervening variables) indicates that suffrage draws support from counties with a high percentage of English and native born residents as well as those with a high percentage of Congregationalists, a pattern similar to that of the Midwest. Negative opposition to suffrage in the West also substantially parallels that of the Midwest, stemming from counties with high population density and a high percentage of Dutch. The similarities between the West and Midwest remain when we look at equation (2) in Table 5, where the intervening variables are added. Here we see the

³¹Space precludes presenting the analysis of Washington data.

³²Up in Mark Twain's Calaveras County, suffrage leaped out to an almost two-to-one margin, and in some far northern counties, such as Modoc (on the Oregon border) and Shasta, the margin for suffrage exceeded two to one.

³³Alameda resident Jack London wrote later that he voted for woman suffrage in the hope that they "would vote John Barleycorn out of existence" and thus serve as "the true conservators of the race." If so, London was something of an exception in his motivation pattern (Sinclair, 1965, p. 323).

³⁰See authors for analysis.

same reduction in the number of direct background variables significantly related to suffrage, which indicates that most background variables on suffrage have an indirect effect vis-à-vis intervening variables (see Figure 1). A new partisan dimension to the suffrage vote is apparent with the negative coefficient for the Democratic vote, perhaps reflecting the identification of the suffrage issue with the progressive reforms of Republican Hiram Johnson as well as the growing dis-

taste of suffrage leaders—particularly in the West—with traditional Democratic party leadership (Snapp, 1975).

In the East we turn to a region where political parties and machines were much stronger and fear of statewide prohibition less of an issue. In November, 1915, referenda on woman suffrage were simultaneously held in several states, including the crucial states of New York and Pennsylvania. In both, suffrage went down to defeat

Table 5. Unstandardized Regression Coefficients for Suffrage Support in California, Oregon, Pennsylvania and New York, 1911-1915

	California and Oregon, 1911-1912 ^a		Pennsylvania and New York, 1915 ^b	
	Reduced-Form Eq. (1)	Structural Eq. (2)	Reduced-Form Eq. (3)	Structural Eq. (4)
Background				
(Predetermined) variables				
English ^c	2.015*	0.269	1.180	-0.658
Scandinavian	0.611	-0.223	2.420***	1.144*
Dutch	-62.562***	4.326	-1.602	-2.814
Italian	1.896*	0.412	1.523*	0.673
Congregational	4.955*	3.412*	0.149	-0.262
Methodist	0.250	0.110	0.895*	0.933***
Episcopal	-5.101*	0.874	0.875	-0.033
Baptist	-1.279	2.080	0.621	0.639
Presbyterian	3.512	2.444	0.176	0.297
Native born	0.374*	-0.021	0.502***	-0.223
Black males	11.263*	4.899	-0.186	-1.328
Jewish	-	-	-1.074	-0.386
German X Catholic	-0.015	0.004	-0.026	-0.029
German X Protestant	0.127	-0.006	0.099***	0.027
Irish X Catholic	0.161	0.043	0.091**	-0.004
Irish X Protestant	-0.054	-0.030	-0.093*	-0.033
Population density ^d	-10.315***	-7.593***	-5.143**	-1.794
Farm ^e	100.316	0.291	-54.940	-85.732
Manufacturing ^f	0.517*	0.107	0.646**	0.429*
Intervening variables				
Education commitment ^g	-	0.651***	-	0.711***
Democratic vote ^h	-	-0.106***	-	-0.002
Suffrage turnout	-	-0.001	-	-0.142***
Constant	0.076	0.010	0.014	0.008
Adjusted R ²	0.904	0.955	0.810	0.858
N	92	92	128	128

^aCalifornia 1911 pooled with Oregon 1912.

^bPennsylvania 1915 pooled with New York 1915.

^cCounty percent.

^dLog of population per square mile.

^eFarm: number of farms per population.

^fManufacturing: percent wage earners.

^gPupils in school, age 10-14.

^hCalifornia 1910 vote pooled with Oregon 1914 vote; Pennsylvania 1914 vote pooled with New York 1916 vote.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

by a substantial margin. Both New York City and Philadelphia turned in massive antisuffrage majorities. However, there were some major differences between the two states, not the least being the Pennsylvania constitutional provision barring a second referendum vote on a defeated proposal for the next five years. In New York, by contrast, an even greater effort was made to have the legislature repass the proposal in 1916, and it was adopted by the voters in November, 1917.

In Table 5, equations (3) and (4), we present reduced-form (background variables only) and structural equations (background variables and intervening variables) for the 1915 suffrage vote in Pennsylvania pooled with the 1915 suffrage vote in New York.³⁴ From Table 5 it appears somewhat more difficult to account for the sources of support and opposition to suffrage than for the other regions examined, as evidenced by the lower adjusted R^2 s. However, for those groups statistically related to suffrage support, we find (equation 3) many more positively related than negatively related, including positive support from the percentage Scandinavian, Methodist, native born, and manufacturing as well as from unexpected positive sources such as the percentage Italian, German Protestant, and Irish Catholic. Groups negatively related to suffrage are percentage Irish Protestant and population density.³⁵ When the intervening variables are added (equation 4), most of the background variables drop out as direct effects, leaving as positive sources of support the percentage Congregational and manufacturing along with the intervening educational variable, a pattern similar to the one reported for the Midwest. The significant negative coefficient for suffrage turnout in the East is difficult to assess in the absence of a measure of prohibition support, but may reflect the intensity and controversy of the suffrage issue independent of an association with prohibition referenda.³⁶

³⁴Percentage Jewish has been added to the usual 18 background variables for analysis of the East.

³⁵Final New York City curbside betting odds were three to one that Tammany would capture the mayoralty (with Judge Hylan), but only even money on the outcome of the suffrage amendment (*New York Times*, 1917).

³⁶Suffrage lost in all five boroughs of New York City, and also in all but five scattered counties in upstate New York (in which we include Suffolk and Nassau for convenience). Within New York City, suffrage received extremely heavy support from predominantly Jewish Assembly Districts of the lower East Side of Manhattan, South Bronx, and part of Brooklyn. Tammany strongholds, however, were heavily opposed. Between the 1915 and 1917 elections, the United States entered World War I, and many women became involved in

New York also provides insight into the possible effects of World War I as well as of generational differences. A major effort was made to encourage soldiers and sailors (most were under 30) to vote, and these votes were tabulated separately, by county. This provides an interesting slice, if not a sample of distinctly younger male voters and also means that the civilian electorate was somewhat skewed in favor of older male voters. The predominantly young soldier and sailor vote was substantially more prosuffrage than the general civilian vote.

Conclusion

A detailed analysis of referenda voting patterns in our three major regions suggests a reevaluation of usual generalizations about the axes of support and opposition. Were northern European immigrants more supportive of suffrage? Yes, in the case of the Scandinavians and English, but not in the case of the Dutch or Germans. Nor were southern European immigrants, Italians in particular, a significant source of opposition. Were Catholics a general source of opposition? German Catholics were consistently opposed to suffrage, the Irish only to some degree opposed, whereas Italian Catholics do not show a statistically significant negative relationship to suffrage voting. The urban-rural split on the suffrage issue is most clearly seen in the opposition of the more densely populated areas rather than in the support of rural areas, whereas manufacturing measures, when suffrage, perhaps indicating support from labor in contradistinction to managerial opposition.

It is understandable why leaders of the woman suffrage movement have stressed the sources of opposition to woman suffrage, particularly the

war-related efforts. For the 1917 referendum there was a massive campaign conducted in the state, and especially in New York City. Woman suffrage was endorsed by all New York political parties, and by such notables as President Wilson, Treasury Secretary McAdoo, "Colonel" Theodore Roosevelt, Governor Whitman, ex-mayor John Purroy Mitchel, Socialist Morris Hillquit, and Tammany mayoral candidate Judge John F. Hylan. The conventional explanation is that Tammany withdrew its active opposition and was the key factor in reversing the outcome. However, even though it is true that Manhattan (New York County) did display one of the larger shifts, it also is true that such shifts were almost precisely matched in the other four boroughs (where Tammany did not have predominant influence), plus adjoining Westchester County. Suffrage thus carried the city by over 92,000 and the state as a whole by roughly 100,000. Much more was involved than the benevolent neutrality of Manhattan-based Tammany Hall.

liquor lobbying interests and the manipulation of electoral processes (Catt & Shuler, 1926).³⁷ Our analysis confirms the intensity of German and urban opposition to suffrage, but also indicates that at the level of the male mass electorate, there were enduring sources of positive support for woman suffrage across votes and across regions. Counties with high percentages of Protestant and northern European inhabitants (with the exception of the Germans) supported suffrage, whereas those with southern European, Irish, and Catholic (with the exception of the Germans) backgrounds did not evidence a systematic orientation to this issue. Thus, at the grass-roots level we find considerable modification of traditional axes of opposition and support for suffrage. Although urban measures are most often related to opposition to suffrage, we do not find support for suffrage from rural measures to the degree expected. Although it is clear that there were enduring sources of both opposition and support for suffrage, we also note the stability of non-aligned positions of Irish, southern Europeans, and (non-German) Catholics, thereby contradicting assumptions that these groups would be negatively oriented toward the suffrage issue.

In addition to delineating background variables related to support and opposition to suffrage, the specification of a structural model also enables us to determine alternative attitudinal paths by which support for suffrage was effected. Of particular importance is the finding that the percentage voting Democratic is usually insignificant in accounting for suffrage support, demonstrating the degree to which the suffrage issue was cut off from traditional party alliances and party identification.³⁸ Also surprising was the consistent and powerful effect exercised in all three regions—West, Midwest, and East—by our educational commitment variable, which proved highly significant in regard to suffrage support, as well as for turnout on suffrage and support for prohibition. We did note that controversy on the prohibi-

tion issue did affect turnout on suffrage, yet turnout itself displayed no consistent effect on percentage voting for suffrage. Finally, we report with surprise our finding that the Progressive vote in 1912 (for Theodore Roosevelt) was not significant as an intervening variable influencing suffrage support. This suggests that the radical premise of the suffrage issue implicit in its prescription for altering traditional patriarchal political relations between the sexes casts the suffrage issue in an unusually complex position in relation to other progressive reforms of this era.

The specification of our structural model also illuminates the mediating role of prohibition. In the first place, the salience of the issue varied from low concern to a dominant issue (as in Ohio). The very presence of a statewide referendum on prohibition indicates concern beyond a certain threshold, a point not reached in New York or Pennsylvania nor in California.³⁹ Secondly, although it is true that suffrage and prohibition shared some of the same sources of support, in general the sources of support for prohibition were drawn from a wider range of Protestant and northern European groups than were present for suffrage voting. This confirms the view that prohibition was a mass movement mobilizing extensive support at the grass-roots level in response to a readily perceived social problem. The smaller base of support for suffrage, although stable over time, confirms interpretations of suffrage as a complex ideological issue entailing a radical prescription for women's entry into public domains traditionally reserved for men. As such, woman suffrage was a progressive reform requiring decades for majorities to affirm even with the constant presence of adherents steadfastly committed to it in an era particularly marked by concern for democratic values.

We have benefitted from accounts of organizational leaders involved in the struggle for women's rights and from historians' perceptive interpretations. However, the analysis of the actual votes cast by millions of male citizens via state referenda is long overdue. New perspectives resulting

³⁷State authorities could put suffrage proposals at a serious disadvantage by printing them on a separate ballot, especially a separate colored ballot (as was done in Wisconsin in 1912 and Iowa in 1916). This facilitated voting by the illiterate and monitoring of the vote.

³⁸The Republican and Democratic parties in the progressive era did not view their role as that of making controversial issues acceptable to voting majorities. Rather, splinter groups and nonpartisan organizations were responsible for initiating attention to social problems, after which political parties would join the debate if necessary. Consequently, neither party was formally involved in woman suffrage until 1916 nor in prohibition until 1920 (Cashman, 1981, pp.6-10; Morgan, 1972, pp. 129).

³⁹The link between prohibition and suffrage received a fatal blow, however, after American entrance into World War I. With grain in short supply for food shipments, wet interests were literally put out of business. In addition, the super-patriotism generated by the war effort brought antipathy to the German-American community which, as our analysis confirms, had been a primary source of opposition to suffrage (Luebke, 1974). Thus, World War I discredited, if not destroyed, two major opponents to woman suffrage: the wet interests and the influence of German-Americans. Support for the long-moribund federal suffrage amendment skyrocketed.

from the investigation of these suffrage referenda clarify sources of support and opposition to the enfranchisement of women, expanding the study

of American voting behavior to include an issue—woman suffrage—basic to the implementation, if not very definition, of American democracy.

Appendix 1.

I. Suffrage and prohibition referenda: Oregon, California, Ohio, Michigan, Pennsylvania, New York.

		Suffrage	Prohibition
West	Oregon	1910	Prohibition, 1910 ^a
	Oregon	1912 ^a	
	California	1911 ^a	
Midwest	Ohio	1912	County Liquor Licensing, 1912 ^a Prohibition, 1915 Prohibition, 1917 Prohibition, 1918 ^a County Liquor Licensing, 1912 Prohibition, 1916 ^a
	Ohio	1914	
	Ohio	1917	
	Ohio		
	Michigan	1912	
	Michigan	1913	
East	Michigan	1918 ^a	
	Pennsylvania	1915	
	New York	1915	
	New York	1917 ^a	

II. Partisan vote: Oregon, California, Ohio, Michigan, Pennsylvania, New York.

West	
California	1910
Ohio	1916
Midwest	
Michigan	1916
Ohio	1916
East	
Pennsylvania	1914
New York	1914

^aReferendum passed.

Appendix 2. Background Variables

Nationality, Ethnicity and Religion ^a	
English (including Scottish and Welsh)	Methodist
German (including Austrian)	Presbyterian
Italian	Baptist
Irish	Congregational
Scandinavian (including Swedish, Norwegian, Danish, and Finnish)	Jewish
Dutch	Protestant (all Protestant denominations)
Native-born of native-born parents	German X Catholic
Black male	German X Protestant
Roman Catholic	Irish X Catholic
	Irish X Protestant
Economic Indicators	
Population density (the log of population per square mile)	
Percentage employed in manufacturing	
Ratio of number of farms to total population	

^aAs a percentage of the total population.

Appendix 3. Unstandardized Regression Coefficients for Suffrage Support in the Midwest, West, and East: 1910-1918 (Reduced Form Equations)

	Suffrage Vote				
	Midwest		West		East
	1912 ^a Eq. (1)	1913-1914 ^b Eq. (2)	1917-1918 ^c Eq. (3)	1911-1912 ^d Eq. (4)	1915 ^e Eq. (5)
Specification I. Axes of opposition and support variables					
Southern European	1.119	0.695	0.305	1.953**	1.289
Northern European	1.415***	1.467***	1.529***	1.719***	2.403***
Native born	0.238***	0.345***	0.307***	0.219*	0.780***
Black males	-0.108	0.159	-0.189	8.708*	-0.899
Catholic	-0.075	0.085	0.122	0.446**	-0.041
Protestant	-0.529***	-0.191	-0.575***	0.197	-0.638***
Jewish	5.568	13.151	39.915***	2.120	-0.733
Population density	6.715***	1.462	5.005*	-8.601***	-1.394
Farm	188.080***	154.834***	235.743***	184.569***	-74.920
Manufacturing	0.391*	0.346	0.608**	0.155	0.597**
Constant	-0.052	-0.039	-0.071	0.105*	0.015
Adjusted R ²	0.873	0.853	0.875	0.925	0.803
N	171	171	171	92	128
Specification II. Background variables without interaction terms					
Irish	2.507*	2.114	3.607**	1.731*	-0.698
Italian	-0.685	-0.877	-0.871	1.731*	0.338
German	-0.162	-0.107	0.047	2.072	1.379***
English	2.289***	2.635***	1.877	0.770	1.181
Scandinavian	0.986***	0.913***	1.125***	1.016**	2.102***
Dutch	0.329	0.907	0.706***	-26.294*	-0.231
Native born	0.160**	0.309***	0.258***	0.681***	0.769***
Black males	-0.746	0.005	-0.911	4.542	-1.106
Catholic	-0.113	0.110	0.023	0.069	0.288*
Lutheran	-0.372	0.069	-0.503	-7.816*	-1.462***
Evangelical	-1.119**	-0.771	-1.025*	-2.353	-0.177
Episcopal	2.361	3.789**	5.064**	-1.619	-0.377
Methodist	-1.228***	-0.695**	-1.146***	-0.156	0.038
Congregational	-0.443	0.197	-0.587	3.098*	-2.048
Presbyterian	1.119**	1.107**	0.786	2.495	0.394
Baptist	0.024	-0.439	-0.078	-1.790	0.129
United Brethren	-0.882	-0.327	-0.927	-1.133	-0.298
Jewish	10.320	15.503	43.218***	-25.338	0.027
Population density	10.629***	3.411	8.208***	-2.993*	-1.216
Farm	180.950***	154.663***	217.360	-32.430	-162.546***
Manufacturing	0.222	0.266	0.296***	0.422**	0.326
Constant	-0.098***	-0.073*	-0.118***	0.031	0.003
Adjusted R ²	0.922	0.909	0.913	0.958	0.875

^a1913 Michigan pooled with 1914 Ohio.

^b1916 Michigan pooled with 1915 Ohio.

^c1916 Michigan pooled with 1916 Ohio.

^dCalifornia 1911 pooled with Oregon 1912.

^ePennsylvania 1915 pooled with New York 1915.

*Significant at .05 level.

**Significant at .01 level.

***Significant at .001 level.

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Rights, Boundaries, and the Bonds of Community: A Qualified Defense of Moral Parochialism

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One effect of the cosmopolitan turn in recent political philosophy is that widely held beliefs and intuitions are being called into question. My purpose here is to scrutinize one of these beliefs—that we should attend to the needs of our compatriots before the needs of the foreigners—from the perspective of a rights-based theory. After sketching a theory that takes the right of autonomy as its cornerstone, I consider four arguments that might support the intuition that compatriots take priority. Only one of the four is sound, I conclude, and even this argument, the argument from reciprocity, supports the intuition only in a highly qualified form.

Political philosophy seems to have taken a cosmopolitan turn in the last several years, and in some ways this is surely a turn for the better.¹ It is timely, certainly, and there is reason to be encouraged whenever political philosophers devote their attention to timely issues—in this case, issues such as immigration policy, the distribution of wealth and other resources among nations, and the morality of intervention in the (ostensibly) internal affairs of sovereign nation-states. There is also reason to welcome any shift in perspective, such as the cosmopolitan turn, that leads political philosophers to question assumptions and intuitions too often accepted and acted upon without proper reflection. Indeed, my purpose in this essay is to follow the cosmopolitan turn by examining one of these intuitions: the belief, as Henry Shue (1980, pp. 131-132) has put it, that “compatriots take priority.”

To say that compatriots take priority is to say that we stand in a special relationship to those men, women, and children who share with us membership in a political community. This relationship is special because it requires us to attend to the needs and interests of our compatriots before we attend to the needs and interests of

foreigners. We may have some responsibility to others, that is, but our first responsibility is to the poor, the hungry, and the homeless citizens of our own country. In a sense, this is the political analog of the belief that “charity begins at home,” except that the requirements involved, duty and obligation, are more stringent than the requirements of charity.

This view—that compatriots take priority—seems to be both widely and firmly held. But should it be? Perhaps it is only a form of chauvinism or moral parochialism which cannot withstand critical scrutiny. Perhaps an examination of our political and moral relationships from a cosmopolitan point of view may reveal that there is no good reason to give priority to our compatriots.

This seems to be an implication of the human rights perspective which has become prevalent in moral and political philosophy. For if people have rights by virtue of the fact that they are human beings, or persons,² then one may wonder whether any moral significance attaches to the fact that almost all human beings are thought to be members (in one way or another) of political communities (of one sort or another). Insofar as a right is a human right, after all, it must be universal and equal, a right that all persons possess in equal measure; and insofar as a human right entails a correlative duty, it must be a duty that falls

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¹Examples of this turn include: Barry (1982), Beitz (1979), Brown and MacLean (1979), Brown and Shue (1977, 1981), Goldman (1982), Lackey (1982), Richards (1982), Shue (1980), Walzer (1977).

²Cf. Benn (1978, p. 66): “It is a mistake . . . to make the distinction hinge on the difference between *human* beings and others: it is not their humanity, a simple biological characteristic having no necessary moral implications, but their personality that makes the crucial difference between right-bearers and other objects.” Since nothing important to my argument rests on this distinction, and since it seems odd to regard humanity as “a simple biological characteristic,” I do not rely on it in this essay.

equally on all persons. If there is a human right to liberty, for instance, it follows that every person has an equal right to liberty and a correlative duty to respect the liberty of every other person. But this right and duty are universal, so they must apply without regard to race, nationality, or citizenship. By the same token, a human right to life or subsistence recognizes no boundaries, for anyone in need of the means to sustain life would have a claim against those who are in a position to meet his or her needs, even if those who are subject to the claim are citizens of a distant country. Nor is there anything in the bare notion of a human right which would lead one to think that his first responsibility, as far as the protection and promotion of rights is concerned, is to his fellow citizens. In this respect human rights seem to be neutral: the rights of one's fellow citizens are to count for neither more nor less than the rights of others. And neutrality in this respect seems to be hostile to the belief that compatriots take priority.

Does this mean that we must either abandon the human rights perspective or surrender the intuition that we stand in a special moral relationship to our fellow citizens? Not if our rights-based theory of politics is properly conceived. If it is so conceived, I shall argue, a rights-based theory of politics actually requires us to recognize that a special relationship obtains between the members of a political community. This is not to say that no changes are necessary, however, for the intuition in question will have to be qualified by the addition of a *ceteris paribus* clause: that is, compatriots take priority *when other things are equal*. Since other things are seldom equal in these matters, what follows may well be regarded as a qualified defense of moral parochialism.

I

To this point I have referred to "the human rights perspective" as if it were a single, unified point of view. It is not. There are common themes which join all those who consider themselves human rights theorists, such as their opposition to utilitarianism; but there are also significant differences among them, as anyone who compares Robert Nozick's *Anarchy, State, and Utopia* with Ronald Dworkin's *Taking Rights Seriously* may readily attest. These differences are troublesome in the context of this essay because we cannot know, without further exploration, exactly what we are trying to reconcile with the belief that compatriots take priority. For this reason it is necessary to begin with a sketch of what I take to be the most satisfactory account of human rights. The cornerstone of this account is the right of autonomy.

It may seem redundant to speak of the right of autonomy, for "autonomy" is sometimes taken to mean the *right* of self-government. This seems to be what we have in mind when we say that a nation-state is autonomous, for instance. But there is also a stronger sense of the word, a sense that better fits our use of "autonomy" to characterize individual men and women. In this stronger sense, "autonomy" means the ability or capacity to govern oneself—an ability or capacity that someone who is free (from external restrictions) to govern himself or herself may not always enjoy. This is why we say that insane people are not autonomous, for it is not the *right* to rule their lives that they lack, but the *capacity* to do so.³ Thus there is no redundancy, on this stronger sense of "autonomy," in maintaining that men and women have a right, understood as a claim against others, of autonomy. To say that everyone has a right of autonomy, then, is to say that every person is entitled to exercise his or her capacity to govern his or her life—to live, in short, as he or she sees fit.

This does not mean that the right of autonomy is nothing more than the right to be left alone or nothing less than the right to do whatever one wishes. Like other abilities, autonomy is not something we either do or do not have; it is something we may possess to a greater or lesser extent, just as the ability to speak English varies considerably among English-speakers. Autonomy resembles other abilities in another respect, too, for it may be cultivated, neglected, or impaired. Its cultivation demands some effort on the part of the individual, of course, but it also requires the aid of parents, friends, teachers, and others—perhaps even therapists of one kind or another. No one becomes or remains autonomous solely through his or her own efforts; everyone needs the assistance of others at times, and the right of autonomy establishes a claim to this assistance. It is more than merely the right to be left alone, therefore, because it is the right to the protection and promotion of the ability to lead a self-governed life.

The right of autonomy is also less than the right to do whatever one wishes. Because it is a human right, it is a right every person shares with every other person, and this limits what we may do *as a matter of right*. Jones may have the right to live as she chooses, but this does not entitle her to violate others' rights to live as *they* choose. This follows from the logical correlativity of rights and duties. If Jones has a right to do something, then there

³And we usually justify the deprivation of their rights on the grounds that the insane lack the necessary capacity, at least temporarily, to exercise them. •

must be someone who has a duty to assist her, or at least not to interfere with her, when she sets out to do it. When the right in question is one that every person has, furthermore, rights and duties are *completely reciprocal*. By this I mean that every person has both a *right against* and a *duty to* every other person. Thus Jones's right to autonomy implies that everyone else has a duty to respect her choices, but everyone else's right of autonomy implies a correlative duty on Jones's part. Given this complete reciprocity, it is clear that the right of autonomy is no warrant to do whatever one may wish.

This limitation notwithstanding, the right of autonomy is the fundamental right because all others follow from it. Recognizing autonomy as a right requires us to respect the dignity of the person; to treat others not as playthings or objects or resources that we may use for our own purposes, but as individuals who are capable, at least potentially, of forming plans, entering into relationships, pursuing projects, and living in accordance with an ideal of the worthwhile life. Everyone possesses this right simply because he or she is a person, moreover, so the right of autonomy is also a right everyone shares equally. This is not a right to equal treatment, but a right, as Dworkin suggests, to be treated *as an equal*: "the right, not to receive the same distribution of some burden or benefit, but to be treated with the same respect and concern as everyone else" (1977, p. 227).

This is all very general, of course, and one may wonder whether the right of autonomy is too vague, too lacking in content, to be of any practical significance. In the following section I shall try to add enough detail to the sketch of the right of autonomy to remove this worry. Because it is the fundamental right, however, the right from which all other rights derive, there is a certain vagueness which must necessarily remain in any account of the right of autonomy.

II

If autonomy is the fundamental right, then every *special* or *general* right is a manifestation of our human right of autonomy. The right of autonomy must necessarily be formal and abstract, in other words, with special and general rights supplying its content. They do this in different ways. "Special rights," as H.L.A. Hart defines them, are those rights that arise out of special relationships between specific individuals (1970, pp. 68-73). The rights that follow from a promise or contract provide the standard examples. These may be said to manifest the right to autonomy in the sense that they are created through the exercise of that right. Without the right of autonomy, there could be no transfer of

rights in a promise or contract, for we must have a right to govern our own lives before we can grant someone else a right against us. Hence each right we receive from or confer upon others indirectly manifests the autonomy of the person who confers the right.

General rights also manifest the right of autonomy, but they do so more directly. General rights—Jones's right to be free from assault, for instance—are known in legal terms as rights *in rem*: rights that hold against everyone, that is, rather than rights against specific, identifiable persons (rights *in personam*). Because they hold against everyone, general rights do not grow out of special relationships involving two or more people. Instead, they are instances, or direct manifestations, of the right of autonomy. Jones's right to be free from assault does not follow from any pledge or promise she exacts from other people: it is her right simply because the right of autonomy is the right to govern our own lives, and if assault on our persons threatens our ability to be self-governing, then assault violates the human right of autonomy. It follows, therefore, that the right to be free from assault is one instance of the right of autonomy. In this way general rights, whether as widely recognized as the right to be free from assault or as controversial as the right to medical care at public expense, grow directly out of—and along with special rights supply the content for—the human right of autonomy.

This may solve, or at least rationalize, the problem of vagueness, but the solution immediately prompts another question: Just how are we to know what special and general rights follow from the human right of autonomy? Questions of this sort cannot be answered easily, but I believe that we can at least discern the leading features of an adequate response. The point of departure is the right of autonomy itself, which I have defined as the right to the protection and promotion of the ability to lead a self-governed life. From this point we may go on to identify four questions we should raise, or four tests we should apply, whenever we need to decide whether a putative right should be regarded as a special or general manifestation of the fundamental right of autonomy.

First, we should ask whether the "right" in question does indeed protect individual autonomy. The rule of thumb here is that men and women should ordinarily be left to decide what is in their own interests.⁴ Autonomy is the ability to

⁴I say that men and women should ordinarily be left to decide what is in their own interests to leave open the possibility of paternalistic intervention to protect and promote the autonomy of an individual. As "ordinarily" suggests, however, the burden of proof must

govern one's life, and the direction of that life should be left to the individual as long as he or she respects the autonomy of others. This means that we must ordinarily respect the choices of individuals, including the choices they make when they confer rights on, and thereby undertake obligations to, other men and women. Special rights thus present no problem, not even in those cases where one party to a special relationship is defrauding, exploiting, or coercing other parties, for in these cases the fraud, exploitation, or coercion violates the victim's right of autonomy and prevents any special right or obligation from arising from the relationship. Some general rights will also fall under the heading of rights that protect our autonomy, with the right to freedom from assault once again providing a clear example. Other general rights, such as the right to speak freely, also follow from the right of autonomy, although these rights will often prove troublesome when their exercise threatens to violate the rights of others. But trouble of this sort cannot always be avoided.

The second test goes beyond the first because it leads us to ask whether a putative right will actually promote, not merely protect, individual autonomy. If it will, then it ought to be regarded as a right, positive and general, which flows directly from the fundamental right of autonomy.³ The stronger sense of "autonomy" as the ability to govern one's life seems to require positive rights of this sort. This point is illustrated by the claim that members of linguistic minorities often make to the right to speak a particular language. When these people—the Quebecois, say, or Hispanics in the United States—lay claim to this right, they ask for more than protection against those who might wish to prevent them from speaking French or Spanish; they ask also for bilingual education and other forms of positive aid which will help them to sustain the use of their language. In this fashion the right of autonomy, through such general rights as the right to speak a particular language, imposes a duty of assistance on those who are in a position

to encourage, cultivate, or foster the autonomy of other men, women, and children. Since the right of autonomy is a human right, moreover, it is characterized by what I have called complete reciprocity: every person has a right to the assistance of others in projects that will promote his or her autonomy, and every person has also a reciprocal duty to provide similar assistance to those who will benefit from it.

This begins to sound as if we must devote our entire lives to service to others. This is not the case—indeed, I shall note shortly some limitations on what may be demanded in the name of autonomy—but there is no denying that this is an activist conception of human rights. If we are truly concerned with the autonomy of the individual, however, such a conception is inescapable. Autonomy admits of degree, and this means that the right of autonomy is a right to widen one's range of choice in order to gain greater control of one's life. But rights also entail duties, so those who are in a position to promote the autonomy of others will share a duty to help widen the right-holder's range of choice. A healthy person has a wider range of choice than someone who is unhealthy, for instance, so it seems reasonable to say that, other things being equal, the healthy person is more autonomous, or in greater control of his or her life, than one who is ill. Hence the right of autonomy seems to entail a general right to health care, a right that holds against anyone who is able to help provide this care. In much the same way an educated person usually commands a wider range of choice than the uneducated man or woman, and this again seems to imply a right to schooling of some sort, even if this right imposes certain costs on those who are in a position to help provide the schooling. Thus we may conclude that the rights to health care and schooling pass the second of the four tests because they are among the rights that promote individual autonomy. Rights of this sort also exemplify the activist aspect of a theory of human rights which takes the right of autonomy as its cornerstone.

Even an activist theory has its limits, however, and these are expressed in the remaining two tests. The third test recognizes that a project or action that promotes or protects the autonomy of some individuals may well narrow the range of choice for others. The question we must pose, then, is whether the "right" at issue, assuming it either protects or promotes someone's autonomy, infringes or violates the rights of others. If it does not, then we may pass on to the fourth test. If it does entail a conflict of rights, we must then decide whether the proposed right ought to override a right already recognized. Something of this sort happens with both the right to health care and the right to schooling, for both may come

fall on those who call for intervention. On this point see Dworkin (1971) and Goodin (1982, especially chap. 3).

³According to Hart (1970, p. 72), general rights are always negative: "In contrast with special rights . . . are general rights, which are asserted defensively, when some unjustified interference is anticipated or threatened, in order to point out that the interference is unjustified." As my argument indicates, I believe that some general rights may be positive rights as well. For a forceful challenge to the validity of the distinction between positive and negative rights, see Shue (1979 and 1980, especially chap. 2).

into conflict with the right to live one's life as one chooses. Acknowledging a general right to health care or schooling will almost certainly require a diversion of resources from some people to others, and there is some truth to the claim that the person who is required to surrender some of his or her wealth for these purposes suffers a loss of autonomy; certainly the transfer of wealth can diminish one's range of choice. How, then, can we resolve this conflict?

There are at least two promising ways to reach a resolution when a conflict of this nature arises. The first is to establish either a floor or ceiling for autonomy. We might say, in other words, that a transfer of resources is just whenever it helps to bring one or more persons up to a certain standard of autonomy—that is, a condition where the range of choice is at least wide enough to begin to resemble true autonomy. Or, conversely, we might consider a transfer to be just when it does not lower those who must surrender resources below a certain level which seems as high as we can reasonably go in the name of autonomy. In the first case, the man or woman who lives in poverty, with little chance for decent food, schooling, or medical care, is able to make some choices; but these are so limited that we are hardly likely to say that he or she is truly autonomous. In the second case, the person who must settle for a Mercedes-Benz because he or she can no longer easily afford a Rolls Royce may suffer a diminished range of choice, but the diminution hardly represents a real loss of autonomy. Autonomy admits of degree, to be sure, and it is related to the range of choice we enjoy in governing our lives. But this is not to say that every change in our range of choice represents a notable extension or diminution of autonomy. In this respect, additions to our range of choice exhibit some of the features of diminishing marginal utility. This is why the floor/ceiling approach may well provide a satisfactory method for resolving conflicting claims with respect to individual autonomy.

The second approach involves the application of something like John Rawls's difference principle to the right of autonomy (1971, esp. pp. 60-82). In this case the intent is to promote the autonomy of the least autonomous people, even if this comes at the expense of those who enjoy greater autonomy. Following this approach, the ideal is to insist on providing everyone with as nearly equal a range of choice as possible unless there is reason to believe that an unequal distribution would somehow extend the range of choice for the least autonomous beyond what they would be likely to achieve with an equal distribution. Applying this principle to wealth, we might say that an unequal distribution is just *only* if such a distribution somehow provides those at the low

end of the distribution with a broader set of choices, perhaps through incentives and trickle-down effects, than they would enjoy if wealth were divided equally.

These proposals are nothing more than tentative suggestions, but they do indicate how conflicting claims arising out of the fundamental right of autonomy might be resolved. This brings us, finally, to the last of the four tests, and the second limiting test, for the special and general rights that supply the content for the right of autonomy. Here we must employ a variant of the maxim "ought implies can" and ask whether a putative right can indeed be realized.⁶ If it can, and if it passes the other tests, then it should be acknowledged as a right deriving from the right of autonomy. If it cannot, even if it passes the other tests, it must be dismissed. The ability to fly by flapping one's arms might enhance one's autonomy—certainly it would add to a person's range of choice—but it would be silly to claim this as a right for the simple reason that we know of no way to cultivate this particular ability. Or, to take a more serious example, we cannot reasonably acknowledge a right to health, but only to health *care* of the best sort available. Which is to say that we cannot recognize a right unless we have the means to perform its correlative duty or obligation.

This conclusion implies that the rights we may acknowledge as genuine instances of the fundamental right of autonomy will vary from place to place and time to time. Everyone possesses the right of autonomy, that is, and everyone may also share some (very) general rights, such as the right to be free from unwarranted assault, but nesting within these rights will be a number of more specific rights which will differ from one set of circumstances to another. Thus it may be reasonable to maintain that the residents of the United States have a right, say, to the use of a hemodialysis unit because this particular right derives from the general right to health care, which derives in turn from the fundamental right of autonomy. We could not have asserted this right 100 years ago, though, nor can we attribute this right now to the millions of people throughout the world who have no access to hemodialysis units

⁶On this point see Flathman (1976, especially p. 75): "*rights* is governed by a rule similar to that which states that *ought* implies *can* and *obligation* implies *ability*. An action or possession can be a right only if there is some *A* capable of serving as the holder of that right and only if there is some *B* capable of discharging the obligation(s) respecting the no-rights, disabilities, and so on, that *A*'s right to *X* imposes. Men and things being what they are, the notion of a right to jump fifty feet straight up is nonsensical."

(although we should probably strive to provide them with this access). Cultural and economic conditions, available technology, the institutions under which we live, even geography, all these will help to determine the particular rights to which we may be entitled under the right of autonomy. Indeed, these various circumstances will even affect the kinds of claims men and women put forward as rights.

There is a sense, then, in which most of the specific rights we enjoy are social in character. All moral rights presuppose the right of autonomy, directly if they are general rights and indirectly if they are special rights. But what counts as protecting or promoting autonomy cannot always be determined without reference to the specific circumstances of specific men, women, and children. When we refer to these circumstances to decide whether a putative right passes the fourth test, we are acknowledging that the human right of autonomy may appear in different guises under different conditions. If this is so, then there is some reason to believe that an adequate theory of human rights will not be so cosmopolitan in outlook that it is entirely hostile to the intuition that compatriots take priority. Whether this belief is well-founded is the question I shall now try to answer.

III

At the beginning of this essay I asserted that compatriots should take priority over foreigners *when other things are equal*. Now I shall defend this assertion. This may best be done by considering the different kinds of arguments that might be mustered on behalf of the intuition in question: arguments from necessity, efficiency, side effects, and reciprocity.

The *argument from necessity* rests on what some regard as a psychological truth about human beings. Samuel Gorovitz puts the point neatly when he says that "one defense of what might be called our moral parochialism is the claim that it is a psychological necessity; that the scope and complexity of the world's population of individuals is such that a person would likely become dysfunctional in attempting to deal with more than a restricted subset—indeed, a very carefully restricted subset—of that population" (1977, p. 140).⁷ We might conclude, then, that compatriots take priority because human beings are too limited to cope with a wider range of respon-

sibilities than that defined by national boundaries. Any attempt by individuals to undertake to provide aid to everyone who needs it would not only fail, it would also destroy the persons, or at least the personalities, of those who make the attempt. In short, compatriots should take priority for the simple reason that our psychological limitations will not allow us *not* to grant them priority.

We need not dismiss this view of human psychology in order to reject the argument from necessity. For even if we grant that "a person would likely become dysfunctional in attempting to deal with more than a restricted subset" of the world's population, we cannot simply assume that this subset conveniently comprises one's countrymen. Many countries are now so large that it seems likely that anyone who seriously attempts to identify and sympathize with all his or her compatriots will be rendered every bit as dysfunctional as the person who takes on the burdens of the entire world. There may be some truth to the claim that psychological necessity requires us to restrict our concerns to a subset of the world's population, but perhaps this subset consists of the state, city, town, or neighborhood. We will, in any case, need an independent argument demonstrating that the boundaries of the nation-state also set the boundaries of this subset before psychological necessity has a part to play in this context—and I doubt that such an argument can be found.

Another reason for rejecting the argument from necessity is that it seems to assume that we must actively identify and sympathize with others before we can recognize that they may have a right to our aid. Although we may be more likely to go to the aid of those with whom we strongly identify and sympathize, I do not believe that we can or will acknowledge a duty to aid others *only* when we feel a bond of this sort. We know that there are men, women, and children in this world who are living, and dying, in desperate poverty. We also know that we can take some steps to help them. And if my account of the human right of autonomy is correct, we know, too, that we have a responsibility to go to their aid. Yet all that this requires in the way of identity and sympathy is the simple recognition that these people have a right, as human beings, of autonomy. Certainly we may feel some anguish when we consider the plight of these unfortunate people, or some distress at the thought of what we may have to give up in order to help meet their needs, but this is hardly likely to render us dysfunctional. Given this, the argument from psychological necessity seems rather beside the point.

If the argument from necessity will not justify the belief that compatriots take priority, perhaps the *argument from efficiency* will. Here the claim is that we ought to aid the needy among our

⁷I should note that Gorovitz is referring only to individual responsibility and actions here; he goes on to say that the situation "appears quite different" at the level of the national government.

fellow citizens before we look to the needs of foreigners simply because this is the most efficient way to meet human needs. We can satisfy more needs at less cost when we begin at home, as it were. It costs less to supply food, shelter, and medical care to the needy within the United States than to supply the same goods to the people of Bangladesh or Ethiopia, so we will provide the most aid for our money if we first take care of our countrymen.

There is something to be said for efficiency, surely, and other things being equal we should prefer an efficient way of achieving a goal to an inefficient one. This notion is not enough to lead us to conclude that compatriots take priority, though, for in some cases it may be more efficient to aid foreigners than fellow citizens. For instance, if I know that there are people who need clothing in Newark, New Jersey, and others with similar needs in Nogales, Mexico, and if I have some clothing that is suitable, mere efficiency may require me to take or send the clothing the 180 miles from my home to the people in Mexico. Or if I am traveling in a foreign country and come across people who need aid that I am able to give, efficiency would hardly require me to find some compatriots whose needs I might satisfy first. As these examples demonstrate, the argument from efficiency does not accord any special status to our fellow citizens, nor does it recognize that we stand in a special relationship of any sort to them. Propinquity seems to be all that matters.

And even propinquity is not enough. If we assume, that is, that as a matter of fact we may more efficiently attend to the needs of our countrymen than to those of men and women in some distant land, we must still go on to assess the extent and severity of the various needs. To take a very simple example, we might face a situation in which we may provide a hearing aid for a compatriot *or* provide food for a month for a starving child in a foreign country. The argument from efficiency presumably requires us to balance the good we can do against the costs we incur while doing it, and in this case it seems that the compatriot would not take priority. Important as it is, efficiency is not enough to establish the priority of compatriots.

Another consequentialist argument, the *argument from side effects*, may come closer to providing a justification for the belief that our first responsibility is to those who share membership with us in a political community. This is because the consequences taken into account here are broader than the argument from efficiency allows. The argument from efficiency requires us to identify our goal—providing aid to those who have a right to it, in this case—and then to take the course of action that will reach that goal at the

least cost. The argument from side effects, however, asks us to consider what the indirect consequences of our actions will be. If these prove to be valuable, then we may have reason to follow a course of action which appears to be less efficient, in terms of the original goal, than another course of action which produces side effects of less value. In the present context, this might mean that we should grant priority to our compatriots because doing so leads not only to the fulfillment of some vital needs, but also promotes solidarity, fraternity, or the sense of community. In caring first for our own, we may help to strengthen the bonds of community by affording special recognition to our fellow citizens. Acting as if shared citizenship and mutual aid are important, in other words, may actually help to make them so. Anyone who wishes to foster the sense of community will thus have a reason to believe that compatriots should take priority.

For those who wish to promote the sense of community, the argument from side effects is somewhat attractive. But it is only *somewhat* attractive because it provides a reason for according priority to our countrymen only under certain limited conditions. To begin with, we must have grounds for believing that the policy of attending first to the needs of compatriots will actually help to strengthen the bonds of community, or produce other valuable side effects. Assuming that the policy does this, we have next to consider whether these side effects might not be outweighed by the positive side effects of a different policy, such as the policy of providing aid to as many people as we can without regard to their nationality or citizenship. Someone who takes a cosmopolitan view might maintain that a policy of the latter kind will promote feelings of universal brotherhood and thus combat chauvinism, jingoism, and racism, for instance. Since the argument from side effects is a consequentialist argument, the case cannot be settled in favor of our compatriots until all the side effects are taken into account and balanced against one another, and I see no reason to expect that compatriots will always win this test of consequences. If they do win, one may still object that these positive side effects simply do not deserve to be weighed in the balance with the fulfillment of human rights. Solidarity and community may be good things, on this view, but rights always take priority, and meeting the needs of starving people must take priority over goods to which we have no right, no matter how desirable these other goods may be.

Both efficiency and side effects count, to be sure, and both may lead us to conclude that *in certain circumstances* we ought to grant priority to our compatriots. But both arguments are strictly contingent. They tell us that we ought to take care

of our fellow citizens first not because our fellow citizens have a right to this priority—not because they stand in a special relationship to us—but only because this policy may sometimes be the way to do the most good. For a stronger justification, we shall have to look to the *argument from reciprocity*.

Stated simply, the argument from reciprocity holds that we should grant priority to our compatriots because, *ceteris paribus*, we owe it to them. Why? Because there is a special relationship, entailing special rights and obligations, between those who share citizenship in a political community. All human beings have general rights against and corresponding duties to all other human beings; this follows directly from the right of autonomy. But we also enjoy special rights and incur special obligations when we enter into special relationships, such as those created by a promise or by participation in a cooperative enterprise. The parties to these relationships are doubly bound, so to speak, for they are related to each other both as human beings, with general rights and duties, and as participants in a special relationship, with special rights and obligations. Although they may be overridden by more compelling moral considerations, the rights and obligations that follow from these special relationships normally take priority over others. In the case at hand, this means that we can justify the intuition that compatriots take priority if we can show that compatriots do indeed share this kind of special relationship.

One way to do this would be to demonstrate that the men and women who are the citizenry of a country have somehow consented to enter into a special relationship, thereby gaining rights against and undertaking obligations toward one another. But the telling criticisms of the different forms of consent theory seem to doom an effort of this sort. A more promising alternative is to argue that the members of a body politic stand in a special relationship to one another because they are engaged in a cooperative enterprise.

To say that a political community is, or can be, a cooperative enterprise is to draw attention to two of the features it may exhibit. First, a political order typically provides benefits for those who belong to it, for it enables them to work together for common purposes and pursue in peace their private interests. If nothing else, the rules established and enforced by the body politic make it possible for citizens to coordinate their affairs and order their lives with a confidence that would be utterly inconceivable in a state of nature, whether it be Hobbes's brutish state of war or Rousseau's brutish state of peace. These benefits are provided, moreover, through the cooperation of the citizens, which is the second feature of the

political community *qua* cooperative enterprise. Their ability to form and act on expectations about the conduct of others—the expectations we rely on when driving, for example—may depend upon the rules of the political order, but these rules would be useless if the members of the body politic did not cooperate with one another by obeying them. Obedience need not be unanimous, of course, but the political order will cease to provide benefits, and perhaps cease to exist, when its rules are widely disobeyed.

When the political order does constitute a cooperative venture of this kind, everyone who joins in that venture incurs an obligation *to the others* to bear a fair share of the burdens of the enterprise in exchange for a fair share of the benefits, which means that every member of the enterprise is under an obligation, other things being equal, to obey the laws of the body politic. Because rights and obligations are correlative, those who take part in the enterprise have rights against as well as obligations to one another. We may all have an obligation to one another to drive on the right-hand side of the road, for instance, and if we do, we must also have a right to require one another to do the same. In this way there is a reciprocity between the cooperating members of a cooperative enterprise. We have, in these circumstances, an obligation to obey the law because we owe it to the cooperating members of the body politic to do so. They have a right to our obedience because they, through *their* obedience, enable us to enjoy the benefits of the political order. They have a claim on us, in other words, as long as the body politic may reasonably be regarded as a cooperative enterprise, a claim that extends to include the notion that compatriots take priority.

This is how reciprocity may require us to grant priority to compatriots; at least, this is a glimpse of how reciprocity imposes a requirement of this kind on us. To see beyond this glimpse, we shall have to examine the argument for political obligation which provides the foundation for the argument from reciprocity. This argument, and the whole conception of rights and obligations arising out of cooperative enterprises, rests on the principle of fair play.

IV

According to the principle of fair play, anyone who takes part in and enjoys the benefits of a cooperative practice must contribute to the production of these benefits, even when his or her contribution is not necessary to their production. To fail to contribute is to act unfairly, for the person who shares the benefits without sharing the burdens is taking advantage of those who

cooperate. Thus the principle of fair play demands what Hart calls a "mutuality of restrictions": "when a number of persons conduct any joint enterprise according to rules and thus restrict their liberty, those who have submitted to these restrictions when required have a right to a similar submission from those who have benefited by their submission" (1970, p. 70).⁸

This principle seems to have an intuitive appeal which is both broad and deep. Whether it is sound, however, is a matter of some dispute, with critics raising three objections to it. The first, advanced by Robert Nozick, is that the principle of fair play implies that others can place us under an obligation to them simply by conferring benefits on us. If this is correct, then the principle allows others to foist obligations on us, which is quite a departure from our usual way of thinking about obligations. As Nozick puts it (1974, p. 95), "You may not decide to give me something . . . and then grab money from me to pay for it, even if I have nothing better to spend the money on."

Nozick's point is surely correct, but is it pertinent? Fair play does not require us to reciprocate *whenever* someone does something that is to our benefit; it applies only to the benefits of a cooperative practice. But Nozick sees the same problem arising within such practices because it may sometimes be impossible to avoid the benefits of cooperative enterprises. This seems to be what he has in mind when he offers the following example.

Suppose some of the people in your neighborhood (there are 364 other adults) have found a public address system and decide to institute a system of public entertainment. They post a list of names, one for each day, yours among them. On his assigned day (one can easily switch days) a person is to run the public address system, play records over it, give news bulletins, tell amusing stories he has heard, and so on. After 138 days on which each person has done his part, your day arrives. Are you obligated to take your turn? (1974, p. 93)

The answer, Nozick says, is "surely not," and it is difficult to disagree. But his claim, by implication, is that anyone who accepts the principle of fair play would have to answer affirmatively. Again, it is by no means clear that this is true, for those who accept the principle may say that the "you" in Nozick's example is under no obligation

to take a turn as broadcaster. To see why this is so, we need only look to the principle as Nozick, following Rawls, formulates it:

This principle holds that when a number of persons engage in a just, mutually advantageous, cooperative venture according to rules and thus restrain their liberty in ways necessary to yield advantages for all, those who have submitted to these restrictions have a right to similar acquiescence on the part of those who have benefited from their submission. Acceptance of the benefits (even when this is not a giving of express or tacit undertaking to cooperate) is enough, according to this principle, to bind one. (1974, p. 90)

Nozick obviously believes that his example meets the conditions set out here. But one may wonder, who has *engaged* in this venture? Perhaps those who found the equipment and began the broadcasts have engaged themselves, but it is far from evident that anyone else has, or that anyone has a right to "assign" a turn at the microphone to others.

The problem here is that Nozick interprets "acceptance of benefits" too broadly (Bell, 1978; Simmons, 1979, pp. 118-136). Merely receiving the benefits of a practice, especially when one cannot avoid them, is not enough to place one under an obligation to comply with the rules of the practice. There must be some sense in which one takes part in the enterprise or leads those who are participating to believe that he or she is taking part in it. Only in these circumstances can we say that someone is obligated to do his or her part. Indeed, it is only in these circumstances that we can sensibly talk about one's *part* in a practice.

We cannot conclude from this, however, that the principle provides the ground of an obligation to obey the law, for now we face a second objection. If we acknowledge that one cannot incur an obligation to cooperate simply by receiving benefits from a practice, we must then go on to show how those who find themselves in a political society may be said to participate in it or voluntarily accept its benefits. But how can we say that any but a few are voluntarily participating in the political society to which they ostensibly belong? (Simmons, 1979, pp. 136-142).

There seems to be two ways to respond to this objection. The first, suggested by Richard Arneson, is to argue that in some circumstances, including those that characterize the body politic *qua* cooperative enterprise, we may acquire obligations under the principle of fair play even when we do not voluntarily accept the benefits of a cooperative enterprise. In these circumstances the political order provides *pure* public goods, and where pure public goods are supplied,

⁸See also Murphy (1979, p. 77): "In order to enjoy the benefits that a legal system makes possible, each man must be prepared to make an important sacrifice—namely, the sacrifice of obeying the law even when he does not desire to do so. Each man calls on others to do this, and it is only just or fair that he bear a comparable burden when his turn comes."

Arneson maintains, "voluntary acceptance of benefits is impossible and so unnecessary to generate obligations according to the principle of fairness. Mere receipt of benefits may suffice to obligate" (1982, pp. 620-621).

The second response is to challenge the strict conception of "voluntary acceptance of benefits" which underlies the objection. "Voluntary" and "involuntary" are not clear opposites, as J.L. Austin has demonstrated, and it seems foolish to insist that everything we do must be either (and clearly) voluntary or involuntary (Austin, 1970, esp. p. 191). When we say that obligations (if not duties) must be undertaken voluntarily, we need not mean that the man or woman who undertakes the obligation must say, or even think, "By doing this I am undertaking an obligation." If we take "voluntarily" to mean something like "with full awareness and conscious intent," we rule out many cases in which we attribute obligations to individuals who have not deliberately—i.e., *with actual deliberation*—incurred those obligations. We hold our students accountable to codes of conduct, including rules against plagiarism, for example, although many of them apparently do not know what these codes require when they matriculate. Yet we could not do this if we restricted obligations to those that are taken with full awareness and conscious intent.

The alternative is to say that "voluntarily" should be taken to mean something on the order of "not under constraint or duress" (Austin, 1970; Fitzgerald, 1961; Murphy, 1981), which recognizes that students, or anyone in similar circumstances, are obligated to obey the relevant rules despite their failure to proclaim, or perhaps even to reflect upon, their acceptance of the obligation. Hard cases will arise, certainly, and we will then find it difficult to determine whether an action should be considered voluntary. So far as obligation is concerned, however, the only way to avoid this problem is to limit obligating actions to express statements of consent, promises, contracts, and so on, and this is consistent with neither ordinary use of "voluntary" nor common practice.

Concerning political obligation we can say that someone acquires an obligation to obey the laws of the political order, other things being equal, when he takes advantage of the opportunities it offers to further his interests. This is enough to constitute the acceptance of benefits required by the principle of fair play. Since there are many of these opportunities when the body politic is a cooperative enterprise, there are also many ways in which the obligation can be incurred: using public roads and facilities, entering into contracts secured by law, soliciting aid from the government, owning property, and attending public

schools are examples. In each of these ways, and in many others that characterize the intricate web of relationships we call a body politic, one may take advantage of opportunities to further his or her interests—opportunities that exist only because of the cooperation of others. And in each of these ways one may also acquire an obligation to these others to share the costs of cooperation. These costs may take a variety of forms, but in a political society based on the rule of law they reduce to one fundamental restriction on our liberty: the obligation to obey the law. For this is what one owes to those whose cooperation and obedience make the rule of law possible.

This brings us to the third objection: that "the obligation of fair play governs a man's actions only when some benefit or harm turns on whether he obeys" (Smith, 1977, p. 111). According to M. B. E. Smith, who raises this objection, if an enterprise is small enough that any participant's failure to obey the rules could reasonably be expected to harm it—perhaps "by diminishing the confidence of other members in its probable success and therefore reducing their incentive to work diligently towards it" (1977, p. 111)—then everyone involved in the enterprise will have an obligation, stemming from fairness, to comply. But this reasoning will not support a political obligation, Smith charges, for one can readily imagine cases where someone's disobedience neither deprives anyone of any benefits nor harms the political enterprise in any noticeable way. If we accept Smith's claim as to when fairness is and is not a moral consideration, consequently, we must also accept his conclusion—that fairness cannot require us to obey the laws of a state.

Smith presents three hypothetical cases to develop and support his position. In the first case *A*'s compliance with the rules of a practice "will confer on *B* a benefit roughly equal to those he has received from *B*"; in the second, *A*'s failure to comply will harm the practice and thus harm *B* indirectly "by threatening the existence or efficient functioning of an institution on which *B*'s vital interests depend" (1977, p. 111). In both cases, Smith says, fairness demands that *A* abide by the rules. But this does not hold for the third case, where *A* is simply a free rider, for "if *A*'s compliance with some particular rules does not benefit *B* and if his disobedience will not harm the community, it is difficult to see how fairness to *B* could dictate that *A* must comply. Surely, the fact that *A* has benefitted from *B*'s submission does not give *B* the right to insist that *A* obey when *B*'s interests are unaffected" (1977, p. 111).

The problem with this argument is that it rests on an unduly narrow conception of fairness. As Smith sees it, one person cannot be unfair to another without directly or indirectly affecting the

latter's interests. But this is simply wrong, as the familiar example of the tax cheater attests. If he is like most citizens of modern states, the cheater's taxes, if he paid them, would constitute an insignificant portion of the state's total tax revenue. This being so, he knows that his failure to pay his taxes will neither bring about the downfall of the political order nor put his favorite programs in jeopardy, nor even affect any of his fellow citizens' interests in any perceptible way. Given these circumstances, and given his notion of fairness, Smith would have to say that the cheater is not acting unfairly. That this judgment is counterintuitive—indeed, the charge typically brought against free riders is precisely that they are acting unfairly by taking advantage of others—demonstrates clearly that Smith's argument rests on a misconception of fairness.

What Smith overlooks is the consideration that underlies the notion of fairness, the conviction that everyone involved in a practice is to be treated as an equal. This holds even when the interests of the parties involved are not affected, for one may be wronged (deceived, treated unfairly) without being harmed.⁹ Those who refuse to cooperate in a practice while they accept its benefits are acting unfairly and wronging those who do cooperate, because they are making exceptions of themselves. They want others to cooperate, or at least enough others to preserve the practice, but they are not themselves willing to bear the burdens of cooperation. By according themselves this special treatment and by exploiting the cooperation of others, they betray a lack of respect for other persons. They are, in Kant's terms, using others as means to their own ends rather than treating them as ends in themselves—as men and women, that is, with a right of autonomy.¹⁰

In short, fairness imposes an obligation on those who engage in cooperative ventures; thus the principle of fair play can provide an adequate foundation for a general obligation to obey the law which rests indirectly on the moral premise that everyone is entitled to equal concern and respect. What remains is to connect this principle with the intuition that compatriots take priority and then to enter some important qualifications.

V

Compatriots take priority because we owe it to them as a matter of reciprocity. Everyone, com-

patriot or not, has a claim to our respect and concern—a claim founded on the right of autonomy—but those who join with us in cooperative enterprises have a claim to special recognition. Their cooperation enables us to enjoy the benefits of the enterprise, and fairness demands that we reciprocate. When the body politic may reasonably be regarded as a cooperative practice, then, we must accord our fellow citizens a special status, a priority over those who stand outside the special relationship constituted by the political enterprise.

In this way the argument from reciprocity supports the intuition that compatriots should take priority. As I have already noted, though, this intuition must be modified by the addition of a *ceteris paribus* clause before it can be accepted. This is not a trivial modification, for the intuition in question holds only with three significant qualifications. The first is that the argument from reciprocity applies not only to the political order, but to *any* cooperative enterprise. This being the case, we may encounter situations where members of two (or more) cooperative practices may have claims against us, and it is hardly obvious that the claim of the fellow citizen must take priority over the claims of a foreign colleague in one's church, for instance, or one's union or business venture.

The second qualification may be even more serious. For if the belief that compatriots take priority rests on the extent to which a political community may reasonably be regarded as a cooperative enterprise, then there are likely to be some ostensible political communities that fall quite short of the mark. Where violence and terror take the place of the rule of law, we may well conclude that the "citizens" of a particular "body politic" have no special rights against or obligations to one another *qua* citizens. There may be good reasons in this situation for obeying the commands of those in power, but reciprocity or fair play will not be among them. Even where the rule of law seems to prevail, furthermore, the distribution of resources and opportunities among the members of a political community may indicate that their relationships are marked not by reciprocity, but by exploitation. When access to property or wealth or positions of political power is effectively denied to some citizens, those without access will have little choice but to labor for the benefit of those who dominate their lives, hardly the hallmark of a cooperative enterprise that gives rise to special rights and obligations among its participants. There is a critical edge to the argument from reciprocity, it seems, an edge that suggests that compatriots in many cases simply have no moral claim to priority.

We must also qualify the intuition, finally, by noting that the priority we give to the rights of our

⁹For the distinction between wronging and harming, see Dagger, 1982.

¹⁰For similar views of fairness, see Sullivan (1975, especially p. 328) and Lyons (1965, especially p. 177).

compatriots is not absolute. Granting the priority of our compatriots' claims, that is, does not entail that we must always honor their claims rather than those of someone who is not bound to us in a special relationship of one kind or another. We should grant our compatriots priority in the sense that we look first to their claims—to what we can do to protect and promote their autonomy. But there may nevertheless be excellent reasons to decide to neglect a compatriot's claim to our aid if we must do so in order to meet a foreigner's. Here, once more, the chief consideration is the severity or extent of the deprivation in question. If we face a hard choice between devoting resources to the attempt to promote the autonomy of some of our countrymen by providing them, say, with opportunities for physical therapy, or to the at-

tempt to promote autonomy by supplying food to starving people in a distant land, then we probably ought to decide in favor of the starving strangers.

For these three reasons, we must conclude that compatriots take priority, but only when other things are equal. Still, this modification leaves the core of the intuition intact. This is significant, in my view, because it means that the adoption of a human rights perspective does not force one to hold that the bonds of political community count for little or nothing. Indeed, if the arguments marshalled in this qualified defense of moral parochialism are correct, our task should be to understand and to cultivate the place of community within a concern for human rights.

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A Biochemical Property Relating to Power Seeking in Humans

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The disposition to seek power in a social arena is tied in this research to a biochemical marker, whole blood serotonin. This finding constitutes the first systematic evidence of any biochemical property in humans which differentiates power seekers from others. The disposition itself is given empirical content with the use of measures of three components of the Type A behavior pattern—aggressiveness, competitiveness, and drive—and of distrust and self-confidence. The statistical fit with serotonin is very good. This discovery echoes similar findings in a species of subhuman primates.

Power seekers are different.¹ Surely few students of politics would assume otherwise. But different how? It will not do to answer that the difference lies in a special attraction to or need for power. That is true by definition. It is by the strength of their desire to dominate and control others that we recognize power seekers. So we must ask what else differentiates them from other people? The answer seems to be that we don't know. Dahl, after scrutinizing the relevant literatures, offers what appears to me an uncontroversial conclusion: "Whatever the reasons may be, some people do seek power more intently than others [but] scientific knowledge about the per-

sonalities and motives of power seekers is still scanty" (Dahl, 1976, p. 116).

This ignorance plainly represents a fundamental void in behavioral political science. After all, we are here dealing with—as Lasswell (1948, p. 19) put it—"political man, *homo politicus*, a basic political type." Understanding the inner drive of those who devote themselves to the capture and use of power roles was for Lasswell central to the understanding of politics. Perhaps most students of politics would agree. But, as noted above, progress in understanding that drive, and in understanding the contextual circumstances that influence it, has been slight.

This research report speaks to that void. Evidence is presented below which suggests that power seeking, or possibly *successful* power seeking, has a biochemical marker, namely whole blood serotonin. Serotonin is a neuroregulatory agent already known to be intimately involved in dominance relations in certain subhuman primates. It is shown here to be strongly tied to a syndrome of human behaviors and orientations characteristic of a drive for power.

This discovery, mysterious in many ways, gives Lasswell's "political type" the beginning of a neurochemical profile. It offers political scientists an important new tool in the behavioral study of influence and the influential, and relatedly, it provides a research bridge from behavioral political science to the neurosciences, perhaps the first such bridge built on a foundation of systematic empirical results instead of untested hypothetical argument.

The report is organized in four sections. In the first, previous research is discussed, with special attention given to the biochemical studies of dominance in the aforementioned primates. In the second, the methods used in the present project are described. The third section presents findings. Finally, there is a discussion of those findings and of possibilities for future studies.

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¹Fortunately, it is not necessary for me to get into the conceptual morass surrounding the term *power*. The only importance of the term here is in describing a circumstance sought with special intensity by some individuals. That circumstance could as well be described as great influence or dominance.

Studying Power Seeking

Although political science is interested in power seekers because of their presumed impact on the nature and conduct of government, it is important that behavioral research on power seekers not be recast as research on the incumbents of potentially powerful government roles. There are three reasons for this. First, all social organizations, not just the general or public governments of broad political communities, have within them a value allocation process (even if not deemed fully authoritative). Hence, all social organizations have power roles—that is, roles that permit their incumbents to exercise disproportionate influence over the allocation process. This is obvious in private corporations and in armies, but it is also true of social cliques and street gangs. Indeed, for most people almost all struggles for influence will take place in nongovernmental arenas, for example, in school, in the workplace, and in social arenas more generally. It might seem easy to dismiss this, to view as trivial the influence that can come from these everyday routes and contrast it with the influence that can come to those at the pinnacle of government. Yet in terms of either attitude or behavior, there is no reason at this point to assume important differences with respect to those seemingly lesser struggles.

Second, if it is true that not all power seekers are public governors, it is equally true that not all public governors are power seekers. Those who fill political roles for which no struggle was required, for example, hereditary monarchs or legislators from uncontested electoral districts, may well lack a power-seeking disposition.

Finally, understanding power seeking will require that we understand its limits. How does the power seeker set boundaries for his ambition? When will the big frog venture from his small pond in search of greater gain? We know that complex societies have nested hierarchies—that is, smaller power games within larger ones, which are themselves within still larger ones. Understanding movement across games will require examination of power seeking at all levels.

In thinking about behavior it is often useful to employ the three-part scheme made famous by the classic whodunits. Behavior, it is argued, results from the interaction of motive, resources, and opportunity. Such a scheme can serve here to organize a sketch of the work relevant to power-seeking behavior. Consider opportunity. We know in a general sense that opportunity is important to political success. In pursuing dominant status, only the right power seeker for the context will succeed. But there has been very limited empirical specification of how context works its effects. Of course, all circulation-of-elites theorists

argue the importance of changing contexts for the emergence and decline of different types of leadership. Max Weber, for example, found in severe social and economic distress the societal conditions that set the stage for the rise of the charismatic political leader (e.g., Weber, 1968). However, although there is evidence supporting this general argument, there still are no specific predictions about how the opportunity presented by the context influences the behavior of power seekers, successful or otherwise.

Similar problems exist with the small group research literature in social psychology (although that literature is far more developed on this point than the political science literature). There is evidence, for example, that highly competitive or otherwise stressful situations set the stage for autocratic leadership (see Blake & Mouton, 1961; Hamblin, 1958; Sherif et al., 1960). But again there is no specification of the kinds of individuals who will gain or lose power in such settings. Fiedler's work (1965) is an exception in that it deals both with context and with effective leadership style. However, the operational definition of context is most relevant to static formal structures, and there is no concern in this work with power seeking.

With respect to opportunity, then, there is some evidence on how room at the top is shaped by context, and one can intuitively appreciate the importance of environmental contingencies for political success. But there is little understanding of how the context influences power-seeking behavior.

The second and third factors, resources and motivation, both shift attention to the individual actor, although, as noted above, their effects depend on context. Separation of the two factors in particular cases is sometimes difficult. For example, although skills would surely be considered a resource, would a disposition like Machiavellianism (Christie & Geis, 1970) or a physical characteristic like energy level be a skill or a feature of motivation?

In discussing the role of resources, Dahl (1975) is representative of many analysts when he writes, "In allocating income, wealth, status, knowledge, occupation, organizational position, popularity, and a variety of other values, every society also allocates resources with which an actor can influence the behavior of other actors in at least some circumstances" (pp. 145-146). But no evidence about which resources come into play for power seekers in particular contexts is available. The political participation studies (e.g., Verba & Nie, 1972) describe certain social and psychological resources of several strata of political activists, but one cannot substitute measures of electoral or community activism for measures of power seeking. As already noted, power seeking occurs in

many arenas, and those defined by local or national elections may not be very important to most people. Beyond that, many activists in elections may themselves have no desire to dominate or control others. Their satisfactions may have little to do with actual power (see Eldersveld, 1964; Marvick, 1967).

Motivation is the least understood aspect of human behavior. Moreover, it has ambiguous status as a construct in studies of behavior because its presence so often can only be inferred from the behavior it is supposed to have influenced. Thus, one finds in some work the rather unenlightening argument that power seeking is a result of a "power drive" (Hutschnecker, 1974). Ultimately, of course, motivation must be defined in terms amenable to causal analysis, and those terms are very likely to be biological. At present, except in the areas of eating and drinking, such definition is missing (Teitelbaum, 1967). Still, we are not entirely without guidance on the motivational underpinnings of power-seeking behavior.

A most interesting contribution comes from Christie and his collaborators (Christie & Geis, 1970), who have derived from Machiavelli's *Prince*, Kautilya's *Arthashastra*, and examples of Chinese legalist philosophy, a set of orientations which these writings hold to be characteristic of the successful power seeker. Among them are a great desire to win, little concern with conventional morality, manipulateness, low ideological commitment, and suspicion of others—but none of these in pathological degree. A psychological scale was built from these characteristics, and the behavioral distinctions between high and low Machs were these: highs lied more plausibly and effectively, were more inclined to engage in and to enjoy the manipulation of others, were likely to initiate and control bargaining situations and to be very successful in such situations, were much stronger and more forceful in social relations, persuading others more often and being persuaded by others less often, and, finally, saw more manipulateness in those around them.

This striking picture is quite close to the popular conception of power seekers. Not surprisingly, Christie and his colleagues found Machiavellianism to be correlated positively with scales measuring need for achievement (.29) and internal control (.43). One could speculate that self-confidence would also be tied in, an argument that has been made by others for political leaders in general (see Gibb, 1969; Stogdill, 1974).

Weaving together the three strains in the whodunit analysis has proven to be an impossible task for students of power seeking. It is clear that characteristics of the individual can be more or less suited to a particular context or vice versa, but no research on human social systems has given

empirical specification to this intuitively obvious point.

The UCLA Studies

By and large, political scientists—even those of the behavioral persuasion—seem not much impressed with animal models of social processes. We surely would not quibble with the contention that animal models have served medicine and psychology well, but the empirically oriented investigators in political science seem to assume that the leap from animal models to human politics is simply too great to be seriously considered.² Whatever the accuracy of this assumption in general, there is in this particular case a body of research using animal models which is of exceptional value.

A team of scientists at UCLA has for some years studied the social structure and political behavior of vervet monkeys.³ McGuire (1982) offers some basic information on this species:

These animals have distinct male and female dominance hierarchies which generally operate independently. . . . Considerably more is known about male-male relationships and they appear to be less complex than their female counterparts. . . . Male vervets generally have a binary type dominance system: one adult male is dominant. Kin relationships (genetic kin) appear to be minimally important in adult male relationships, and agonistically based coalitions—two or more animals joining together to fight one or more other animals—are seldom observed. . . . When animals engage in aggressive encounters, dominant animals most often win. . . . As sub-adult males reach maturity, they inevitably challenge the dominant male through display. . . . Depending on the intensity and frequency of such behaviors, aggressive encounters with the dominant male will follow. In most instances, say 80% of the time, the sub-adult male does not become the dominant male. Rather, he continues his challenging behavior for several months and then assumes a non-dominant position in hierarchy. If he fully assumes a non-dominant status, the probability that he will become the dominant male is considerably reduced.

In their experimental studies, the UCLA investigators most often have worked with vervet

²That is not to say that there is no published speculation about the relevance of animal studies for political science. Indeed, the unfortunately named subfield, biopolitics, is awash with such argument. But among the many empiricist students of political behavior, Wahlke (1979) is one of the very few calling for greater attention to relevant work on animal models.

³The species name is *Cercopithecus aethiops sabaeus*.

subjects which, although reared in the wild, had been in captivity for several months before the research commenced. Groups typically included three male and three female adult members housed in outdoor cages. Note that these arrangements provide very important simplifications for the study of behavior. First, the context is relatively constant; hence, ascension to power is less likely to be dependent on environmental factors. Constancy is also found in the kinds of resources that permit success. It is the skills involved in a direct challenge which are important, rather than the presence or absence of allies, for example. Motivation is still a mysterious factor, but recall that all subadult males apparently will sooner or later mount a challenge; this clearly provides more constancy than in the human case.

The UCLA researchers initially focused on the effects of social factors on vervet behavior. Of much greater interest here, however, is their work that connects status differences with distinct biochemical profiles. A key finding was that among captive male vervets there is a clear biochemical marker for dominance, namely the level of serotonin in the blood (McGuire, Raleigh, & Johnson, 1983; Raleigh & McGuire, 1980; Raleigh et al., in press).⁴ For nondominants the level of whole blood serotonin was on average 650 nanograms/milliliter (ng/ml), and for dominants, it was 937 ng/ml. The difference is significant at $p < 0.001$. Serotonin level also correlates with a repertoire of behaviors closely tied to dominance, such as approaching others, not avoiding others who come near, and initiating aggression (Raleigh et al., 1981), and most interesting, serotonin levels track changes in status: dominant males who become nondominant exhibit a decline in whole blood serotonin from the 900 ng/ml range to the 650 ng/ml range, whereas nondominant animals who become dominant show the reverse (McGuire, Raleigh, & Brammer, 1982; Raleigh et al., in press). In other words, biochemical change here follows status change, which suggests that for vervets the success or failure of power seeking, and not power seeking itself, is crucial to serotonin levels. I will return to this point later.

The connections between peripheral serotonin

and central nervous system function have been partially worked out (Raleigh, Brammer, & McGuire, 1983; Raleigh et al., 1983). However, for immediate purposes the details of this relationship are not important. The interesting question is whether or not the whole blood serotonin findings with respect to dominance can be replicated in man.

Methods

The present research was launched with the major purpose of examining the relationship between psychological stress levels and distributions of influence in small social units that are processing problems of collective importance. The concern with testing for serotonin correlates came later.

The design was experimental and involved 12 groups of six men, half of the groups in the high-stress condition and the other half in the low. Subjects were male undergraduates who were recruited through an advertisement in the student newspaper. Volunteers were excluded if they had a chronic illness, high blood pressure or severe allergies, were using medications or other drugs on a daily basis, or smoked a pack or more of cigarettes a day.

All sessions were held in a university hospital. Subjects were fully informed of the nature of the study and of the procedures involved. All physiological procedures were carried out by medical staff.

Tension was high as the experimental sessions began. Each group was directed to devise group solutions to 10 very difficult logic puzzles, presented seriatim.⁵ For each puzzle only 10 minutes were allowed. For a puzzle solution to be registered by a group, at least five of the six members had to vote in favor of that solution. Thereafter, one of the group had to report the group's solution and why it was the only correct one. (There was a financial incentive for members to resist being stampeded to a solution.)

Three types of observations were recorded. First, the session for each group was videotaped for subsequent coding of each subject's behavior. Second, questionnaires collected behavioral, judgmental, and attitudinal data. (The questionnaire items are given in the Appendix.) The basic questionnaire was administered three times: when volunteers assembled in a large room in response to the advertisement, about a month later when each set of six volunteers for a particular session

⁴Serotonin, also known as 5-hydroxytryptamine (5-HT), is a biochemical agent having neuroregulatory functions (Muller, Nistico, & Scapagnini, 1977). Those functions are not yet well understood. Serotonin is found in the central nervous system, in the blood, and in several other locations in the body. In vervets, whole blood serotonin has no significant seasonal or daily fluctuations (McGuire, Raleigh, & Brammer, 1982). In humans, it also appears to be a stable biological feature, not affected by normal variations in diet and not showing short-term fluctuations (Yuwiler et al., 1981).

⁵An example of the kind of puzzle used is this: Three boxes are labeled apples, oranges, and apples & oranges. Each label is incorrect. You may select only one fruit from one box. How can you label each box correctly?

had come to the hospital, and when a session was finished. The third type of observation required that a sample of blood be taken from each subject every 20 minutes throughout the session.⁶ Later, these samples would be assayed for a number of biochemical properties.

Each group was processed on a separate day but at the same time of day. A session began with the subjects' arrival at the hospital at 4 P.M., at which point vital signs and baseline blood samples were taken. At 5:30 all subjects were given a standard meal. At 6:00 the questionnaire was administered. From 6:30 until roughly 8:30 the group dealt with 10 logic problems, each of which required an agreed-upon solution (or the expiration of the allotted time) before the next puzzle could be addressed. Subjects were sent home at about 9:00 P.M.

In the blood sampling for the serotonin analysis, about 7 ml of venous blood was drawn, frozen, and maintained at -70°C .⁷ When all samples had been collected, they were packed in dry ice and sent by overnight express to UCLA where serotonin levels were determined.⁸ The UCLA investigators were blind to other information collected in this study.

The basic questionnaires had three major foci, two attitudinal and one more behavioral. The first attitudinal focus was the perceived locus of control (internal vs. external) over events in one's life. Items from the Rotter (1966) scale were used in that assessment, but they (as well as the other items in the questionnaire) were converted to a magnitude scaling format.⁹ The second attitudinal focus was perceived normal anxiety levels, for which modified items from Spielberger, Gorsuch, & Lusheme (1970) were used.

The behavioral items in the questionnaire dealt

with the Type A pattern. Because this typology has been little noted by political scientists, it will be useful to consider it in some detail. The Type A (vs. Type B) behavior pattern was originally observed by Friedman and Rosenman (1974) through structured interviews with cardiac patients. (See also Rosenman, 1978.) The behavior pattern has four key elements: extremes of aggressiveness, easily aroused hostility, a sense of time urgency, and competitive achievement striving (Matthews, 1982). The burgeoning research attention to Type A behavior derives from its connection with coronary disease, but obviously that connection is not in point here. Rather, it is hard-charging competitiveness itself which is of interest.

To permit larger epidemiological studies of Type A effects, Jenkins and others (Jenkins, 1978; Jenkins, Rosenman, & Friedman, 1967, 1968) developed the Jenkins Activity Study (JAS), a self-administered questionnaire of approximately 50 questions about the kinds of personal behaviors assessed in the structured interview. The JAS items were validated and weighted in a discriminant function analysis of a criterion population (i.e., a population of individuals already classified as Type A or Type B on the basis of structured interviews). Test-retest reliability checks of the Jenkins survey have produced correlations of approximately .65 over a year or more (Jenkins, 1978). The JAS has seen widespread application, but there remains a need for better measurement of the components of the Type A pattern. One effort along these lines, a factor analysis of the JAS items (with, for unexplained reasons, a varimax solution) did not produce dimensions having predictive power with respect to coronary disease (Zyzanski & Jenkins, 1970). Magnitude scaling was introduced in this study in response to these measurement problems.

The specific Type A items used in this study, 19 in number, come from Glass (1977), who created a student version of the Jenkins survey. This version involves minor rewordings to make questions suitable for an undergraduate population. Glass confirmed the comparability of this student version with the original JAS through the examination of factor structures.

In conceptual terms, the Type A pattern has clear overlap with Machiavellianism. Both include aspects of dominance, and, in fact, Glass (1977) shows a strong empirical relationship between the student JAS and a measure of dominance. Both emphasize competitiveness. Both emphasize (and correlate with) achievement striving and sense of personal control over the events of life (Glass, 1977). There is, however, an important difference in the ways the two have been measured: Machiavellianism is assessed primarily in terms of at-

⁶Blood samples were drawn from a special type of catheter placed in a vein of the arm (the antecubital vein) at the start of each session and left there until its finish about three hours later.

⁷These samples were drawn into a special type of vial (an EDTA vacuum tube) which has within it a substance to prevent clotting of the blood.

⁸Whole blood serotonin was assayed at UCLA using the procedure of Yuwiler et al. (1970). The assay was conducted with the use of duplicate 2-ml samples. Mean variation of duplicates was less than 4%.

⁹Lodge and Tursky (1979, 1981) have provided dramatic evidence of the superiority of magnitude scaling over the conventional category scaling used in most questionnaires. In this case, the mode of response was line drawing, to indicate intensity of agreement or disagreement with each item, relative to a standard line for "slight intensity." The technique was explained and illustrated for subjects before they began the first questionnaire. For further discussion of psychophysical scaling, see Stevens (1975) and Hamblin (1974).

titude, whereas the Type A pattern has always been tied more closely to (reported) behavior.

Findings

The analysis of serotonin levels (introduced after the above basic design was completed, remember) was exploratory. The only expectation was that some feature of power seeking or dominance would be tied to whole blood serotonin (WBS), but how the particular descriptions of self elicited by the questionnaire would relate to WBS was unknown.¹⁰ This, then, could be no more than a loose test in humans of the UCLA group's fascinating finding for male vervets.

The distribution of WBS in this set of subjects was normal, with a mean of 146 ng/ml and standard deviation of 49. The highest value was 295 ng/ml, and the lowest 40 ng/ml. The responses to the questionnaire items were also approximately normal.

Preliminary analysis of the questionnaire measures involved testing their interrelationships. The items (given in the Appendix) representing each of the three scales—internal control, anxiety, and the Type A behavior pattern—should reflect a common domain, and they do. The mean correlation within the set of four items measuring internal control was 0.38, and that for the eight measures of anxiety, 0.42. For the 19 measures of Type A, it was only 0.19; however, this is not appreciably different from the mean interrelationship in other analyses of JAS items. Moreover, the orthogonal factor structure of these Type A data included the *H* (hard-driving) and *S* (impatience) dimensions found by Zyzanski and Jenkins (1970) in their factor analysis of the full set of JAS items, and with much higher communalities for the defining measures. Glass (1977) also has found these two dimensions, using the same factoring techniques. Hence, it seems that the magnitude scaling version of the A-B measures was valid.

Correlational analysis showed significant covariation between WBS and a number of the questionnaire items. However, further inspection of these relationships showed that in most instances they were not linear but instead depended heavily on the more extreme values at one end of the distributions. For example, the simple correlation between the response to the statement, "I find competition stimulating," and WBS was +0.13. But when that measure is rescaled with the

lower two-thirds of the distribution scored zero and the upper third scored one, its correlation with WBS increases to +0.25. This was not a matter of dividing the distribution at the agree-disagree point; it was well into the agree response that the flat relationship continued. Put differently, it was not those who agreed that competition was stimulating who were more likely to have elevated WBS values; rather, it was those who strongly agreed. This, of course, further illustrates the value of magnitude scaling; had category scaling been used, this relationship might well have been lost.

The questionnaire items having a significant zero-order relationship ($p < 0.05$) with WBS are presented in Table 1. The form of each item—dichotomous or continuous—is indicated. All Type A items are also indicated.

What is immediately apparent in Table 1 is that Type A items show up in striking proportion. Ten of the 13 significant items are drawn from the JAS. This is more than half of the A-B items in the pool. What does not show up here are the items tapping anxiety and sense of internal control. Other than A-B items, the significant correlates of WBS are self-confidence, dislike of being in a leaderless group, and distrust of others.

These simple findings are most intriguing. The aggressively competitive type defined as Type A does in fact seem to have elevated WBS. In addition, the discovery of the WBS correlation with distrust of others, when put in the context of a Type A behavior pattern, suggests plainly the orientation Christie labelled Machiavellian.

An obvious next step was to test multiple-item scales. The findings were straightforward: first, the composite measures of internal control and anxiety, whether items are weighted or unweighted, do not show significant relationships with serotonin. In the case of the Type A measures, neither a summed index, a first principal component, nor the orthogonal dimensions described earlier show a relationship with WBS as great as the correlations for most of the individual items given in Table 1. With the summed index, it was once again noted that the relationship was curvilinear: the takeoff in WBS values came at the upper end of the index. Hence, the A-B items were refactored, this time with the "optimal" form for each item. Because there seemed no reason to continue following Zyzanski and Jenkins (1970) in seeking orthogonal structure, the defining items of the three resulting factors (given in the Appendix) were simply added together after being standardized. This produced three indexes: drive, aggressiveness, and competitiveness. Their correlations with WBS were +0.41, +0.44, and +0.33. When given simultaneous tests in a regression analysis, com-

¹⁰The questionnaire data in this analysis were collected immediately before the beginning of the experimental session.

Table 1. Correlations of Whole Blood Serotonin with Selected Questionnaire Items

Everyday life is filled with challenges to be met.	.249*	(A)
I would dislike being a member of a leaderless group.	.248*	
I probably eat faster than I should.	.364*	(A)
People can be trusted.	.275**	
I am more responsible than most college students.	.272*	(A)
I often set deadlines for myself.	.258*	(A)
I have more willpower than most people seem to have.	.335*	(A)
I plan to find a career that is difficult and challenging.	.267	(A)
I lose patience with speakers who don't get to the point.	.322*	(A)
I find competition stimulating.	.251*	(A)
I get a bit angry with people who can't understand simple ideas.	.320*	(A)
Normally I feel self-confident.	.340*	
It makes me angry when people are late for appointments.	.363*	(A)

All correlations are significant at $p < 0.05$. Coefficients marked with * indicate the attitude item is dichotomous, with the upper third of the distribution scored one. Those marked with ** also indicate a dichotomy but with the lower third of the distribution scored one. Items from the student version of the Jenkins Activity Survey are marked (A).

petitiveness dropped out of significance, both substantively and statistically. Both of the others proved to have about equal impact (betas $\approx .30$; t -ratios ≈ 2.5). The resulting multiple correlation, adjusted for degrees of freedom, was 0.487. That competitiveness dropped out is a reflection of the correlation this index has with the other two. It should not be taken as evidence that competitiveness per se, or even of this particular index of competitiveness, has no role in the statistical explanation of WBS levels. What is suggested here is that its effect is indirect, flowing through the other two factors.

Another technique for exploring these data is provided by stepwise regression analysis with backward elimination of statistically insignificant variables. (The procedure stops when all coefficients are significant at $p < 0.05$.) This is a brute force method which plainly lacks theoretical grounding. Moreover, criticisms of the caprice that can operate in such an exercise are well-founded (Lewis-Beck, 1978). There is every reason, then, not to pay much attention to the individual coefficients that result from such an exploration. Nonetheless, here the technique can serve a very useful purpose: namely, to give a rough picture of the stamina of the WBS correlates in what is now a multivariate setting. Starting with the full set of questionnaire items (again, with optimal forms), the stepwise procedure continued until a final model incorporating 14 variables was reached. (The variables are identified in the Appendix.) Of these 14 variables, nine were from the Type A set. Distrust also remained. In a purely empirical sense this model constitutes a very good statistical explanation of WBS levels; after adjusting for degrees of freedom, 50% of the variance in WBS was statistically explained.

Perhaps more important, however, is the evidence given here of the resilience of the Type A items in a multivariate analysis.

Discussion

What is to be made of these findings? First, there is good evidence here of a connection between a peripheral biochemical measure, WBS, and a number of reported behaviors and attitudes. Note that this is not at all an obvious connection, as would be the case, for example, in the correlation between an attitudinal measure of fright and adrenaline. Moreover, with respect to the Type A measures, it is a connection with an extensively analyzed syndrome of behaviors and orientations, a syndrome important in its implications for other (i.e., coronary) features of human physiology. Finally, it is a connection that has in its biochemical component a quite stable biological property (Yuwiler et al., 1981). Changes do not occur quickly and reflect only ephemeral, and therefore for the political scientist uninteresting, human states. Of course this stability complements the demonstrated stability of the Type A pattern and the assumed stability of the attitude measures.

Second, and more important for students of political behavior, there is in the WBS-related amalgam of behaviors and attitudes a striking portrait of the kind of person one would intuitively take to be power oriented. Hard-charging, competitive, impatient, aggressive, distrustful, confident—all in all a remarkable mix, and one readily tied to Machiavellianism, lacking only evidence of the ruthless and manipulative aspects to make the match very close. The strong relationship WBS has with this amalgam makes it a bio-

chemical marker of fundamental importance to political science. For the first time, evidence is at hand which suggests that Lasswell's political type may be at least partially characterizable in terms of biological properties.

This finding has immediate and obvious methodological implications for the study of influence and the influential in human social units. However, its substantive implications, potentially enormous, can for now only be a matter of speculation. There is no theory about how power seeking might drive WBS; we do not yet know what consequences elevated WBS levels might have. With respect to the latter, it might be tempting to argue some sort of changed brain function, because serotonin is a neuroregulator, but such an argument would be premature. Recall that the connections between WBS and brain serotonin have not been fully worked out, and within the brain itself, the role of serotonin is still highly uncertain. However, even in the absence of that kind of knowledge, one can see in this discovery clear linkage between behavioral political science and the neurosciences, which reminds us that many of the most enduring research problems in political science have investigatable biological foundations.

Third, there is in these findings a clear, although not closely specifiable, connection with the UCLA group's results with male vervets. One important implication of this connection is that animal models can indeed prove useful in bio-behavioral studies of politics. Of course such models cannot substitute for direct investigation of human behavior, but they can serve as invaluable heuristic guides in the specification of hypotheses about the biological substrata of that behavior.

Of greater immediate importance, however, is the fact that the connection established here with the previous work on vervets confers additional validity upon the interpretation of the present findings. A power orientation related to dominance seems clearly involved in human subjects with high WBS levels, and this view gains still more support in pilot work by the UCLA group which has found that leaders of campus social organizations have significantly higher WBS levels than do initiates (McGuire, personal communication).

However, note that there is a disparity between the vervet findings and the view that high WBS levels in humans mark power *seeking*. In the vervets, recall WBS levels *followed* status, rising with ascent and falling with descent. With the humans there was no dynamic analysis; only cross-sectional correlations are available. Because WBS is a stable biological property over many days at a time, we know that success or failure in

the experimental context could not produce the recorded WBS values. What we do not know is the subject's power or dominance status in the social settings of greatest relevance to him. Thus, the available data do not permit us to rule out the possibility that for humans, a high WBS level is a marker of *successful* power seeking, as it was for vervets. I have opted for the less restrictive interpretation (i.e., that WBS marks power seekers, successful or not) because of the general characterization of self found in the questionnaire data and because I have no evidence that my volunteers were dominant in any social arena. Obviously, sorting this out is a high priority in future work.

A note on the measurement of power seeking in this study: It goes without saying that questionnaire data here are only second best and that direct and detailed observations of actual social behavior would have been preferable. These findings obviously must be judged within the limits imposed by questionnaire data (a caveat that could appear in almost all political behavior research reports; see Wahlke, 1979). Bear in mind, however, that the questions treating the Type A pattern have been developed and refined on the basis of careful and frequent assessments of their ties to actual behavior (and to the coronary disease that is thought to be associated with that behavior).

What are the next research steps that might be taken by political scientists (necessarily in collaboration with medical and biological consultants or co-investigators)? The first need, of course, is a study that attempts to reproduce the findings presented above. The design for such a study should provide for much greater attention to the social environments from which volunteers are drawn, taking as a special concern the power status of each volunteer within his natural environment. It should also involve greater specification of the elements of the Type A pattern and of Machiavellianism which are related to WBS level. A second possibility would be replications in different subject populations, for example, in women or in various age groups. A third would be to work backward from blood samples, perhaps available from other projects, to the individuals in their social contexts. One could select a sample of males with high and low WBS levels, for example, and then collect ethological or questionnaire data on them and on their natural environments. A fourth possibility is close to being the reverse of the preceding one: to go to structured social environments and take blood samples from high and low dominance individuals. Finally, one might try to create a natural social environment in the fashion of Sherif et al. (1960) and to monitor both power status and WBS levels over the course of a number of weeks.

In summary, then, there is in these research results good evidence of a tie between whole blood serotonin and a power or dominance orientation in humans. That discovery echoes similar findings in a species of subhuman primates and

constitutes the first systematic evidence of any biochemical property in humans that differentiates power seekers from others. This finding represents for political science a major new direction in the behavioral study of power.

Appendix. Questionnaire Items

Type A Behavior Pattern

Aggressiveness

I lose patience with speakers who don't get to the point.
I get a bit angry with people who can't understand simple ideas.*
It makes me angry when people are late for appointments.

Drive

I probably eat faster than I should.*
After eating, I want to get busy instead of sitting around.*

Competitiveness

I am more responsible than most college students.*
I often set deadlines for myself.
I need to slow down, to be less active.
I plan to find a career that is difficult and challenging.
I am more precise than most people.
I find competition stimulating.*

Other

Everyday life is filled with challenges to be met.
I stay up late at night to relax and unwind.
I am relaxed and easy going.*
When I do a job, I do it well.*
My friends see me as competitive and hard-driving.
Frequently I think of other things when someone is talking to me.
I like to get up and get going in the morning.*

I give more effort to my work than do most college students.
I am more serious than most people.*

Internal-External Control

I do not believe that luck plays an important part in my life.
When I make plans, I am almost certain I can make them work.
Most people's lives are controlled by chance circumstances.
I have more will power than most people seem to have.*

Anxiety

Normally I feel secure.*
Sometimes I feel useless.
Much of the time, I feel under stress.*
Normally I feel calm.
Normally I feel a little bit anxious.
Normally I feel self-confident.
Often I feel worried.

General

I would dislike being a member of a leaderless group.
Strong leaders are what made this country great.
People can be trusted.*
Once I make up my mind I seldom change it.
I have less energy than most people my age.*

*These items were left in the final regression model.

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A Politico-Economic Theory of Income Redistribution

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This study integrates models of income redistribution developed by economists, who suggest that citizens voluntarily redistribute because of interdependent preferences and rely on the state for implementation owing to the public-good nature of redistribution, and political scientists, who focus on conditions that lead to demands that the state intervene to assist the poor and on the development of institutions that facilitate such demands. We propose a testable theory of redistribution and apply it to data from the American states.

The empirical analysis addresses determinants of the Aid to Families with Dependent Children guarantee, adjusted for Medicaid and food stamps to which a family receiving the guarantee would be entitled. We posit significant links between the guarantee and both observable explanatory variables, such as per-capita income, and latent constructs, such as liberal party control. We specify observable indicators for the latent constructs and use the LISREL method to estimate parameters for the indicators and structural coefficients. The findings show that both political and economic variables significantly affect the level of the guarantee.

Of the many topical frontiers that join the social sciences, the study of income redistribution is an important one to economists and political scientists. Speculation about causes of redistribution neatly sets up concepts that mark their two disciplines: voluntary and coercive behavior. Some economists suggest that redistribution by government occurs because citizens value income transfers to the poor for direct reasons (satisfaction of charitable impulses, knowledge of poverty is personally unsettling), or indirect reasons (redistribution might make the poor more productive members of society or help reduce crime). Because the direct and indirect consequences of redistribution are public goods, everyone will benefit if they are provided. When everyone benefits, citizens might freeride if redistribution were voluntary (Hochman & Rodgers, 1969). Thus, government action is desirable to ensure that the entire community contributes.

Political scientists and some economists with a public choice orientation adopt a different, although not necessarily conflicting view. Schumpeter (1947), Downs (1957), and other theoreticians suggest that redistribution by government is largely a coercive, extractive process. Political parties in a modern democratic state compete for the votes of poor and nonpoor alike by promising to extract from others, the wealthy or the uninformed. There are limits to this extraction, an important one being the power and influence of the wealthy.

We began our research by asking: "What ought to characterize a theory of redistribution as a public good?" It became clear that although political institutions collectively determine redistributive policy, factors that shape individuals' choices are properly economic and political. An inclusive theory must address both the choices of individuals with diverse attitudes towards welfare and with varying resources acting as voters, members of interest groups, and supporters of political parties, and the aggregation of these choices via states' political institutions.

We advance the political-economic analysis of explicitly redistributive programs in several respects. For reasons discussed later, we, unlike others, focus on the income guarantee for the Aid to Families with Dependent Children program (AFDC) in the American states. Each state legislatively establishes its guarantee, which is the maximum benefit for a family with no other income, through an explicit political process. The guarantee, we suggest, expresses a collective

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choice of the preferred level of income support for the state's needy.

In addition, we develop a theoretical model of the redistributive process in the American states which integrates the voluntaristic perspective of economists with the institutional perspectives of political scientists.¹ We posit significant links between the welfare guarantee and both observable explanatory variables (e.g., per-capita income) and latent constructs (e.g., voter preferences and interest group strength). We also develop in greater detail than previous work several testable theoretical relationships among the political-institutional variables. In the empirical work we specify observable indicators for the latent variables and use Jorsekog's LISREL method to generate estimate coefficients for the indicators and in the structural equations.

AFDC has features that make it an attractive subject for investigation. Unlike Social Security, widely perceived by voters and recipients as an elaborate insurance program, or the present Supplemental Security Income program, widely seen as assisting the deserving poor, AFDC tests the political limits of redistribution. There is substantial political conflict over the deservingness of its recipients—female-headed families, disproportionately minority (47% of the caseload is non-white), often with illegitimate children (33% of caseload families have an illegitimate child).² Additionally, AFDC is a genuinely federal program. State legislators and executives establish the levels of benefits, but conditions of federal assistance are important influences on their choices.

The balance of this article has six sections. The second section briefly reviews previous related scholarship in economics and political science. The third section discusses why we chose the welfare guarantee rather than other program measures as the dependent variable. The fourth section presents our politico-economic model. The fifth section discusses the data and estimation technique, and the sixth contains empirical findings. The last section provides a conclusion.

¹There has been little sustained cross-disciplinary effort on this topic. An important economic review of the income redistribution literature (Rodgers, 1974) cites one political science article (Wilson & Banfield, 1964). The standard textual treatment in political science (Albritton, 1983) cites no economic articles. Yet economic and political analyses of income redistribution largely complement one another, and an integration of the two is feasible.

²Data on the caseloads are drawn from *1977 Recipient Characteristics Study: Part I—Demographic and Program Statistics* (U.S. Department of Health and Human Services: Social Security Administration; Office of Policy, Office of Research and Statistics, June 1980), pp. 1 and 28.

Review of Previous Scholarship

Economics

Three approaches to the determinants of income redistribution have received attention by economists: 1) voluntary redistribution owing to interdependent preferences and the use of government because of the public good nature of redistribution (i.e., the approach discussed in the introduction), 2) an insurance model of voluntary redistribution, and 3) self-interest models of involuntary redistribution as an outcome of voting, legislative, and bureaucratic processes.³

The second approach perhaps better suits analyses of social insurance programs such as unemployment insurance or workers' compensation, rather than needs-tested ones such as AFDC. We will not discuss it here. The involuntary model is largely consistent with work by political scientists discussed below.

The most ambitious work in the interdependent preference/public good framework is by Orr (1976, 1979).⁴ In his model a taxpayer gains utility from his disposable income and also from transferring income to welfare recipients. Each taxpayer balances the marginal utility from higher benefits against the marginal disutility from larger taxes to choose an optimal benefit. After each taxpayer has chosen his own preferred benefit level, the median voter's choice determines state policy. Orr's model implies that willingness to transfer is conditioned by personal income and the cost to the taxpayer. Holding the benefit level fixed, the cost per taxpayer depends on the ratio of recipients to taxpayers and the federal matching rate for AFDC benefits. Work in political science and earlier economic analyses had overlooked this price incentive. Testing the model with state-by-state data on mean AFDC benefits during the 1963-1972 period, Orr finds strong support for it. His later article extends the model to account for the fiscal incentives of the food stamp program as they affect states' AFDC decisions. The model again receives empirical support using 1975-1976 data.⁵

³See Rodgers (1974) for a useful summary of each approach and Mueller (1979), *passim*.

⁴See also comments on the 1976 article by Spall (1978a) and Schiller (1978) and Orr's replies. Earlier studies of state welfare policy include Tresch (1975), Albin and Stein (1971), and others cited in Orr (1976). As Orr (1976, fn. 8) observes, these articles did not develop models of policy determinants.

⁵Spall (1978b) analyzed state-by-state data for 1975 within a framework similar to Orr's. His findings also supported the interdependent preference/public good model. Parsons (1982) provides an elementary analysis

Orr's clarification of the crucial roles of fiscal incentives in redistributive policy is a major contribution. Yet he paid little attention to the role of political variables in shaping policy and simply assumed that the political process proceeds straightforwardly to translate the median voter's preferences into public policy.⁶

More recently Gramlich (1982), Moffitt (1983), and Hulten and McCallum (1982) have analyzed state AFDC policy. All adopt models similar in spirit to Orr's, but extend the empirical work in different directions. Like Orr, none attempts to integrate the economic analysis with institutional and theoretical perspectives of political science.

Political Science

Studies of the formation and implementation of welfare and income redistributive policies date from about 1963.⁷ In the 1960s and 1970s, scholars concentrated on quantifying data on welfare policy, applying various simple statistical techniques, exploring the power of political explanations of redistributive policy, and consciously building on the research of others.

Research focused on institutional models of redistribution. For example, some analysts speculated that the degree of interparty competition shaped inducements for electoral support and ultimately redistributive policy (Dawson & Robinson, 1963). Others argued that the extent of legislative malapportionment affected the representation of the urban poor (Dye, 1966).

Complicating the discussion of the 1960s was the discovery that these institutional links seemed to be dependent on the level of socioeconomic development in the states. Empirical work suggested that the more developed the state, the more competitive the party system and, therefore, the more generous the welfare program. Socioeconomic development was measured eclectically: Dawson and Robinson (1963) utilized indicators of per-capita income, urbanization, and industrialization, whereas Dye (1966) added education.

Jones (1973) and others criticized this early research. As Hofferbert (1972, p. 32) put it, these studies had "high propositional vitality," but "low theoretical content." There were strikingly

few well-reasoned arguments why particular practices should be the focus of political research, nor were there arguments that linked the various propositions into consistent theoretical models of the causes of redistribution. The logic justifying the role of interparty competition seemed to be based largely on an argument derived from Key's *Southern Politics*, with little attempt to spell out why variations in competition across the states outside the south will lead to greater sensitivity to the poor (Uslaner, 1978). After all, the poor are less informed than the not-so-poor and therefore more difficult to reach, less likely to participate in politics, and more difficult to mobilize. Additionally, a perfectly reasonable argument suggests that greater interparty competition leads to greater sensitivity to the wealthy because of the crucial quality of their resources in competitive elections. Similarly, there was little theoretical rationale for why increasing income, industrialization, or similar factors ought to lead to more competition between the political parties and to greater welfare generosity.

In the more recent past, research on state welfare and income redistribution policy diversified. Studies used more sophisticated statistical techniques (Lewis-Beck, 1977). Analysis of longitudinal data sets appeared (Jones, 1974; Winters, 1976). More carefully specified theoretical approaches were developed (Jennings, 1979; Uslaner & Weber, 1975), and studies focused on policy measures more proximate to the decisions of political leaders (Gary, 1973; Gronbjerg, 1977; Tompkins, 1975).

The political analysis of redistribution has advanced over the last 20 years. Yet critics (Burstein, 1981; Godwin & Shepard, 1976) claim that the links among voters, political institutions and public policy still remain poorly established. This article presents an explicit model that links these variables and provides a theoretical rationale for many of the traditional propositions. Our purpose is to integrate the political-institutional-organizational arguments for redistribution with voluntaristic-public goods ones.

Use of the Guarantee as the Dependent Variable

The actual redistributive impact of a welfare program depends on its eligibility and benefit rules, and the distribution of germane socioeconomic characteristics in the population. For most direct income support programs, the latter would typically include age, earnings, other sources of income, marital status, and family size. The rules and characteristics jointly determine the size and demographic composition of the recipient population and the average benefit per recipient.

of state Old Age Assistance policy that compares altruistic and voting power explanations for the aged's welfare benefits. Neither of these studies is as thorough as Orr's.

⁶This criticism also applies to studies cited in footnote 4.

⁷Hofferbert (1972, 1974), Jacob and Lipsky (1968), Gray (1976), and Uslaner (1978) present excellent reviews of the literature.

ent. In AFDC, a family's actual cash benefit equals the income guarantee minus a fraction of the family's earned income and a (usually higher) fraction of its unearned income such as Social Security or alimony. Legislators and governors can only directly alter the rules of the redistributive programs.

Once one recognizes the interaction of both types of factors, it becomes clear that the redistributive intent of a program is probably best reflected in its rules, not in the final distribution of benefits, the mean benefit, the number of recipients, or total outlays.⁸ The legislated rules may partly reflect considerations of how many persons are likely to qualify for benefits and the expected level of total spending. But these latter variables are products of both legislative choice and factors outside legislative control. For this reason we suggest that the legislated rules are appropriate and important variables for analysis.

In income-conditioned programs such as AFDC, the crucial rules are the income guarantee, the eligibility requirements, and the rate at which benefits are reduced as other income rises. This study focuses on the guarantee, an important indicator of the state's willingness to redistribute income to the needy. Joint analysis of all three parameters remains for future research.

A simple measure of the guarantee is the maximum annual cash AFDC payment for a four-person family with no other income. Additionally, most AFDC families are eligible for the federally funded food stamp program. Food stamps provide a maximum in-kind benefit, which varies by family size, to families with no cash income. As a family's cash income including AFDC rises, federal law requires the food-stamp benefit to be reduced at a specified rate until net food-stamp benefits reach zero. AFDC guarantees have been sufficiently low that any family receiving the full guarantee also would qualify for some food

stamps. Because food stamps are excellent substitutes for cash, states may well take into account contributions to total income of this food assistance when legislating AFDC cash guarantees. If this is so, the appropriate dependent variable would be the sum of the AFDC guarantee (G) and the annual food-stamp benefit (FS) that would be gained by a family of four if its only cash income were G .

One other important factor may condition the AFDC guarantee. AFDC recipients are eligible for Medicaid, which annually provides hundreds of dollars of aid per eligible person. Variation in the Medicaid benefit differs from that of the AFDC guarantee alone because, within limits of federal law, states differ in the scope of medical services they choose to cover. Also, Medicaid utilization rates by eligible persons vary by state. We suggest that legislators, aware of both the past utilization rates and number of Medicaid services, recognize that there is an implicit, but not legislatively mandated, guarantee of medical aid for the indigent. If so, legislators would adjust the cash guarantee accordingly to obtain their desired overall cash-food-medical welfare package. Let MED denote the value of the medical services that a four-person AFDC family would expect to receive in a year. An alternative and for legislators probably the most relevant measures of state redistributive policy is, therefore, $G + FS + MED$. The empirical work examines all three notions of a welfare guarantee.

A Politico-Economic Model of Welfare Policy

Legislative decisions, affirmed by the governor, directly establish redistributive policy. The impact of any political or economic variables then, occurs only by the way it affects statutory action. We suggest that the path diagram of Figure 1 summarizes the causal links and directions of the relationships among these variables. Political variables exert both direct and indirect effects on the guarantee (paths 1 to 12, 18, 19). The economic variables have only direct effects (paths 12 to 18).

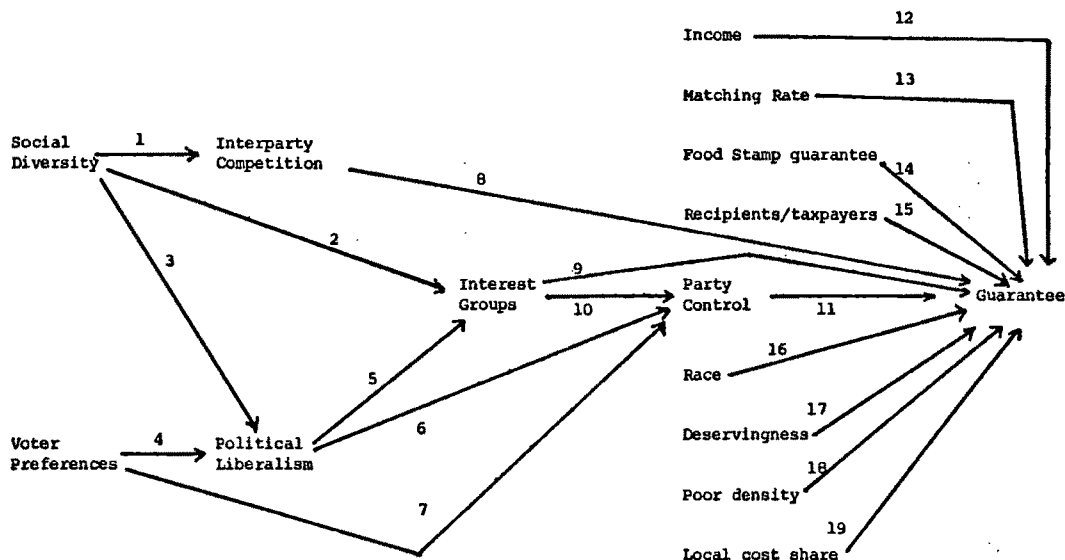
The Political Segment of the Model

Our model of the state political process draws on theories of pluralism and party competition. It posits a series of relationships among social diversity, voter preferences, interest group activities, political party control, interparty competition, and welfare policy.

Social Diversity. In our formulation, states are more socially diverse the less accurately any single category, be it racial, ethnic, religious, or economic, portrays the population. Our concept

⁸Almost every study of the determinants of redistribution or welfare focuses on other measures of spending, for example total welfare spending, total per capita, per \$1000 of personal income, or per recipient. These measures do not represent the direct political choice regarding the proper levels of statewide redistribution. Rather they indicate outcomes shaped by the factors separate from and often occurring after the political decision on the guarantee. For example, mobility into the state might be a response to the level of the guarantee and would then shape total spending, number of recipients, and aggregate per recipient. Studies that focus on these *consequences* of political choice in economics are Orr (1976) and Spall (1978); in political science, Fry and Winters (1970), Dawson and Gray (1971); in sociology, Gronbjerg (1977), and Isaac and Kelly (1981).

Figure 1. A Politico-Economic Model of Welfare Policy



of social diversity, although not systematically applied to the political analysis of the American states (however, see McConnell, 1966; and Dahl & Tufte, 1973) is related to the more general political concept of socioeconomic development.

The notion of development in the writings of the 1960s appears to be a composite of factors such as personal income, urbanization, and industrialization. Early analyses devoted little effort to explicitly detailing the links between changes in different dimensions of socioeconomic development and changes in welfare policy. Therefore, we will unpack the more general concept, examine carefully the causal role of each factor, and explain how the factors enter our own model.

Among the discrete political science components of development, personal income and urbanization are compatible with a theory of income redistribution based on interdependent preferences. A high level of development in part simply means larger numbers of citizens with high personal incomes. Greater affluence allows larger numbers of persons to indulge at higher levels their impulse to aid the needy. This logic is identical to that in the interdependent preferences-public goods theory of economists, which predicts a direct connection between income and the guarantee.

Analysts argued that greater urbanization leads to more generous welfare policy. Although the authors did not usually spell out the logic, we infer that they assumed that the urban poor were probably easier to organize than the rural poor for reasons of proximity and lack of informal social

controls. However, it is also reasonable to hypothesize that citizens in states with large urban populations have more direct contact with the urban poor and therefore more direct knowledge about their condition. All too often the destitute are in the streets, directly visible to the urbanite. If, as the interdependent preference theory maintains, citizens' own senses of well-being are sensitive to the welfare and happiness of the poor, then a direct causal link from the density of the poor (an indicator of how frequently one would likely interact with a poor person) to redistribution in the states is consistent with the voluntaristic theory, which assumes taxpayers' well-being is reduced by encountering the destitute.

A third model of the effects of development focuses on economic, social, ethnic, and religious diversity. Such diversity directly affects the number of politically active and important groups within the states and the sensitivity of the political parties to the special pleadings of these organized interests and probably tips the balance of political sentiments in a more liberal direction (Dahl & Tufte, 1973; Eulau & Prewitt, 1973; McConnell, 1966). Although these consequences of diversity may directly affect redistributive policy, diversity exerts an indirect affect in this model.

A final view of how development affects welfare policy, a view commonly found within the political science literature, addresses processes of rapid and substantial industrial transformation. This view of development's impact meshes with the political view, the extractive hypothesis. Industrialization creates a factory class, which

faces a more marginal existence than the wealthy or farm families and may be forced to rely on welfare at times. The growing factory class probably leads to a transformation of the party system into largely class-based parties in which the traditionally liberal party favors extractive redistribution from the wealthy and business sectors for insurance against economic insecurity. Additionally, for reasons of political convenience alliances with the nonworking poor will probably develop.

In the industrial transformation view, development changes the nature of political institutions, the number and content of political demands placed on them, and ultimately the choices regarding redistribution. Development's impact is indirect. This model essentially offers a historical interpretation of the origin of redistributive policies in the states. Since we are dealing with the post-transformation period of the 1960s and 1970s, this view of development lies outside our model. The empirical work that follows addresses the first three aspects of development.

The general concept of state development, therefore, has several separate dimensions, each with its own causal links. Personal income is treated as a separate independent variable because of its direct effect on personal choice. The economic logic, discussed more fully below, seems compelling: greater income leads to a greater ability to pay for redistribution to the needy. (See path 12 in Figure 1.) We have treated urbanization as a causal agent that also directly shapes citizens' willingness to transfer to the poor (path 18).

Socioeconomic development will manifest itself in diversity that drives other political factors related to redistribution. Diversity is more likely to manifest itself in a highly competitive political arena (path 1) and a larger number of interest groups (path 2). Such states will have a larger number of politicians and groups who advocate and support redistributive programs (path 3). We now discuss more carefully the effects of social diversity and the other paths in Figure 1.

Diversity and Interparty Competition. Diversity directly affects the strength and competitiveness of the political party system (path 1). The competitive balance between political parties is likely to be closer in the more diverse states. Groups obtain maximum influence on party behavior when they can claim that the votes of their members formed the marginal bloc of the winner's electoral coalition. As diversity increases, more groups can reasonably assert their crucial marginality and represent themselves as influential. Greater diversity thereby increases the parties' sensitivity to the pleadings of more groups. In the diverse states, party leaders will lack precise information on the validity of the claims of group influences and will become captive to the pleas of many of them.

Interparty Competition and Redistribution. Probably the best known link in the comparative state politics literature is that between two-party competition and redistribution to the "have-nots" (Key, 1949; Lockard, 1959; Price, 1970). There are good theoretical reasons to expect such a consequence of competitiveness (path 8). Under conditions of routine competitiveness, the state will be active in many areas as political leaders vie in offering the authority and services of the state to various groups in return for electoral support. Policy initiatives to the poor simply represent one more attempt to use the authority of the state to benefit selectively a particular constituency.

The politics literature correctly points out that low income people form a large pool of unmobilized voters. They are conventionally thought of as Democratic voters, but both the statistics and the more calculating professionals suggest otherwise.⁹ The politically unmobilized quality of the poor, their numbers, and the uncertainty of their response to policy overtures make them an attractive target for the politician operating in the highly uncertain world of competitive politics in a diverse American state.

Diversity and Interest Groups. As the states increase in economic diversity, the number of special interest groups increases. The poor and those who support or express the interests of the poor will be represented among these groups. Although there is no necessary reason why organizations must appear to represent the latent interests of individuals (see Olson, 1965), it is a widely accepted argument that as society, or as we argue here, the American state, increases in population size, urbanization, and economic and social differentiation, diverse organizations will develop to represent these segmented interests (Dahl & Tufte, 1973; Eulau & Prewitt, 1973).

Interest Groups' Impact on Party Control and Redistribution. Interest groups will seek special advantages through laws and regulations (Olson, 1983; Peltzman, 1976; Stigler, 1971). Their objectives (in our case, more generous welfare programs) can come about through two processes. First, an interest group can negotiate its goals with the leadership of the parties, offering its support, votes, and financial contributions for successfully promoting the ends of the groups (path 10). Thus, strong interest group efforts on behalf of the poor

⁹For a useful statistical summary of the contribution of the poor to various Democratic presidential election coalitions, see Axelrod (1972). Moynihan (1973) succinctly analyzes the Nixon administration's perception of the Republican electoral gains to be made from among the ranks of the working poor.

will lead to more liberal party control of the state government.

Second, a group can lobby elected officials, whatever their party affiliation or ideological convictions, as a direct means of influencing legislative action (path 9). The more powerful the interest groups acting on behalf of the poor, the more likely it is that a high welfare guarantee will be enacted.

Diversity and Political Liberalism. Our earlier analysis suggests that diversity leads to ever larger numbers of expressions of political sentiment. We further propose that more socially diverse states will also be states where voters and leaders express a more enduring political liberalism (path 3).

The logic yielding this conclusion starts from Key's observation that interests of conservatives, those preferring the status quo, and the wealthy are already well organized in every state, diverse or not (Key, 1949; McConnell, 1966; Price, 1970). The wealthy have a straightforward, tangible interest that the state not redistribute. They possess political skills, contacts, and information that make for political potency (Dahl, 1961). Sentiments promoting the welfare of the indigents obviously face more severe costs of political organization (Lipsky, 1968). But as the state becomes more diverse, greater numbers of organizations arise to represent groups that are, by default, less elite than those already entrenched politically. Thus, the balance of political forces begins to tip away from the already well-organized forces of the wealthy and business community in favor of the more difficult to organize, yet larger numbers of the poor.

Two expressions of greater fundamental liberalism will be increased strength of pro-redistributive interest groups (path 5) and a greater frequency of liberal party control (path 6).

Liberal Party Control and Redistribution. Control of the state legislature and governorship by the relatively more liberal political party will also be positively related to redistribution (path 11).

Voter Preferences and Redistribution. Preferences for redistribution may vary across the states. For example, state per-capita contributions to United Way charities differ widely. More altruistic preferences lead to a stronger enduring liberalism which produces larger numbers of liberal elected officials (path 4). Also, more altruistic voters will directly elect more liberal lawmakers (path 7). Note that voter preferences do not influence the policy decision directly. Preferences only enter indirectly in a representative democracy via election of like-minded legislators and, after the election, by direct or indirect (via interest groups) communication to these lawmakers.

Local Cost Sharing and Redistribution. Counties or localities share the nonfederal cost of AFDC in a few states. Cost sharing may encourage state officials to raise the guarantee, since political blame for any ensuing tax increase will be partly shifted to county and city officeholders. Pressure from these officeholders upon state officials to hold down the costs of state-enacted policies that raise county and local tax needs exerts an offsetting force. Hence, we cannot offer an unambiguous theoretical prediction of this variable's impact on the guarantee (path 19).

The Economic Segment of the Model

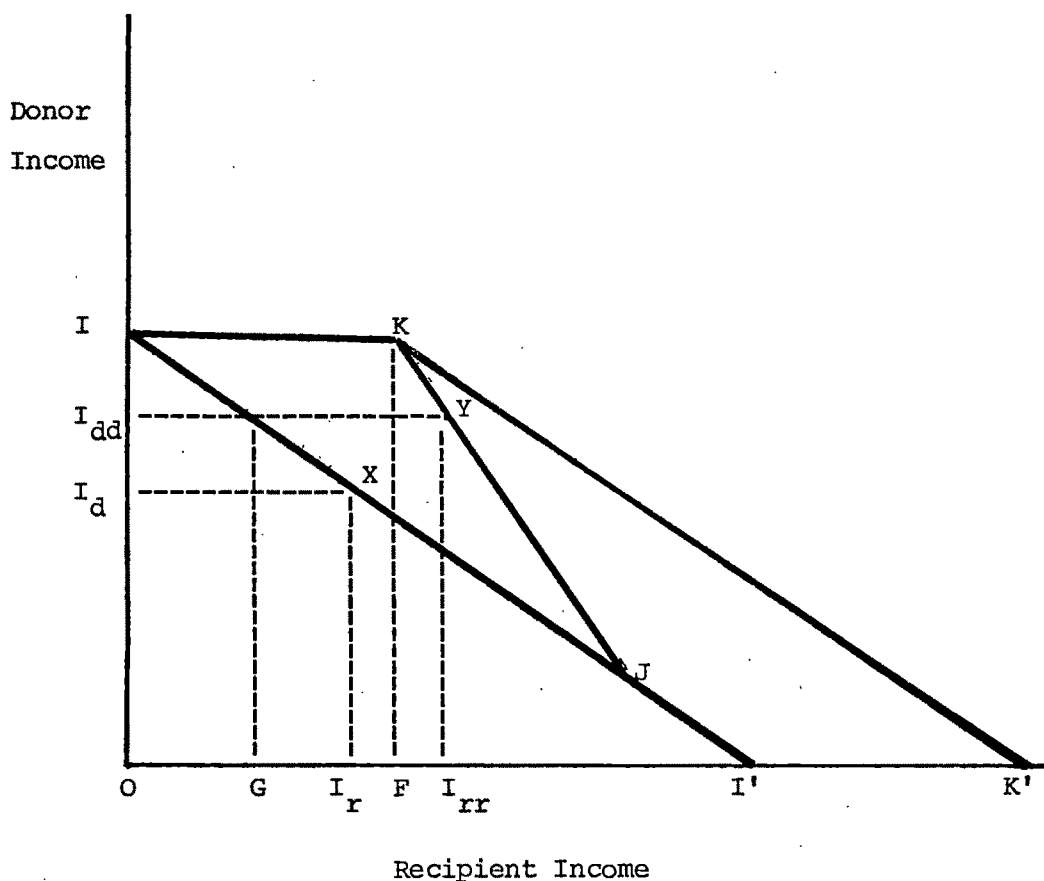
Economic theories of voluntary redistribution derive from the general theory of individual choice with the assumption of interdependent preferences: a person's utility partly depends on the economic circumstances of other persons who, in this context, are less fortunate than he. The individual choice is the size of the welfare guarantee he is willing to finance.

Figure 2 portrays the key financial factors in this decision.¹⁰ For simplicity, consider a case with one recipient, *R*. One of many potential donors (taxpayers), *D*, is choosing his preferred guarantee. Suppose the tax price of providing one dollar to *R* is *t*, a figure less than unity because other taxpayers share the cost. Given income *I*, *D* could consume all of it privately and provide nothing to *R* (point *I*). Alternatively, he could choose to give it all to *R* and have nothing for himself (point *I'*, where $I' = I/t$). In principle, he could select any point on budget constraint $I'I$, which has a slope of $-t$. Given the financial constraint $I'I$, his preferences for redistribution will determine the actual point he chooses. If *D*'s utility depends partly on *R*'s income, *D* will maximize his own utility by locating at, say, point *X* along $I'I$ and being willing to provide *R* with income *I_r* at a private cost of $I - I_r$. In the absence of interdependent preferences, *D* would choose point *I* instead.

If *D*'s income increases, the budget constraint shifts out parallel to $I'I$. This will usually lead *D* to provide a higher income to the recipient by sharing a portion of his increase. The theory does not predict the extent of sharing, however. Holding income constant, if the tax price rises, the constraint pivots to the left around point *I*. *D* would

¹⁰This discussion rests largely on Orr's (1976) work, which should be consulted for a formal exposition. The treatment of food stamps is new, though related to Orr (1979). The seminal theoretical contribution appeared in Hochman and Rodgers (1969).

Figure 2. Taxpayer Equilibrium Choice of Welfare Guarantee



probably reduce the amount of redistribution he is willing to buy when its price rises.¹¹

Suppose now that R is eligible for the federal food-stamp program as well as the cash benefit provided by D and other state taxpayers. The food-stamp program changes the constraint in an important way and hence affects D 's choice. Assume D ignores the tax cost of food stamps, which are paid almost entirely by out-of-state citizens. Further, assume that D views a dollar of aid in the form of food to be equivalent to a dollar

of cash aid. Under provisions of the food stamp program, even if D gives R nothing in cash, R will receive F dollars in food stamps, where F is the federally established food-stamp guarantee. If this were the only feature of food stamps, the constraint would simply shift parallel to $I'I$ over to $K'K$. But in addition, the benefit reduction rate of food stamps changes the constraint's slope. According to federal rules, as R 's cash income including the welfare benefit financed by D rises, his food stamp benefit will be reduced by f , the food stamp tax rate. Therefore, given the tax price of cash aid, t , the true effective tax price to D of raising the full aid package by a dollar is: $t/(1 - f)$, which exceeds t . The constraint's slope returns to t (at point J on Figure 2) if cash aid exceeds the food-stamp breakeven level of F/f .¹²

¹¹This proposition does not contradict our earlier conclusion that the effect of local cost sharing on state policy would be ambiguous. The crucial difference there was that county and local cost sharing does not alter the total cost to the state's citizens of welfare, but only shifts the distribution of cost among governments. There is, then, no economic reason to expect greater or lesser benefits since the budget constraint is not changed. Any effect can only arise from the interplay of political factors discussed in the text.

¹²The breakeven point is where net food-stamp benefits reach zero. Any AFDC recipient is always eligible for a minimal amount of food-stamp aid, even if cash

With the new constraint $IKJI'$, suppose D selects point Y to maximize his utility. The new guarantee I_r delivers OG in cash and GI_r in food stamps. Clearly the net food stamp benefit GI_r will always be less than the food-stamp guarantee OF . One would expect I_r to exceed I_f owing to a positive income effect from the food-stamp guarantee. The tax price increase due to f creates an income effect that reduces desired welfare support, but it is dominated by the positive income effect. The price change, however, induces a substitution effect which tends to lower the desired total cash and food-stamp guarantee. Observe that in Figure 2 the recipient obtains a higher total guarantee while at the same time D 's taxes decline to $I - I_{dd}$. Donor and recipient both may gain from the federal program.¹³

If food stamps are offered only to a portion of AFDC families, as occurred before 1975 when the program was a county option, constraint $I'JKI$ is altered. Suppose the fraction of recipients covered by the program is p . Then F falls to pF ; $K'K$ shifts to the left, accordingly; and the net tax rate becomes $t/(1 - pf)$. The food-stamp breakeven level does not change.

Medicaid can be straightforwardly incorporated into the diagram. State and federal revenues jointly fund this program, and in all but a handful of states the tax price of Medicaid is identical to that of AFDC. Medicaid benefits do not decline as cash or food-stamp income rises. Thus, I_r can be reinterpreted as the sum of the AFDC and Medicaid guarantees.

We conclude that three financial variables affect the choice of I_r : donor income, the net tax price, and the food-stamp guarantee. Assuming that legislators understand the magnitude of the constraint, these three variables have direct effects on the guarantee (paths 12, 13, and 14).

In a polity with many recipients the number of them relative to the taxpayer population will affect D 's choice of guarantee per recipient. Holding constant the tax price and total amount of income that D is willing to sacrifice, a higher recipient-to-taxpayer ratio implies there will be a smaller guarantee per recipient (path 15).

Along with these financial considerations, taxpayers' preferences will influence the size of the

guarantee. For example, given $I'I$, taxpayers with stronger tastes than D for redistribution might desire a guarantee larger than I_r . In the context of welfare policy, preferences can be usefully split into two dimensions. First, preferences may take into account recipient characteristics and the nature of recipient-taxpayer interactions. Taxpayers' feelings of sympathy may depend upon their perceptions of recipients' deservingness (e.g., morality, as evidenced by the frequency of out-of-wedlock births) or demographic traits such as race. Willingness to provide aid may be greater if taxpayers frequently encounter the poor and are distressed by evidence of destitution, malnutrition, and bad health, or by unsanitary, unsightly slums. Because recipient characteristics can be easily observed by lawmakers or communicated to them by interested groups, such characteristics will directly influence the decision (paths 16, 17, 18).

Second, holding recipient traits constant, the strength of voters' basic altruistic tendencies matter in choosing a guarantee.¹⁴ We discussed this aspect of preferences and its role in the public choice process earlier (paths 4, 7).

Estimation Method, Equations, Data, and Measurement Model

The structural model of Figure 1 can be expressed by a recursive set of equations:

$$IPC = b_1 DIV + e_1 \quad (1)$$

$$LIB = b_2 DIV + b_3 PREF + e_2 \quad (2)$$

$$INT = b_4 DIV + b_5 LIB + e_3 \quad (3)$$

$$PTY = b_6 PREF + b_7 LIB + b_8 INT + e_4 \quad (4)$$

$$GUAR = b_9 INT + b_{10} PTY + b_{11} IPC + BX + e_5 \quad (5)$$

where

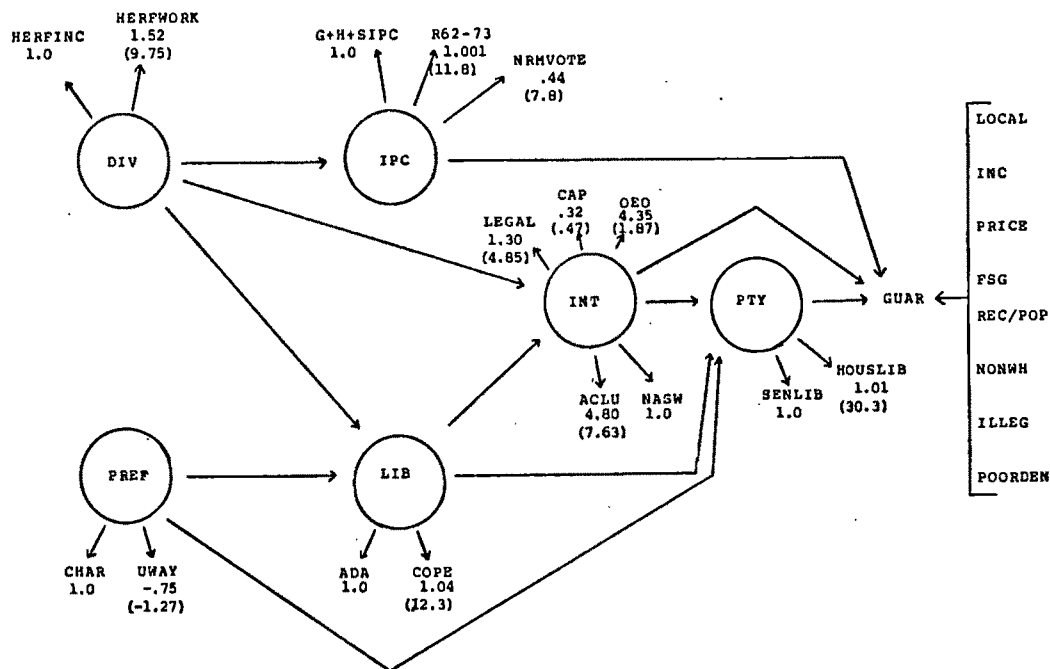
DIV	= social diversity,
$PREF$	= voter preferences,
IPC	= interparty competition,
LIB	= explicit liberalism,
INT	= interest group strength,
PTY	= liberal party control,

income exceeds the breakeven level. We ignore this complication since no actual AFDC guarantee is high enough to create this situation.

¹³For donors initially choosing low levels of cash aid, the food-stamp program may move them to K . This is not most donors' preferred choice (if public policy bears even a faint resemblance to majority wishes) because no state has reduced its AFDC guarantee to zero since food stamps became a national program.

¹⁴Strictly speaking, there is no true altruism in the model. Donors provide aid to increase their own utility. However, while aware of the semantic and philosophical quicksand surrounding the notion of altruism, we see little objection to calling such motives altruistic.

Figure 3. Empirical Specification for the Full Model with Results on the Measurement Model for Latent Variables^a



^aFactor loadings appear below each indicator name. Indicators assigned as unit of measurement have loadings fixed at 1.0, and, thus, no standard errors. Asymptotic *t*-statistics for other loadings are in parentheses.

GUAR = annual *G* + *MED* + *FS* for a four-person family (see above),

X = a vector of other exogenous variables discussed below and the *e*'s are error terms. We assume the equation errors are mutually uncorrelated.

The data for estimating this model consist of state-by-state observations on welfare guarantees and a wide variety of observable indicators and exogenous variables. Minnesota and Nebraska are omitted because of their formally nonpartisan legislatures. Data are for the 1971-1972 period (*N*=96). We deflated all monetary variables to constant 1967 dollars.

We had to modify the key dependent variable *GUAR* because food stamps were not available nationwide before 1975. Instead each county had the option of offering them. Thus, in measuring a state government's AFDC and food-stamp benefit package, we should multiply the food-stamp portion of the total by the fraction of AFDC households living in counties with this program. Such information is not available. As an approximation of this proportion, we used data on the fraction of counties offering food stamps in each

state (*AVAIL*) for years before 1975. The dependent variable used in estimation was, therefore, $G + FS * AVAIL + MED$.

Four of the endogenous variables—*IPC*, *INT*, *LIB*, and *PTY*—and two exogenous ones—*DIV* and *PEF*—are unobservable theoretical constructs. One can estimate their coefficients in the equations, though, by positing a measurement model that specifies observable indicators for these six variables and applying Joreskog's maximum likelihood LISREL method. This method also estimates factor loadings to show which indicators are most closely related to the theoretical construct.¹⁵ We assume uncorrelated errors across all equations in the measurement model. Joreskog (1973) and Joreskog and Sorbom (1982) present details of the statistical procedure. Carmines and McIver (1983) discuss applications to political science.

The LISREL methodology is especially appro-

¹⁵In the Joreskog approach there is really one measurement model for unobservable exogenous constructs and a second for unobservable endogenous ones. The distinction is unnecessary here, though important when setting up the estimation procedure.

priate when there is reasonable concern about the errors of measurement of the variables. Such measurement problems are likely to be substantial in the analysis of comparative state politics. The concept of social diversity illustrates the problem. Although the concept often appears in the literature and there seems to be agreement about its importance, there is little agreement on how to measure it. LISREL allows us to posit multiple observable indicators of the underlying latent construct and, through the use of confirmatory factor analysis, to propose and to test a measurement model of the construct and its indicators.

This section will now discuss the measurement model of each theoretical construct. Then the observable variables, which require no measurement model, are described. Figure 3 reproduces the path model of Figure 1 and adds the indicators of the latent constructs and their estimated factor loadings. Latent variables are circled, whereas observable variables are not.

We have two indicators of diversity (*DIV*). *HERFINC* and *HERFWORK* are modified Herfindahl indices of the concentration of income generated or workers employed by the leading industrial sectors.¹⁶ Higher values of the modified indices correspond to greater industrial diversity. We assigned the unit of measurement for the latent construct of diversity to equal *HERFINC*'s by fixing this indicator's loading at 1.0. *HERFWORK* bears a positive relationship to the index variable. The loading has a *t*-value of 9.75. We examined other indicators: Sullivan's (1973) measure of diversity, the percentage of foreign stock in the state, a Herfindahl index of foreign stock in the states, and a per-capita measure of "who's who in the black community." None showed significant factor loadings.

We lack direct measures of voter preferences for redistribution (*PREF*). We rely on two indicators of concern for others: contributions to United Way charities per capita and charitable deductions claimed on federal income tax returns as a percentage of adjusted gross income. The charitable deductions indicator (*CHAR*) is the index variable. The coefficient on the United Way contributions indicator (*UWAY*) is statistically

significant and unexpectedly negative. This construct is the least satisfactory in the model and may reflect the difficulty in obtaining accurate measures of political sentiments.

Competition between the political parties (*IPC*) is a well-studied aspect of American state politics with a sizable literature. Our measurement model of competition (*IPC*) exploits the features of LISREL. We assume there is an underlying dimension of competition that characterizes electoral conflict in the state. Indicators of this dimension include competition in the last gubernatorial election and the party balance in the state House and Senate (*G+H+SPIC*), competition over the recent past (*R62-73*) as measured by indices developed by Austin Ranney (1976) for statewide political competition, and a measure of expected party balance (*NRMVOTE*). We cannot confidently point to any single one of these measures and claim that it fully measures competition. Rather, each is an imperfect indicator of the underlying dimension. The measurement model works well. With *G+H+SPIC* as the index variable, both factor loadings are positive as expected and quite significant. The *t*-values exceed 7.8.

Indicators for the organizational and political strengths of groups directly interested in welfare benefits (*INT*) are state memberships per 1000 population in the American Civil Liberties Union (ACLU) and National Association of Social Workers (NASW), and per-capita outlays in the state on Legal Assistance (*LEGAL*), Office of Economic Opportunity Programs (*OEO*), and Community Action Programs (*CAP*). With *NASW* as the index variable, the indicators have the predicted positive loadings, most of which are statistically significant.

We constructed two indicators of liberal party control (*PTY*). One equals the fraction of a state's upper house which is Democratic multiplied by the AFL-CIO COPE voting index of all federal representatives and senators from that state who were Democrats, plus the corresponding product for Republicans. The result is an indicator of the average degree of liberalism of the state's senate (*SENLIB*), assuming that within-state politicians share fundamental liberal or conservative traits with their congressional counterparts. COPE indices for the state senators themselves would be preferred, but these are not compiled, and we believe they would be similar. Our second indicator is constructed in identical fashion for the state's lower house (*HOUSLIB*).¹⁷ With *SENLIB*

¹⁶The standard Herfindahl index is computed by summing the squared value of each sector's share. The maximum value is 1.0 if a single sector accounts for the entire population. Our modified variable equals one minus the usual Herfindahl index so that higher values indicate more diversity. We find such an index preferable to a simpler measure such as percentage of state income derived from manufacturing, which ignores where the rest of the income is generated. Our diversity variable accounts for the relative contribution of all economic groups. •

¹⁷Initially we used the fraction of the state's upper and lower house members who are Democrats as indicators of *PTY*. As we and critics of earlier drafts

as the index variable, *HOUSLIB* has a factor loading of 1.01 with a *t*-ratio of 30.3. We also developed a similar pair of indicators using the Americans for Democratic Action (ADA) index. It yielded similar results.

Explicit liberalism (*LIB*) is gauged by two indicators, the ADA and AFL-CIO's COPE indices of voting records for each state's entire U.S. House and Senate delegation. These indicators of the general political climate are admittedly imperfect, well removed from the state capitals, and based on national issues, but are among the few both available and comparable. The two indicators are closely related to one another. With ADA as the index variable, the COPE variable loads 1.04 with a *t*-value of 12.3.

Among the observable variables in the *X* vector, the only political variable is the local cost share (*LOCAL*). It equals the percentage share of benefits paid by county or local governments.

The remaining observable exogenous variables represent elements of the budget constraint or recipient characteristics. Income (*INC*) is represented by real per-capita income. The price of welfare (*PRICE*) equals the state matching rate for AFDC divided by one minus the food-stamp benefit reduction rate, both as established by federal rules.¹⁸ The food stamp guarantee (*FSG*) is the value for a four-person family set in U.S. Department of Agriculture regulations multiplied by *AVAIL*. Although *FSG* is uniform across states in each year, by using pre-1975 data which require the *AVAIL* adjustment, we obtain variation in this variable and can estimate its effect. We obtain recipients as a percentage of taxpayers

(*REC/POP*), following Orr, by dividing the number of AFDC recipients by total state population, both lagged one year to eliminate possible simultaneity between the caseload and the guarantee.

Recipient characteristics that may affect voters' willingness to pay for welfare include the percentage of the caseload that is black (*NONWH*), and the percentage of AFDC mothers with an illegitimate child (*ILLEG*). The number of poor persons per square mile (*POORDEN*) is a proxy for the likelihood of interaction with needy individuals and, thus, discomfort at seeing destitution.¹⁹

Results for the Structural Model

Table 1 presents estimates of the structural coefficients of equations (1) through (5) using $G + FS*AVAIL + MED$ as the dependent variable in the last equation. The proposed link in equation (1) between our constructs of social diversity (*DIV*) and interparty competition (*IPC*) is supported. The coefficient is positive with a *t*-value of 3.7.

The relationship hypothesized in equation (2) received no support. Our construct for charitability of voters (*PREF*) is negatively related to the liberalism (*LIB*) of the political climate, and the coefficient is significant at the .01 level. This results, we suggest, from the imperfect measure of altruism of the states' populations. Although this first effort at measuring voter sentiments at the state level and identifying its effects on policy is disappointing, it demonstrates the need for better measures of this theoretically important variable. The coefficient on *DIV* has the expected positive sign, but its *t*-value of .30 is insignificant.

Equation (3) predicting interest group strength receives modest support. States with liberal political climates have stronger interest group systems (*t*-ratio of 5.1). The relationship with diversity is positive as predicted although not significant (*t*-ratio of .35).

Our explanation for the strength of liberal party control (*PTY*) works well. The coefficients on *LIB*, *INT*, and *PREF* are positive, and the first two are significant at the .01 and .05 levels, respectively.

The last equation is the most important. The expected relationships are generally supported. Interest group strength (*INT*) directly affects the

realized, such indicators are insensitive to regional differences in a party's officeholders' liberalism. This is especially likely to be a problem because of differences between southern Democrats and northern/western Democrats. By following the method described in the text, which explicitly builds in ideological differences, we think we adequately capture the north-south difference.

¹⁸The matching rate is set by federal authorities and varies across states because it is correlated with per-capita income. As Orr discusses, the AFDC matching rate was endogenous for those states which chose to share costs with the federal authorities using the "old" matching formula. With that formula, the matching rate was determined by the average benefit (and, thus, indirectly by the guarantee). To purge the results of any simultaneity bias that could result, we will follow the same two-step procedure for states still on the old formula that Orr used. States that use the newer Medicaid formula face a constant matching rate at all levels of benefit. When the dependent variable is measured as just the cash AFDC guarantee, the price of welfare is not adjusted for the food stamp tax rate.

¹⁹We measure the density of the pre-welfare poor (persons who fall below the poverty line before their welfare income is counted).

Table 1. Coefficient Estimates of the Structural Model^a

(1)	$IPC = 1.87^{**} DIV$ (2.69)					
(2)	$LIB = 50.21 DIV - 76.9^{**} PREF$ (.30) (3.72)					
(3)	$INT = .003^{**} LIB + .18 DIV$ (5.09) (.35)					
(4)	$PTY = .86 LIB^{**} + 38.4^{*} INT + 23.4 PREF$ (4.50) (2.01) (1.47)					
(5)	$GUAR^b = 2263^{*} INT + 732 IPC + .18 PTY + 6.3^{*} LOCAL + .83^{**} INC - 1421 PRICE$ (2.41) (1.55) (.04) (2.17) (4.07) (1.61) $+ .92^{**} FSG - 115^{**} REC/POP - 6.2 NONWH - 17.7^{*} ILLEG + 8.6^{**} POORDEN$ (5.59) (2.85) (1.93) (2.22) (2.75)					
Chi-square for the complete model = 1011 with 256 d.f.						
(6)	$GUAR^c = 2076^{**} INT + 690^{*} IPC + -3.3 PTY + 1.7 LOCAL + .62^{**} INC - 1119 PRICE$ (3.00) (2.08) (1.10) (.84) (4.40) (1.82) $+ .98^{**} FSG - 94.4^{**} REC/POP - 3.3 NONWH - 17.5^{**} ILLEG + 6.0^{**} POORDEN$ (8.52) (3.36) (1.47) (3.15) (2.77)					
Chi-square for the complete model = 995 with 256 d.f. (First four equations under this specification not shown.)						

^aAbsolute values of asymptotic *t*-statistics are in parentheses. No constant terms appear because the equations are derived from a covariance matrix in which variables are measured as deviations from their means.

^bDefined as $G + MED + FS * AVAIL$. *GUAR*, *INC*, and *FSG* are measured in 1967 dollars.

^cDefined as $G + FS * AVAIL$. *GUAR*, *INC*, and *FSG* are measured in 1967 dollars.

*Significant at the .05 level.

**Significant at the .01 level.

annual guarantee and achieves a .05 significance level. Interparty competition (*IPC*) has the predicted positive effect, but achieves statistical significance only at the marginal 12% level.²⁰ The degree of liberal party control (*PTY*) has no discernible effect on the guarantee. Both its coefficient and *t*-statistic are near zero. The final political variable, local cost share, had a significant (*t*-ratio = 2.17) positive effect on *GUAR*. According to our earlier discussion, the positive coefficient implies that the price incentives to state authorities that result from cost sharing dominate the interests of local authorities in controlling local costs.

The coefficients on the economic variables are quite satisfactory. Per-capita income (*INC*) is

positively related to the guarantee and significant at the .01 level. A \$1.00 increase in average income is associated with a \$.83 rise in the welfare guarantee. Because recipients are a small proportion of the total population, this does *not* mean that nearly all income increases are spent on welfare. For example, if 2% of a state's population receives AFDC and the state's matching rate equals 25%, the \$1.00 increase in mean income among the other 98% that leads to a \$.83 rise in the guarantee will cost each taxpayer about \$.005 ($2 * .83 * .25 / 98$), leaving \$.995 for other uses. Finding that welfare guarantees rise with income supports the interdependent preference framework.

The matching rate, or *PRICE* of welfare to the state, has the anticipated negative effect and is marginally significant at the .11 level. If the rate rises by .01, the guarantee falls by \$14.21. The food-stamp guarantee, *FSG*, has a strong (*t*-ratio = 5.59) positive impact on the dependent variable. Raising *FSG* by \$1.00 leads to an increase in the full welfare guarantee of about \$.92. This implies that an extra dollar of federal support for the

²⁰The seemingly large coefficients on *INT* and *IPC* are consistent with the units in which these constructs are measured. For example, *IPC* is indexed to the $G + H + SIPC$ indicator, which can only range between 0 and 0.5.

needy in the form of food (recall, food stamps are entirely federally funded) ends up helping both the poor, who enjoy a rise in the overall guarantee, and taxpayers, who reduce the cash-Medicaid part of the guarantee by \$.08 and thereby save on state taxes. An outcome in which both sides gain from a federal program makes political sense.

The recipient/population percentage (*REC/POP*) has the expected negative impact on the guarantee. A one percentage point increase in the caseload burden, relative to state population, tends to lower *G* by \$115 or about 3.7% of the mean *GUAR*, which was \$3107. This effect is significant at the .01 level.

Along with these effects of the budget constraint parameters, some recipient characteristics also affect legislative decisions on *GUAR*. The percentage of the caseload that is nonwhite (*NONWH*) has an effect on *GUAR* that is significant at the .06 level. A one point rise in this percentage tends to lower *GUAR* by \$6. Interestingly, the extent of illegitimacy (*ILLEG*) among welfare recipients has a larger negative effect on welfare generosity, which achieves significance at the .05 level. A one-point rise here tends to reduce *GUAR* by \$18. Finally, density of the poor (*POORDEN*) is positively, significantly (*t*-ratio = 2.75) related to *GUAR*, as predicted. Because these characteristics of the welfare population are significantly associated with the size of *GUAR*, the interdependent preference view is supported.

The *R*-square for the equations predicting the five dependent variables (*IPC*, *LIB*, *INT*, *PTY*, and *GUAR*) are .09, .66, .37, .78, and .71.

We also estimated equations (1)-(5) using the cash guarantee (*G*) and $G + FS \cdot AVAIL$ as alternative and less satisfactory measures of the welfare guarantee. For both estimates, results for equations (1)-(4) were virtually identical to those in Table 1.²¹ Interesting differences did appear in predicting $G + FS \cdot AVAIL$ as reported in equation (6) in Table 1. The coefficient for *IPC* achieves significance at the .05 level, whereas the effects of *NONWH* and *LOCAL* fade to statistical insignificance. The coefficient on *PRICE* becomes nearly significant at the .05 level. Coefficients on *INT*, *REC/POP* and *ILLEG* become more highly significant. In equation (5) the only insignificant variable in predicting the fully adjusted guarantee other than *IPC* was our measure of political party control (*PTY*). In the estimate using $G + FS \cdot AVAIL$, the coefficient on *PTY* remains small and insignificant, although perversely, its sign becomes negative.

²¹Full results for specifications for *G* and $G + FS \cdot AVAIL$, and details on the data used in all estimates are available from either author.

Conclusion

We conclude with a discussion of the advances made in this article toward a politico-economic theory of income redistribution.

We have argued that the legislatively established welfare guarantee is the appropriate focus of politico-economic analysis. Analyses that have used indirect measures such as payment per recipient, size of recipient population, payments per \$1000 of personal income, or even broader variables of the aggregate redistributive impact of all expenditure and revenue programs are flawed. They focus on variables that are several steps removed from the actual political choices regarding redistribution. The adjusted guarantee is an explicit figure most proximate to the political process.

The approach we have taken yields other gains. Our general theory of redistribution gives a theoretical basis for the oft-observed correlation between economic variables (e.g., personal income or industrialization) and policy choice, and hence bypasses a problem that afflicts the political literature. From our perspective, the conflict between economic and political explanations of redistribution is an artificial one. Redistribution is not simply a function of economic or political factors, but is a result of legislative decisions shaped by voter preferences for transfers, interest group pressures for and against assistance, party competition, and the financial condition of the states.

Secondly, the discussion of diversity and voter preferences as related to the traditional state politics concept of socioeconomic development adds an important theoretical distinction heretofore missing in the literature. According to our model, variables that traditionally measure socioeconomic development properly should be broken down into 1) those financial and economic factors that directly shape voter choices, such as income, urbanization, and proximity to the poor, and 2) a broader collective or community notion that has here been labelled diversity. The former operate directly on the voters by shaping their ability and willingness to pay, whereas the latter is a property of the system that bears on the character of political institutions.

Last, the model properly incorporates the price incentives and financial constraints that condition individual and institutional choice.

The least satisfactory aspects of our analysis are the weak empirical links among our measures of redistributive preferences, diversity, and party, and their hypothesized effects. Our behavioral measures of statewide charity are unsatisfactory. Contrary to prediction, per-capita contributions to the United Way charity and federal tax deductions for charitable contributions are nega-

tively related. Thus, further search for non-obvious measures of this important determinant of party and group political behavior is in order. Similarly, our measure for diversity is not linked to interest group activity or to liberalism in the predicted manner. We may need to examine several different dimensions of diversity as the analysis by Sullivan (1973) implies. Finally, the insignificant link between liberal political party control and redistribution is puzzling. The erratic effect of party on policy is a common finding in the literature (Jones, 1973; Winters, 1976). However, others point out that the effect of party may be quite indirect, possibly being felt only via interaction between party control and competition (Jennings, 1979), or among party control, party liberalism, and changing control.

Underlying our entire analysis has been the search for an integrated politico-economic theory of redistribution. We have not sought to compare and contrast political and economic explanations of redistribution, nor have we tried to demonstrate empirically that one approach is better than the other. Instead, an inclusive theory must address both the choices of individuals with diverse attitudes toward welfare and with varying resources acting as voters, members of interest groups, and supporters of political parties, and the aggregation of these choices via states' political institutions. We believe that the theory presented here takes a major step in this direction.

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Modernization and Traditionality in a Multiethnic Society: The Soviet Case

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Exposure to modernizing institutions such as the school, factory, and city has been found to be associated with the breakdown in traditional values, beliefs, and behaviors in a wide variety of cultural settings. Socioeconomic change has had a similar impact on the late-modernizing Islamic minorities in the USSR. Although patriarchal attitudes and behaviors have persisted in some areas of family life, extensive shifts in family values and approved gender roles have occurred, particularly in the last two decades. These trends parallel those found in nonsocialist settings.

A growing body of evidence on the impact of modernizing institutions in cultural settings from Bangladesh to Boston suggests that exposure to institutions such as the school, factory, and city tends to be associated with a breakdown in traditional values, beliefs, and behaviors. The most ambitious attempt to explore the syndrome of individual modernity is Inkeles's and Smith's (1974) six-nation study. Their results show that males with the most exposure to modernizing institutions tended to score higher on a "modernity" scale. A sampling of modernity studies shows a similar link between modern institutions and individual modernity in many different national settings and social groups: adult male factory workers in Mexico and Brazil (Kahl, 1968); schoolchildren in Brazil (Holsinger, 1973) and Puerto Rico (Cunningham, 1974); schoolboys in Nigeria (Armer & Youtz, 1971); urban males in Pakistan (Ghazanfar, 1980); male parents in Ghana, India, and Brazil (Park, 1980); two generations of married men in India (Daykin & Hertel, 1978); teenage males in Mexico and North Carolina (Godwin, 1975); families with adolescent children in Tunisia (Klineberg, 1973); male workers in Tunisia (Sack, 1973); and blacks in Boston (Suzman, 1973).

The modernity literature has been criticized on both conceptual and empirical grounds. Some critics have argued that the measures of modernity

are more accurately described as measures of western values (Armer, 1977a; Portes, 1973a). Others have criticized the assumed unidimensionality of the concept, the validity of the scales, and the methods used (Armer, 1975, 1977b; Armer & Schnaiberg, 1972; Coughenour & Stephenson, 1972; Godwin, 1975; Gough, 1977; Guthrie, 1970; Hagen, 1975; Rau, 1980; Stephenson, 1968). Still others have noted that much of the work on the relationship between modernization and modernity rests on observation of male subjects; the implicit assumption of research that ignores females is that the primary vehicle of value change within a given culture is the changing values and attitudes of its male component (Klineberg, 1973; Portes, 1973b; Rosen & LaRaia, 1972; Weisner & Abbott, 1977). Nonetheless, there is little doubt about the basic finding of the modernity literature: the process of modernization is associated with a breakdown in traditional attitudes, values, and to a lesser extent behaviors (Armer & Isaac, 1978; Kumar & Waisanen, 1979; Nachmias & Sadan, 1977; Sutcliffe, 1978; Trivedi, 1977).

To date, most individual modernity studies have focused on nonsocialist societies. There has been no systematic attempt to test the hypothesized relationships between modernization and traditionality in the Soviet Union. This is unfortunate because the USSR provides a particularly intriguing test of the individual modernity thesis. The Soviet Union is a multiethnic society whose minority citizens are separated from the dominant Russians by language, culture, lifestyle, and modernization level. These differences are particularly striking for Islamic minority groups in the USSR's less-developed southern provinces. The Soviet leadership has attempted to ameliorate the most glaring socioeconomic disparities, energetically endorsing a series of affirmative action programs

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explicitly designed to help its late-modernizing ethnic groups and regions to catch up with the more modernized Russians. These efforts have met with some degree of success (Bahry, 1983; Bahry & Nechemias, 1981). Although progress has been uneven, the less-modernized minorities are being drawn more fully into modern Soviet life (Jones & Grupp, 1982, 1984).

Has the modernization process resulted in the value change observed in nonsocialist settings? In particular, has increased exposure to modernizing institutions produced a modification of the traditional values and behavior patterns characteristic of the USSR's Islamic minorities? The major difficulty in evaluating the extent to which modernization has resulted in value change is the availability of appropriate data. The individual modernity studies conducted in nonsocialist settings generally employ psychological tests, social surveys, or both to identify the modernity levels of individuals. Clearly, the most desirable way to test the relationship between exposure to modern institutions and traditional values in the USSR is to develop analogous instruments and to administer them to samples of Soviet citizens. The closed nature of Soviet society, however, precludes this approach.

There are alternative strategies. First, we isolated one dimension of modernity that would serve as the focus of investigation. In this study, we concentrate on that dimension of modernity/traditionality relating to sex roles and family values. We chose these values as a focus for research because the way people feel about home and family are among those values most resistant to change (Portes, 1973b; Stephenson, 1968).² Moreover, traditionality in family life represents one of the most dramatic characteristics distinguishing the late-modernizing Soviet Muslims from their more Europeanized counterparts in the north and west. Both Soviet and pre-revolutionary ethnographic studies underline the strength of patriarchal family values among Soviet Muslims.³

¹The terms Muslim and Islamic are used throughout the article to refer to cultural heritage, not religious belief. The major Muslim groups in the USSR include the Azeri, Kirgiz, Kazakhs, Turkmen, Uzbeks, Tadzhiks, and Tatars.

²On the relationship of modernity to fertility and family practices, see also Miller and Inkeles (1974); Mukherjee (1977); and Inkeles (1980).

³There were, of course, significant variations in the pre-revolutionary social systems that produced important ethnic, regional, and socioeconomic class differences in the social position of Muslim women. In general, the social systems of nomadic and seminomadic Muslim peoples, such as the Kazakhs and the

Families operated as an economic unit of production; the oldest male, generally considered to be the head of the family, controlled the family's resources and property. The younger family members were required to defer to the elders, females to males (Sukhareva & Bikzhanova, 1955). Women were assigned an exclusively domestic role and derived social status from their position as wife and mother. Mothers of large families were considered particularly blessed, especially those who had many sons. Unquestioning obedience and fidelity to the husband and male head of household was mandatory and enforced by both physical fear and the wife's economic dependence on her husband. In short, pre-Soviet Muslim culture involved very rigid gender roles that limited females to those of wife, mother, and homemaker.

Examination of change and continuity in family traditionality in the Muslim southern tier thus provides a promising test of the individual modernity hypothesis. If modern institutions have operated in Soviet Asia as they have in other national settings, there should be considerable change in traditional family roles and values: those individuals with the most contact with modern institutions (city, school, and factory) should have the most modern orientations toward the family.

The alternative hypothesis is that exposure to modern institutions has produced little change in values and basic life styles, despite the efforts of Soviet authorities to promote such change. Indeed, some western observers of developments in the USSR's Muslim borderlands are dubious of the Soviet ability to modify traditional value systems among Islamic groups (Rywkin, 1982). These doubts are most emphatically embodied in Helene Carrere d'Encausse's concept of *Homo islamicus*:

Kirgiz, involved somewhat more egalitarian sex roles. Women's roles in traditional oasis cultures of Soviet Asia, characteristic of Uzbeks and Tadzhiks, were more strictly circumscribed (Bikzhanova, 1959; Gadzhieva, 1971; Kislyakov, 1954; Lobacheva, 1975; Luzbetak, 1951; Smirnova, 1968). Women practiced veiling, and homes were frequently divided into men's and women's quarters to conform with Islamic law forbidding a woman to show her face to men other than her husband and close relatives. Even among oasis peoples, however, sexual segregation was less strict in rural areas. Similarly, patriarchal traditions and sexual segregation were practiced more fully among higher status families (Bacon, 1980). These ethnic, regional, and class differences in sex roles among Muslim peoples are important because they help to explain the varying degree of success enjoyed by Bolshevik programs to "liberate" Muslim women.

Without doubt in every modern society, urbanization, improved transportation, education, and the media all serve to break down barriers, to alter and standardize human thought. In the USSR, however, despite these factors which are an integral part of modern life everywhere, despite a continuous effort by the political system to encourage interethnic contacts and dispel differences in Soviet political culture, it seems clear that in the Moslem regions the attempt to transform human thought has run into an almost impenetrable socio-cultural situation. (Carrere d'Encausse, 1979, p. 249)

If this emphasis on resistance to modernization is correct, the USSR's Islamic minorities represent an exception to the individual modernity theory. This would be an important exception for two reasons. First, Soviets of Muslim heritage represent a substantial and growing proportion of the USSR's non-Russian population. Developments affecting these groups will surely have a salient impact on cultural patterns in the Soviet Union as a whole. Second, and more important from the perspective of comparative research, the USSR's Islamic groups share a core of cultural values with many Muslim groups beyond the Soviet borders. Trends affecting Soviet Muslims have much to tell us about how other Muslim populations may react to the impact of modernization.

To test these competing hypotheses, we used two types of data: the published results of Soviet surveys and aggregate behavioral indicators of female roles. Relevant Soviet surveys include demographic studies of ethnic differences in expected and desired family size and family planning use. These surveys tend to be based on very large samples of Soviet citizens and are likely to be representative of the larger population. Also available are ethnosociological studies that include questions on attitudes toward employed wives and mothers, communal child care, the distribution of household tasks, family decision making, and parental authority. These questions tend not to have unambiguous "right" or "wrong" answers. Although official Soviet statements often idealize large families and discourage family planning, many official Soviet commentators endorse medium-size families. Official attitudes toward employment and motherhood are similarly ambiguous. In other words, the findings of the survey questions in these areas are not apt to be skewed by respondents who give what they perceive to be the "expected" response. Findings from these surveys provide useful data for evaluating the individual modernity theory. If this theory is correct, those Soviet Muslims who have been most exposed to modernized institutions should display attitudes similar to those of their counterparts in the USSR's European regions.

Soviet surveys have two major shortcomings as a source of information on values. The first is that the raw data collected by Soviet researchers are not available to western scholars for secondary analysis. The problem with this is not that the Soviet researchers select out findings that are not in conformity with regime expectations; indeed, much of the data presented by Soviet social scientists runs counter to official ideals. Rather, the problem is that results are most often presented in bivariate format. For example, Soviet authors often present tables showing age differences in response to a particular question, while additional tables may present the results broken out by educational level. Without access to the raw data, however, one cannot investigate how age and education might interact with each other (or with other variables such as social class, gender, or ethnicity). The bivariate results are interesting and informative, but they do not permit multivariate modeling of the individual modernity process.

A second shortcoming of these data is that they are cross-sectional. We cannot examine trends over time and, therefore, demonstrate that the respondents are becoming more modern. For that we must rely on aggregate behavioral indicators of female participation in modern Soviet life. Also examined are the relationships between these behavioral indicators of individual modernity and measures of exposure to modernizing institutions. If the trends and relationships demonstrated by the aggregate analysis are consistent with the findings of the surveys, we can be more confident that our results are not contaminated by the ecological fallacy.

Survey Results

Attitudes toward Family Limitation

One of the most graphic reflections of traditionality in life styles among Muslims inside and outside of the Soviet Union is the high value placed on large families.⁴ To what degree have traditional preferences for large families shifted under the impact of modernization? Recent Soviet demographic surveys show that ethnic dif-

⁴Preference for large families, however, is not always accompanied by high levels of fertility. Poor health conditions, lengthy breastfeeding, postpartum sexual taboos, and high infant and child mortality often operate to keep fertility and completed family size low (Coale, Anderson, & Harm, 1979). In many developing regions, birth under-registration keeps reported fertility at artificially low levels (Jones & Grupp, 1983). Reported fertility, then, is not apt to be a good measure of the extent to which a culture values large families.

ferences in family size expectations are still very great; Russian women report an average expected family size of two children; for many Muslim groups, the expected family size is nearly six (Vishnevskiy & Volkov, 1983, p. 187). Nonetheless, age, education, and place of residence breakdowns for these indicators suggest that these differences are eroding in precisely the directions suggested by the individual modernity hypothesis (Belova, 1983; Jones & Grupp, 1981). Younger Muslim women expect much smaller families than their older sisters. Muslim women from urban areas, particularly large cities, report a much smaller expected family size than do their rural counterparts. Educational level is also negatively correlated with Muslim family size. These research findings indicate that urbanity, education, and occupation are having the same natality-inhibiting affects on Muslim women that they had earlier on their European counterparts. When one or more of these characteristics is controlled, ethnic family size differences narrow dramatically. For example, a survey of young, unmarried women in Baku (a large city in the Muslim southern tier) found only marginal differences between the family size desires of the European Russians and the Islamic Azeris (Akhmedov, 1974). Such trends suggest that traditional values regarding the family are slowly eroding among the less modernized groups.

These changes in the value placed on large families have been accompanied by modifications of the entire system of values relating to home, family, and appropriate female roles, including changes in Muslim attitudes regarding family planning, working wives, child care, and the sharing of household chores. Use of family planning methods is of particular importance because achievement of the smaller family sizes that frees married women for a more active occupational role is not possible without broad acceptance of family limitation techniques. Although comparative data on family planning in the USSR are extremely limited, it appears that use of family planning among Muslims, as among other Soviet women, is most widespread among better educated, employed urbanites. For example, a 1972 study of Russian and Kirgiz women in the southern tier republic of Kirgizia showed that among indigenous nationalities, 50% of the urban respondents and 27% of the rural respondents used family planning methods. The corresponding findings for European nationalities were 76% and 69% (Abdullin, 1977).

Muslim women who want to limit their families rely heavily on abortion as a means of family planning. Abortion use among the more traditional women, including Muslims, is directly related to education and status (Dzhangishiyeu,

1973). A study of family formation among urban Muslims with four or more children, for example, found a higher incidence of abortion among the most highly educated women (Katkova & Mamatokhunova, 1976). The results of a study of child-bearing activities in the first seven years of marriage among Muslim women in Alma Ata are similar. Muslim women with higher education averaged 2.28 abortions; those with secondary education only 1.99 abortions. The European and Islamic women in this study reported nearly identical abortion rates (Katkova, 1977). A similar pattern is evident in family planning data from the southern tier republic of Azerbaydzhan. A republic-wide survey of women seeking abortions found that highly educated women and urban residents had the most frequent recourse to abortion (Mamedov, 1976). The direct link between status indicators and abortion use for Islamic women was also found in a sample survey of pregnancy and birth histories in the four Central Asian republics (Uzbekistan, Tadzhikistan, Kirgizia, and Turkmenistan) (Aliakberova, 1976, 1978, 1980). In European areas of the USSR, by contrast, abortion use is inversely related to status, apparently because high-status women in these areas who want to limit their families are making more effective use of contraceptive techniques (Arutyunyan, 1968; Katkova, 1971, pp. 66-72; 1977).

Muslim women make less frequent use of contraception than their European counterparts. In the study of Muslim mothers of four mentioned above, only 16% of the total sample used contraceptive techniques. Although no directly comparable data for other nationalities are available, several studies of family planning practices in other areas of the USSR showed that 70-80% of the couples who have completed their families practice contraception (Darskiy & Belova, 1969; Katkova, 1971, p. 80; Shlindman & Zvidrinish, 1973, pp. 134-145). Higher status women of all nationalities, however, report more frequent use of contraception. One study of Islamic women, for example, showed that 27% of the working wives and only 4% of the housewives reported use of contraceptive methods (Katkova & Mamatokhunova, 1976). In the Azerbaydzhan survey, well-educated women and urban respondents made more effective use of contraceptive methods (Mamedov, 1976). Muslim use of family planning methods of any kind is much less frequent among rural women, particularly housewives (Buriyeva, 1978; Golin, 1971).

Although it emphasizes the resilience of traditional preferences for large families, the data on family limitation are consistent with individual modernity theory. Large families are still popular in the Islamic southern tier regions of the USSR,

and family planning methods are far less widespread than in the European areas to the west and north. Those Muslim women most exposed to modern institutions, however, prefer and expect strikingly smaller families than their less-modernized counterparts, and they are willing to use family-planning methods like abortion that are strongly condemned by traditional Muslim values. Younger, better-educated, urban women and those employed in high-status jobs have been most affected by the erosion in family size values, whereas older, rural, less-educated housewives have been virtually unaffected. This pattern may result partly from the conservatism of rural communities and partly from the fact that religious and cultural disapproval of birth control practices is probably strongest in the countryside.

Attitudes toward Employed Wives and Mothers

Another key aspect of the changing role of women is popular acceptance of employed wives and mothers. Soviet survey researchers typically include at least two questions touching on this issue. In one, respondents are asked which of two family life styles is most attractive to them. Two women with an identical specialty marry and have children. In one family, the wife works; she, the husband, and a grown son all share the housework equally. In the other family, the wife is a full-time homemaker. Because traditional patriarchal values in many Islamic areas have stressed the woman's role as wife and mother, one would expect to find widespread resistance to the more egalitarian life style of the employed mother. Comparative data demonstrate, however, that most Soviets, including those from traditional Islamic areas, favor a combination of career and motherhood. In the Uzbekistan study, for example, 64% of the urban respondents and 72% of the rural respondents favored employment for the wife (Mirkhasilov, 1979b).⁵ In rural Estonia (a European area), 65% of the women and 53% of the men reported a preference for the first lifestyle (Arutyunyan & Kakhk, 1979, pp. 54-55). This pattern of stronger support among females for the employed-mother role was found in Islamic Uzbekistan as well. In both the urban and rural samples, support for the employed mother was stronger among females.

A similar pattern emerged when respondents were asked a second question: whether or not they

favored employment for the mother even if family finances didn't require it or she was "fully occupied" with housework and children. Even under these conditions, 59% of the rural Muslim respondents and 56% of the urban Muslim respondents in Uzbekistan still favored employment outside the home; an "overwhelming majority" of urban Muslim women in Kirgizia endorsed the working mother role (Karakeyeva, 1979, pp. 149-156; Mirkhasilov, 1979a, b). In rural Estonia, 57% of the females and 47% of the males chose this response (Arutyunyan & Kakhk, 1979, pp. 54-55). Higher status, younger and better educated respondents from both European and Muslim areas tended to be more in favor of an active professional life for the women (Arutyunyan, 1973b, 1980, p. 92; Mirkhasilov, 1979a).

Strong support for paid employment was also found among women employed in various fields in a separate study conducted in the Islamic province of Dagestan between 1973 and 1978. Respondents were asked about their attitudes toward the participation of women in the paid labor force; 76% of the urban respondents and 61% of the rural respondents considered such participation "necessary." The European women in this survey reported somewhat stronger support for female employment than their Islamic counterparts; 86% of the ethnic Russian women, compared to 65% of the Islamic Dagestanis, considered employment a necessity for women. Only 3% of the Russians felt that a woman should confine herself only to housework, compared with 26% of the Dagestanis. However, social class, not nationality, was the major factor differentiating employment motives, with women in white-collar jobs more likely to report that they were motivated to work by a combination of financial reasons *and* the desire for self-realization (Abdusalamova, 1981). The Dagestan study is particularly revealing because this area is among the most traditional of all Muslim regions in terms of the gender role measures of native women, yet acceptance of the dual mother-worker role among employed Dagestani women is remarkably strong.

The patterns demonstrated in the data reviewed thus far indicate a relatively high degree of acceptance of the employed wife and mother in the USSR by both European and non-European groups, even when the family does not need her income. How does this compare to attitudes about female employment in other industrialized countries? A 1975 European Economic Community poll of 10 European countries also included questions designed to tap popular attitudes toward working wives.⁶ In this case, each male

⁵The higher rural percentage may reflect the greater ease with which many rural wives and mothers can integrate both family and employment responsibilities; female employment in an urban environment more often means non-agricultural employment.

⁶Euro-Barometer 3. Data made available by the ICPSR.

respondent (married men only) was asked whether he preferred his wife to work, and each female respondent (married women only) was asked whether she preferred to work. Only 41% of the males favored employment for their wives. As in the Uzbekistan data, women more readily endorsed employment; 64% of the married women preferred to be employed. The American public is somewhat more strongly supportive of the full-time homemaker, especially if she has children. A 1978 *Time* magazine survey in the United States found that 77% of the sample agreed that a mother with young children should not work outside the home unless it was financially necessary (Barron & Yankelovich, 1980, p. 61). Because the questions used in these surveys are not identical to those used by Soviet ethnographers, these findings are not directly comparable to the Soviet data, but they do permit us to place the Soviet data in better perspective, highlighting the relatively strong public support for working wives and mothers in the USSR. The impact of modern life, coupled with economic necessity and an energetic socialization program that stresses the working-mother role model, have produced strong approval for female labor-force participation in the Soviet Union, even among the most traditional Islamic groups.

Communal Child Care

Other Soviet data suggest that an important corollary to working motherhood—communal child care—receives generally favorable support among women from traditional southern-tier areas. One study of Islamic women with large families in Uzbekistan showed that although 60% of the respondents favored caring for infants in the home, women with longer employment histories had more favorable attitudes toward communal child care (Katkova & Mamatokhunova, 1976). Strong support for communal child care for older children (ages three and above) were reported in a study of Russian and Muslim (Kazakh) families in the Asian republic of Kazakhstan (Zhelokhovtseva & Sviridova, 1973). An ethnographic survey in the Muslim province of Tataria showed that Russian and Muslim urbanites had virtually the same proportions of children in child-care facilities (Arutyunyan, 1973, pp. 185-186). Although far from conclusive, these findings suggest a relatively high level of receptivity to communal child care, even for very young children, among later-modernizing groups that might be expected to advocate that mothers remain in the home caring for their small children.

Family Life and Divorce

Another useful barometer of family values is the extent to which the traditional division of household labor has persisted even in the face of high female participation in the paid labor force. In the traditional patriarchal families that dominated the social system in Soviet Asia before the Bolshevik takeover, virtually all domestic tasks were the woman's responsibility (Khashimov, 1972, pp. 107-113). Soviet ethnographic studies of Islamic areas published in the 1950s and early 1960s noted that traditional attitudes toward housework had begun to change. By the postwar period, some husbands, particularly the younger ones, had begun to assist their wives in tasks that had earlier been considered women's work, such as child care. This was particularly true in families where the young wife was employed outside the home. But many men, particularly those in the older generation, still scorned such necessary but mundane chores as demeaning (Dzhumagulov, 1960, p. 60; Sukhareva & Bikzhanova, 1955, pp. 190-191).

More recent ethnographic data indicate that even husbands in traditional Islamic families have grudgingly shouldered a more equitable share of the housework. Fifty-eight percent of the urban Muslim respondents in Uzbekistan reported that the husband helped with both the housework and child care (Mirkhasilov, 1979b). Another question, which was asked in several surveys, allowed the respondent to specify the extent of that help. In 21% of the rural Muslim families in Uzbekistan, couples shared household chores equally; in 52% of the families, the husband assisted his wife; and in 7% of the families, husbands did not help at all (Mirkhasilov, 1977, pp. 36-41). These findings are comparable to those for European groups (Arutyunyan, 1973b). Another survey of the distribution of household responsibilities in Islamic Kirgizia showed that Muslim husbands were about as likely to lend a hand with household tasks that are traditionally defined as woman's work (laundry, food preparation, cleaning, and mopping) as their European counterparts. Sharing the household chores was much more common among urban families of both nationalities (Kharchev, 1979, pp. 259-260). Supporting this finding is a mid-1970s study of rural Muslim families; in a third of the families surveyed, the husband did a significant portion of the housework. Husbands whose wives contributed a large portion of the family income were the most likely to take a more active role in household chores (Sushanlo, 1979, pp. 45-51).

The available data on sharing of tasks in Muslim families suggest that patterns of family responsibility here are similar to those in central

and western USSR. Of course what is missing from all of these surveys is an actual breakdown of time spent by each spouse on each task. Individual perceptions of assistance and equal sharing are highly subjective; and Soviet studies that present results broken down by husband and wife demonstrate substantial differences in perceptions, with husbands evaluating their own contributions to housework more generously than their spouses (Yurkevich, 1970, pp. 189-190). Moreover, results of time-budget surveys show huge disparities in the actual amount of time spent by each spouse on domestic obligations. According to a recent survey conducted in eight cities in the European republic of Lithuania, for example, women typically spent far more time on housework than did men (Klichyus, 1980, pp. 32-39). A study of working-class families in Islamic Azerbaijan demonstrated a similar pattern (Abbasov, 1980, p. 57). As such studies make clear, the typical Soviet husband may be fairly willing to lend a hand with household tasks, but the bulk of the time spent by the family on housework is spent by his wife.⁷ Nonetheless, the available information suggests that although the Soviet wife still suffers from a double burden of job and housework, her husband's willingness to share at least some of the domestic tasks depends less upon his nationality than on his social status and place of residence. Traditional Islamic gender role values have not, it seems, spared the Muslim husband from at least a small portion of the housework.

Another area where exposure to modern institutions has resulted in some erosion of traditional views is that of distribution of family authority. To be sure, the traditional view regarding the husband as the head of the family and primary decisionmaker still persists in many areas of the Soviet southern tier (Gantskaya, 1974, 1977; Mirkhasilov, 1979). However, these traditional patterns of family authority are less pronounced for the younger and better-educated members of Islamic groups. For example, in a 1969 survey of rural Muslim couples in Dagestan, younger husbands were more likely to consult with their wives and placed a higher value on her opinion than did the older-generation husbands (Abdurakhimov, 1970). Ethnographic surveys in Muslim Uzbekistan indicate that, as in European settings, more modernized couples were more likely to share

family decision making (Arutyunyan & Drobizhiva, 1976; Mirkhasilov, 1979b). In Islamic Kirgizia, researchers found only marginal differences between Kirgiz and Russian attitudes on whether the husband should always act as head of the family (Attokurov, 1983, pp. 94-96).

Traditional values remain much stronger with regard to divorce. Even the more modernized Muslim couples are considerably less willing to terminate an unhappy marriage than European couples (Drobizheva & Tul'tseva, 1982). A higher percentage of Uzbeks reject the possibility of a divorce if the couple has children; 87% of the rural Uzbeks and 84% of the urban Uzbeks rejected divorce under these conditions. This compares with significantly lower rates among the more modernized European groups: 73% of the urban Georgians, 67% of the Moldavians, and 51% of the Estonians (Arutyunyan, 1977, pp. 244-246; Drobizheva, 1981, pp. 164-165; Mirkhasilov, 1979).⁸ Age-specific data from the Estonian study suggest that the widespread acceptance of divorce in families with children occurred only recently; older respondents were more insistent on keeping the marriage intact if the couple had children (Arutyunyan & Kakhk, 1979, pp. 51-52). The strong Islamic insistence on keeping the family together has affected the attitudes of ethnic Russians who live in these regions. Of the Russians residing in Uzbekistan, 63% rejected divorce if children were involved; this compares with 54% of the Russians residing in the Russian republic and 50% of those residing in Estonia, one of the most westernized of the Soviet republics (Arutyunyan & Kakhk, 1979, pp. 118-119).

Parental Authority

Another area that for at least some Muslim minorities has been relatively insulated from the impact of modernization is the pattern of attitudes toward the older generation. To measure values relating to parental authority, Soviet ethnographic surveys typically ask whether the respondent feels that parental approval is needed to get married. Respondents from the southern tier area, both Muslim and non-Muslim republics, are considerably more deferential to parental authority than are their counterparts in European USSR (Arutyunyan, 1977, p. 245; Arutyunyan & Mirkhasilov, 1979; Mirkhasilov, 1979b). The contrast between the European Estonians and the Muslim Uzbeks is particularly striking. Compared

⁷Soviet women are not alone in their double burden. A 1978 *Time* survey in the United States found that over half of the respondents felt it was "the wife's responsibility to make sure that the house is clean and neat, even if she works as hard as her husband" (Barron & Yankelovich, 1980, p. 80).

⁸In a 1978 U.S. survey, only 39% of the respondents rejected divorce if young children were involved (Barron & Yankelovich, 1980, p. 84).

to 92% of the Uzbeks, only 25% of the Estonians felt that parental approval was required for marriage. For European groups, the effect of modern institutions (i.e., urban residence) is to erode the perceived need for parental approval. This has not occurred among Islamic groups. Adherence to traditional values that mandate parental approval before marriage is only slightly lower among Uzbek urban dwellers than their counterparts in the country. Moreover, the high value placed on parental approval is still very strong even among the youngest Uzbeks surveyed: 90% of the 20- to 24-year-old rural Uzbeks claimed that parental approval was a prerequisite to marriage—a proportion only marginally lower than the 94% of the over 60-year olds who voiced the same opinion (Arutyunyan, 1980b). As with attitudes toward divorce, the prevailing value system that mandates deference to older generations in the Islamic regions appears to affect the attitudes of Russians who reside there. Among Russians, only 35% living in Estonia and 38% living in the RSFSR felt that parental approval is mandatory, compared to 55% of those Russians living in Uzbekistan (Arutyunyan & Kakhk, 1979, pp. 118-119).

These findings demonstrate both change and continuity in measures of traditionality. Exposure to modern institutions is indeed associated with more modernized family life styles; the more Europeanized Muslims—the better educated, younger urbanites—are more Europeanized in many aspects of family life. Other facets of traditionality, however, have been more resistant to the impact of modern life (Arutyunyan & Drobizheva, 1981). Respect for parental authority, for instance, is still much stronger among Islamic groups than their European counterparts; youth and urban residence appear to have only a marginal influence on this core value.

Behavioral Indicators of Family Values

The cross-sectional data summarized above permit us to examine variations in the strength of traditional family values within late-modernizing ethnic groups and to compare and contrast traditionality between European and non-European minority groups. Those data do not, however, allow an assessment of trends over time, because the Soviets have not released either time series data or the results of panel studies. To evaluate changes in life styles, we must turn instead to behavioral indicators of family values that are reported in the aggregate for nationalities. Three behavioral measures are examined below: female access to education, female labor force participation in high status occupations, and early marriage.

Female Education

One manifestation of the strength of traditionality in gender roles in the Muslim southern tier regions was the widespread resistance to the Bolshevik program of sending young girls to school (Vagabov, 1968). Female access to education, therefore, provides one measure of traditionality in family values. The use of female educational opportunity as a surrogate measure of traditionality finds strong support in survey results from nonsocialist settings. Scanzoni (1975) found that education levels and the rejection of the traditional-wife role were positively correlated among American wives, except for older Catholics. His findings were confirmed by Bagozzi and Van Loo's (1980) reanalysis of the data. Similar links between the woman's acceptance of more egalitarian sex roles and her education level have been reported by other researchers (Jones, 1980; Mason, 1975; Thornton & Friedman, 1979; Tobin, 1976). Nor does the relationship between education and traditionality in family values appear to be limited to western industrial societies. It has also been reported for Taiwan (Sun & Soong, 1979); Brazil (Rosen & LaRaia, 1972); and Ghana (Smock, 1977).

Time series data on female educational attainment in the USSR show that more restrictive attitudes toward women's roles in the Islamic southern tier are mirrored in relatively low female educational attainment. However, considerable change has taken place in the past two decades among late-modernizing groups (Table 1, cols. 1-4). Generally speaking, minority women who were less well-educated than Russian women have made significant gains in the percentage of women over age 10 with completed secondary education, college education, or both. To be sure, significant differences in female attainment still separate the late-modernizing minorities from their European counterparts, and the Soviet press still contains stories critical of parents who neglect to send their daughters to school and of husbands who interfere with their wife's desire for further education. However, considering how far Muslim women have come in six decades, enhanced female access to education, even among groups traditionally resistant to female schooling, provides persuasive (although indirect) evidence of a substantial shift in popular attitudes toward the proper roles for women among the traditional Soviet minorities.

Female Access to High Status Jobs

Another indicator of female roles is female access to modern, high-status occupations. This measure was chosen instead of female labor force participation *per se* because research findings on

Table 1. Female Participation in Soviet Life (Number per 1,000 Women)

	High School Graduate and Above ^a		College Graduate ^b		Specialists ^c		Early Married ^d	
	1959	1970	1959	1970	1959	1970	1959	1970
Slavs								
Russians	158	258	23	42	106	190	93	91
Ukrainians	113	216	13	28	71	141	101	112
Belorussians	107	202	10	24	71	136	70	76
Balts								
Estonians	165	256	16	40	109	213	42	49
Latvians	155	251	17	37	103	191	45	59
Lithuanians	83	177	11	30	74	164	48	54
Muslims								
Uzbeks	42	136	3	12	20	62	318	217
Kazakhs	51	151	4	17	36	100	287	123
Azeri	86	155	13	22	58	101	278	183
Kirgiz	47	139	4	14	32	77	442	201
Tadzhiks	32	108	2	7	16	40	366	249
Turkmen	37	116	3	9	15	47	320	191
Bashkirs	58	118	4	11	36	86	130	85
Balkars	30	125	1	15	49	101	70	60
Tatars	83	162	9	20	63	118	80	71
Chechens	4	36	0	2	4	19	404	199
Dargintsy	17	55	1	8	NA	NA	208	186
Kumyks	40	114	3	14	NA	NA	214	152
Lezgins	39	101	3	12	NA	NA	185	130
Ingush	15	62	2	7	5	37	149	90
Kabardinians	68	167	4	15	61	92	111	140
Avars	17	64	1	8	NA	NA	202	185
Adygir	84	199	6	27	NA	123	NA	NA
Karachayev	35	126	2	21	NA	93	NA	NA
Cherkess	59	161	2	22	NA	154	NA	NA
Karakalpak	27	106	2	12	NA	64	395	231
Abkhazy	122	231	11	41	NA	121	102	95
Others								
Georgians	284	401	47	75	110	169	107	134
Moldavians	38	98	3	11	33	70	149	119
Armenians	210	308	33	49	96	151	158	152
Buryats	93	206	13	40	84	214	80	40
Komis	112	170	11	21	130	260	69	61
Maris	37	75	3	8	29	65	61	56
Udmurts	62	113	5	12	46	96	70	69
Chuvash	70	127	5	11	45	91	44	63
Yakuts	71	184	7	25	79	176	55	32
Jews	606	687	240	304	NA	452	NA	NA
Mordvinians	42	96	2	7	28	73	71	67
Kalmyk	29	116	2	16	38	115	115	56
Karelians	63	117	4	12	65	144	58	59
Osetins	198	312	24	46	100	170	35	53
Tuvins	26	150	1	18	33	113	157	84
Altay	57	112	4	15	NA	172	NA	NA
Khakas	52	111	6	20	NA	127	NA	NA
Komi-Permyaks	54	94	1	6	NA	NA	NA	NA
CV	1.14	.652	2.77	1.66	.591	.587	.742	.543
Soviet Score	139	234	20	37	90	171	112	106
T value	21.25*		9.19*		12.30*		3.95*	

*P < .001.

NOTES TO TABLE 1:

^aNumber of women with high school diplomas or more education per thousand women aged 10+. Source: *Itogi*, vol. 4, pp. 393-548.

^bNumber of college graduates per thousand women aged 10+. Source: *Itogi*, vol. 4, pp. 393-548.

^cThe number of specialists per thousand women aged 20-54. Specialist data derived from *Zhenshchiny v SSSR*, pp. 76-77. Nationality age groups derived from: *Itogi*, 1970, vol. 4, pp. 360-382; *Itogi vsesoyuznoy perepisi naseleniya 1959 goda SSSR*, pp. 211-225; and *Itogi*, 1959, *RSFSR*, pp. 388-409.

^dThe number of married women per thousand women aged 16-19. Source: *Itogi*, 1970, vol. 4, pp. 383-392.

the relationship between female employment in the paid labor force and the retention of traditional values in nonsocialist settings are mixed (Sanday, 1974; Scanzoni, 1975). There is stronger empirical support for use of female access to high-status occupations. For example, Rosen and Simmons (1971) found a strong relationship between female participation in high-status jobs and non-traditional sex role attitudes. For our test in the Soviet context, we used the "specialist" portion of the white-collar labor force. Specialist occupations range from mid-level status, such as technicians and nurses to higher-status occupations such as scientific researcher, engineer, and physician.⁹

The data on female participation in the specialist workforce (Table 1, cols. 5-6) reinforce the conclusions suggested by the education data. The more traditional gender role values in the Islamic regions are clearly reflected in more restricted female access to high-status jobs in these areas. Nonetheless, dramatic increases in the proportion of working-aged women who held specialist jobs occurred between 1959 and 1970; indeed, almost all of the less-modernized minority groups increased their scores relative to the Russians during this period. There remains, to be sure, a substantial gap between the Russian women and some of their less-modernized counterparts. Almost a fifth of the 20- to 54-year-old Russian women were specialists in 1970, whereas fewer than 2% of the Chechen women were specialists. Clearly the Soviets have a long way to go to integrate the Muslim woman as fully into the specialist labor force as her Russian counterpart in Moscow (Lubin, 1981). Nonetheless, these data provide dramatic evidence that cultural barriers to female participation in the modernized labor force among traditional minority groups are being overcome.

⁹Status ratings for various professions are drawn from Soviet studies of the social prestige of occupations. For example, see Titma (1982, pp. 8-35), Titma (1975, pp. 128, 144), *Molodezh i trud* (pp. 85-88), and Vodinskaya (1969).

Early Marriage

The third indicator of the social role of women is early marriage. The traditional stress on the women's role as wife and mother was coupled in many areas with social pressures to marry at an early age. Early marriage was undesirable to the Bolshevik leadership for both economic and ideological reasons, but legislating the marital age upwards in the Soviet southern tier proved difficult because many Muslim families simply ignored the marriage law (Vagabov, 1980, pp. 139-143). Nonetheless, the proportion of early marriages (in which the bride is younger than 20) decreased dramatically among Soviet Muslims and other traditional minorities during the 1960s (Table 1, cols. 7-8). Fragmentary ethnographic data indicate that this trend continued in the 1970s (Gadirzade, 1983). The marriage data reinforce the conclusion—drawn from the educational and occupational data—that a gradual breakdown in traditional female roles is occurring in the late-modernizing Muslim areas.

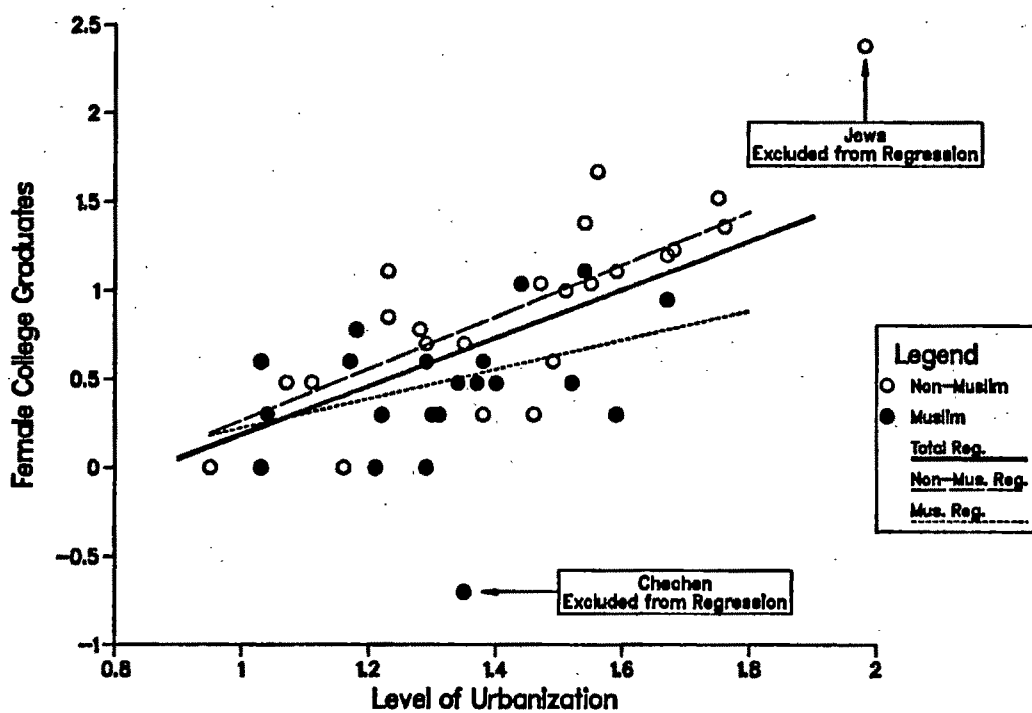
The Statistical Link between Modernity and Traditionality

The survey data summarized earlier indicate that those individuals with the greatest exposure to modern institutions expressed more European, less traditional attitudes on many issues relating to family life styles and sex roles. The census data reported above indicate that female roles have changed over time in the USSR as a whole and among late-modernizing groups in particular. To test the relationship further, we examined the statistical link between one measure of exposure to modern institutions (urbanization) and various measures of female participation in modern life.¹⁰

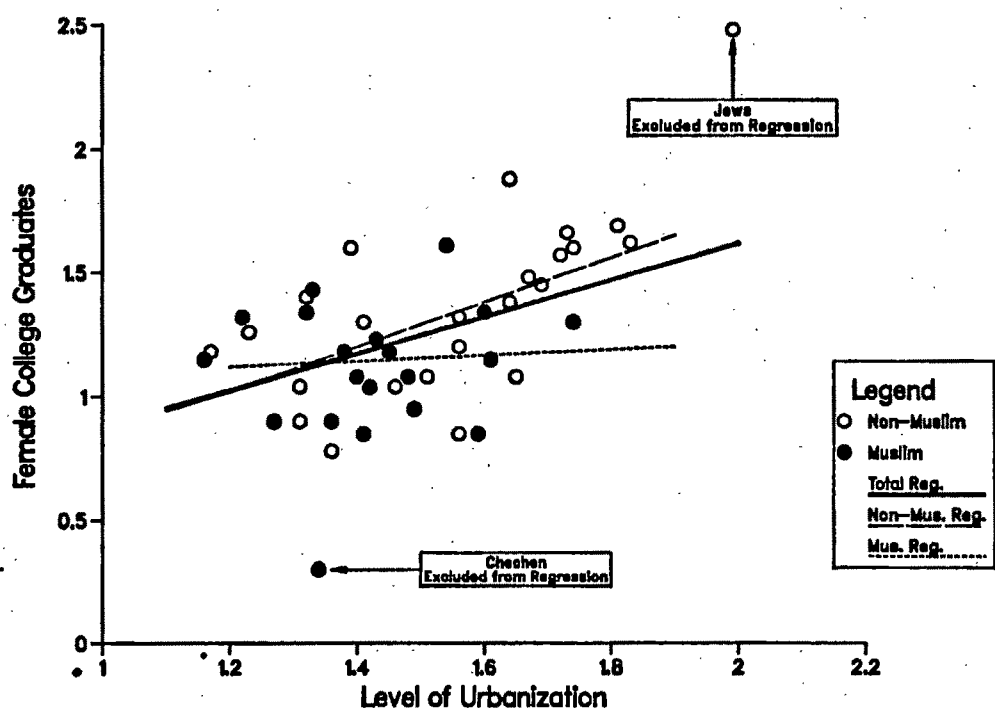
¹⁰Our use of educational and occupational participation as surrogate measures of female traditionality limits our choice of independent variables in explaining the change that took place between the two Soviet censuses to urbanization level. Clearly, we cannot have educational and occupational measures on both sides of the equation.

Figure 1. Urbanization and Female Higher Education
All Variables Logged Base 10

1959



1970



The results of these tests lend limited support to the individual modernity hypothesis.

The strongest link is between urbanization and female college education. Figure 1 presents the results of regressions of the percentage of adult females (logged) with college degrees on the percentage urban (logged) for 43 nationality groups in 1959 and 1970.¹¹ Two other groups were excluded—the Jews, the most modernized, urbanized and educated nationality group, and the Muslim Chechens, who score lowest on all three dimensions; the two outliers are plotted in Figure 1, but not included in the calculation of the regressions. In 1959, 43% of the variance in female higher education (adjusted r square) is explained by urbanization level ($t = 5.77$; $p < .001$). Also plotted in Figure 1 are the regression lines computed separately for the 23 non-Muslim and 20 Muslim groups. The stronger relationship between urbanization and female education among the non-Muslim groups in 1959 is not unexpected—traditional Muslim values regarding female roles dampened, but did not halt, the effects of modernization. Adding a dummy Muslim variable to the 1959 regression ($t = -2.7$; $p < .01$) increases the adjusted r square by eight percentage points to 51%.

The analogous regression for 1970 data explains a smaller, but still statistically significant, 21% of the variance ($t = 3.51$; $p < .005$) for the 43 nationality groups. For the non-Muslim groups, a relatively strong linear relationship remained in 1970 (beta, .582, down from .750 in 1959). This weakening of the relationship among the more modernized non-Muslim groups was the result of the muting of urban-rural differences that occurs as the value of female higher education becomes diffused throughout society. For the Muslim groups, the linear relationship between urbanization and female higher education disappeared by 1970 (beta, .077, down from .473 in 1959). For the 1970 data, the dummy Muslim variable was statistically insignificant.

The absence of a relationship between urbanization and female higher education for the Muslim groups in 1970 was probably the result of government intervention. Urbanization rates for some of these groups increased only marginally in the intercensal period, but access to higher education expanded significantly, partly because of government education policy, such as quotas for minority students, and partly because of the youthful age structures of the more traditional groups. Many of the dramatic gains in Muslim

higher education by 1970 can be traced to this increased educational opportunity (Jones & Grupp, 1984). The 1960s were also a period in which government-sponsored programs to increase female access to higher education were accelerated. That Muslim *women*, as well as Muslim men, were willing to take advantage of this enhanced educational opportunity during the intercensal period is evidence of considerable value change among those groups. The net effect of these changes was to wash out the relationship between urbanization and female higher education.

Other measures of female roles show much weaker links with urbanization. The relationship between urbanization and early marriage rates, for instance, is in the predicted direction, but is relatively weak: $r = -.31$ in 1959 and $-.28$ in 1970 for the 38 groups for which we have complete data. The statistical relationship between urbanization and early marriage practices was even weaker when we lagged the data; the correlation between 1959 urbanization and 1970 early marriage is only $-.21$.

Analysis of the statistical relationship between modernization level (as measured by percentage urban) and traditionality (as measured by female education and early marriage rates) provides findings that, although not overwhelmingly strong, are generally consistent with the individual modernity hypothesis. It may be argued, however, that the hypothesis should be tested dynamically. Accordingly, we computed change scores¹² for level of urbanization and for female participation rates. Correlation of change in percentage urban with change in the various measures of female participation provides results that are either weak or contrary to the hypothesis. For example, the correlation between change in urbanization and change in early marriage rates is .55, that is, in the direction opposite to that expected: those ethnic groups with the smallest increases in percentage urban showed the greatest decline in early marriage rates. The urbanization level of the Chechen, for example, actually declined between 1959 and 1970, yet their early marriage rate declined by 51%, from 404 per thousand women in 1959 to 199 in 1970.

Results more consistent with the individual

¹¹The variables were logged to help overcome the skewness of the distribution and reduce the impact of extreme values.

¹²Psychologists have led the way in condemning the use of change scores in analyzing treatment effects, primarily because they compound measurement error. Here, however, the individuals are different (16- to 19-year-old females at two time periods) and the measurement errors, although unknown, are likely to be consistent across the two censuses. Early marriage rates for Soviet nationalities from the 1979 Soviet census have not been released.

modernity hypothesis were obtained when we controlled for modernization level through the use of two-stage regression. First, female change scores were regressed on the analogous change in male rates for the same indicator (except for early marriage, discussed further below), and then the residuals were regressed on change in urbanization. In other words, the modernization level of the group as indicated by the appropriate male rate was removed from the analysis to determine the independent contribution of urbanization. The rationale for this procedure is that groups where males have much higher levels of achieved education than their female counterparts are qualitatively different in terms of modernization from groups where both sexes have nearly equivalent (either high or low) educational achievement.

Change in urbanization explains 14% of the variance in the residuals when change in female specialized secondary education is the dependent variable ($t = 2.74$; $p < .01$). Analogous two-stage regressions for change in female college and specialist participation produced weaker results; change in urbanization explains 5% and 8% of the variance respectively. As before, early marriage does not conform to the individual modernization hypothesis. No matter what male rate is used to partial out modernization, the residuals are always related to change in urbanization positively, not negatively as required by the hypothesis, which indicates that changes in family values and female roles can occur very rapidly once they begin to be diffused throughout a society.

In sum, statistical tests of the modernity-traditionality link provide only limited support for the hypothesis that levels of traditionality are statistically related to modernization levels. These mixed results are probably owed at least in part to the weaknesses of the available measures. Urbanization in particular is likely to decline in strength as a predictor of modern values. Increased mass communications are likely to diffuse urban values to rural regions even without a commensurate increase in the percentage who are urban.

Conclusion

The evidence we examined points to areas of both change and continuity in family life styles. Patriarchal attitudes and behavior patterns have persisted most strongly in four areas: family size, family planning use, acceptance of patriarchal authority patterns, and attitudes regarding the role of marriage. Survey results on certain issues, such as those relating to parental authority, suggest that even those members of the late-modernizing groups most exposed to modern institutions (i.e., young urban residents) hold traditional values virtually identical to those of their

older and less-modernized counterparts. The persistence of these behavioral and attitudinal patterns indicate that there are, as Carrere d'Encausse has suggested, important aspects of continuity in family values.

These areas of continuing ethnic difference, however, should not obscure the extensive shifts in life styles, attitudes, and basic values regarding home and family that have occurred and continue to occur. Over the last two decades, there has been dramatic change in gender roles in the late-modernizing regions of the USSR. Data on female educational attainments provide persuasive evidence that the Soviet strategy of promoting fuller female access to education has worked very well. Data on labor force participation show increasing levels of female entry into the modernized labor force, even in Muslim southern-tier areas where such access had been quite limited. Public attitudes toward the working mother in the Muslim areas do not seem very different from those in the European regions to the north and west. Many women from even the most traditional areas exhibit a surprisingly high degree of acceptance of institutional child care.

The increasingly active "public" role of women from traditional minorities has been accompanied by changes in their domestic role as well. Data on marital patterns indicate that these trends have been paralleled by dramatic declines in the percentage of early marriages among groups for whom early marriage had long been the predominant way of life. The traditional support for very large families in the southern tier has also begun to diminish. Although use of family planning is still relatively limited in the traditional areas, the younger, better-educated, and employed women are beginning to display patterns of family planning acceptance similar to those of their European counterparts. Some of the more dramatic ethnic differences, even in those areas of family life that display the most resistance to change, virtually disappear when key socioeconomic variables are controlled. The identical pattern of abortion use by young Kazakh and Russian women in the Alma Ata study and the similarity of family size desires among young Azeri and Russian women in Baku are two examples. Distribution of household tasks seems to be determined more by social status variables than by nationality or region of residence. These findings point to aspects of life style and values that are generally compatible with the "Soviet" family system in central USSR. These trends are all the more significant precisely because many of the changes documented above are in areas thought to be least susceptible to change.

These findings are in close accord with the conclusions reached by western scholars studying the

role of modern institutions on the attitudes, behavior, and personality traits of individuals. Those studies have consistently found that exposure to modern institutions—school, factory, and city—increased individual modernity. The most powerful predictor of modern values across a variety of national settings was education (Holsinger & Thiesen, 1977). Some of the modernity studies included questions similar to the Soviet ethnographic survey questions. For example, Kahl's study in Mexico included a series of questions on family modernism, tapping acceptance of divorce as a way of resolving an unhappy marriage, beliefs regarding parental authority, and attitudes toward working wives. Composite scores on this measure were positively related to socioeconomic status (Kahl, 1968, pp. 33, 47-48). Other studies by western scholars that focus more narrowly on gender roles show a similar nexus between exposure to modern institutions, particularly school, and support for egalitarian sex roles (see, for example, Scanzoni, 1975, pp. 19-62). The Soviet data, which demonstrate a direct link between exposure to modern institutions and modern family and sex role values and behavior, are quite consistent with these findings.

The USSR's late-modernizing ethnic groups, then, do not appear to be an exception to the generalization that contact with modern socioeconomic settings tends to produce individuals with less traditional, more modern values. Although the cultural heritage of Islam has made some of the USSR's Muslim regions more resistant to the erosion of traditional family values associated with modernization, Islamic heritage has not rendered those groups immune to the effect of socioeconomic development. Exposure to mass communications, education, urban life, modern occupations, and the modern bureaucracy in the USSR has produced dramatic changes in family values and gender roles—changes that parallel those found in nonsocialist settings.

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War Making and State Making: Governmental Expenditures, Tax Revenues, and Global Wars

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Addressing the disputed relationship between war and the expansion of governmental expenditures and revenues, Box-Tiao intervention models are applied to a number of British (1700-1980), United States (1792-1980), French (1815-1979), and Japanese (1878-1980) spending and taxation series. Distinguishing between global and interstate wars, the more intensive and extensive bouts of warfare (global wars) tend to bring about abrupt, permanent impacts in contrast to the temporary changes associated with most interstate wars. The observed displacements are reflected in both war-related and nonwar-related types of expenditure and are also observed before 1900. Although our findings are not universally applicable and are subject to various other qualifications, they may be interpreted, in general, as reinforcing the need for an appreciation of the persistent centrality of war, especially global war, in the discontinuous growth and expansion of the modern state.

We live in an era in which the appropriate scope of governmental activities is a subject of intensive political debate. Conservatives complain that governmental expansion and the encroachment of the public sector on the private sector have gone too far and must be curbed. Liberals, on the other hand, complain that the process has not gone far enough to satisfy the demands of public welfare. Whatever one's ideological persuasion, it is clear that governments have expanded the scope of their activities and functions. But even if this last statement enjoys consensus agreement, it is by no means evident that we have a clear understanding of how and why states have expanded their scale of operation. Toward the end of the nineteenth century, for instance, advocates of Wagner's Law contended that governmental activities would expand in roughly linear response to the development of growth-related social problems (see Bird, 1971; Wagner & Weber, 1977). However, Peacock

and Wiseman (1961) have argued that governmental expansion has outpaced economic growth in the twentieth century and that increasing the scope of governmental functions requires the advent of national crisis so that the popular reluctance to accept tax increases can be overwhelmed by the need to respond to the crisis. We agree that long-run governmental growth tends to be discontinuous but contend that, for certain states at least, the principal agent of change is a singular type of crisis—global war—and that the fundamental war making-state building process is much older than developments relatively unique to the twentieth century. Throughout the remainder of this article, we will attempt to provide empirical support for this longer-term view by examining the long-term pattern of changes in governmental expenditures and revenues in relation to the onset of different types of war. In the process, we hope to contribute to the overcoming of our collective underappreciation of the general significance of war (see Stein & Russett, 1980) to an understanding of domestic changes and developments.

The Expenditure Displacement Hypothesis

Expressed most simply, Peacock's and Wiseman's (1961) displacement hypothesis states that governmental expenditures will increase during periods of national crisis and that although expenditures may decline in the post-crisis period, they will remain higher than pre-crisis expenditure levels. This permanent displacement effect on ex-

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penditures is attributed primarily to crisis-induced shifts in taxpayers' perceptions of what tax-burden levels are viewed as tolerable. Depending on the governmental decision makers' attitudes toward the roles of public expenditures and their willingness to take advantage of the opportunity, major crises, especially wars, work to weaken or override normal (non-crisis) resistance to increased taxes. Tax revenues therefore are easier to raise. Wars, in addition, offer excellent opportunities to revise or overhaul the prevailing tax system and to create new sources of revenue. As a result, the widened tax base facilitates greater spending after the war or crisis than before.

Peacock and Wiseman (1961) present a detailed examination of Great Britain's principally twentieth-century expenditure patterns as a test of their hypothesis. Because their central hypothesis is attitudinally based, there are of course very real constraints on the extent to which expenditure data can be used to address their thesis. Through the visual examination of a large number of spending series, however, they are able to demonstrate that World Wars I and II brought about major shifts in governmental spending that cannot be accounted for by either population growth, inflation, or economic growth. Further support for their interpretation is produced by the finding that the expenditure increases are not due solely to increases in war-related and military costs. If this had been the case, the expenditure displacement process would represent merely the periodic augmentation of military spending and the gradual accumulation of war debts and pensions. But because non-military and war-related spending increases as well, Peacock and Wiseman view this indicator as evidence of genuine governmental role expansion made possible by the involvement of a society in crisis or war.

Beyond the difficulty of tapping taxpayer attitudes with spending patterns, the Peacock-Wiseman evidence is also restricted by the small sample of one state and their exclusive reliance on the visual examination of longitudinal plots of numerous expenditure series. Yet their hypothesis has stimulated a respectably extensive literature that sometimes uses more sophisticated empirical techniques or expands the number of countries surveyed or both. Although the most popular examples remain the British and American ones, the following states have also received some expenditure displacement effect attention: Brazil, Canada, Costa Rica, Dominican Republic, France, Germany, Guyana, Haiti, Honduras, Iceland, India, Japan, Panama, Sweden, and Taiwan. Looked upon in the aggregate, the empirical outcomes represent some decidedly mixed support for the original Peacock-Wiseman assertion. Four conclusions seem to have emerged.

Crises associated with world wars:

1. bring about permanent changes in state expenditure levels (Emi, 1963; Gupta, 1967).
2. sometimes bring about permanent changes in state expenditure levels (Andre & Delorme, 1978; Blondal, 1969; Kaufman, 1983; Tussing & Henning, 1974).
3. create, at best, only temporary, not permanent, shifts in state expenditure levels. The temporary impact may be attributed to the wartime higher priority given to military over civilian spending (Borcherding, 1977a, b; Musgrave, 1969; Pryor, 1968; Reddy, 1970) or postwar increases in civilian spending presumably related to addressing war damages and reconstruction (Bonin, Finch, & Waters, 1967; Rosenfeld, 1973).
4. may effect expenditure growth but so do a number of other influences (Bennett & Johnson, 1980; Meltzer & Richard, 1978) including non-world wars (Emi, 1963; Leff, 1982; Nagarjan, 1979), depression (Blondal, 1969; Gupta, 1967), and changes in governmental philosophies during peacetime (Goffman & Mahar, 1971; Mahar & Rezende, 1975; Reddy, 1970).

Clearly, the four conclusions do not add up to anything resembling a consensus. Nor can the extent of disagreement be explained away by the variety of examined countries. For example, all four conclusions have been arrived at by different analysts of the same or similar British and American expenditure data. To be sure, some of the findings should be viewed with more skepticism than some of the others, owing to various research design problems.¹ Nevertheless, it is apparent that several questions deserve further investigation. Does the war-induced expenditure displacement phenomenon apply to Great Britain? To what extent does it apply outside of Great Britain? Alternatively, Peacock and Wiseman's explanation stresses "national crises," but their examination is restricted primarily to the effects of World Wars I and II. Yet several authors contend that some interstate wars (e.g., the 1904-1905 Russo-Japanese War for Japan, the 1962 Indo-Chinese War for India) had greater impacts on spending patterns than some of the twentieth century's world wars. Other authors find significant impacts associated with World War I but not World War II and vice versa. Do all wars influence governmental spending in idiosyncratic ways and to

¹For Peacock's and Wiseman's reactions to some of this literature's design problems, see Wiseman and Diamond (1975) and Peacock and Wiseman (1979).

varying extents? Or, are some kinds of warfare, such as "world wars," more likely than others to bring about permanent expenditure displacement effects?

The War Making- State Making Interpretation

The variety of specification problems raised by the empirical studies that have followed Peacock and Wiseman do not exhaust the questions that need to be raised about the asserted displacement process. The Peacock-Wiseman emphasis on the crisis-induced stretching of tax burden tolerances is not implausible, and we see no need to reject it completely. But it is very much a twentieth-century explanation in flavor, just as it is based primarily on the examination of twentieth-century data. If the displacement phenomenon, however, is much older than the twentieth century, it is conceivable that a more general or broader and less time-bound explanation is needed.²

Support for our contention that the displacement process predates the twentieth century is not particularly difficult to find. Tilly's (1975, 1979) expression that "war made the state and the state made war" summarizes a number of overlapping views on the long-term contribution of war to several hundred years of European state-building (see Ames & Rapp, 1977; Anderson, 1974; Ardant, 1975; Bean, 1973; Braun, 1975; Eichenberg, 1983; Gilpin, 1981; Gourevitch, 1978; Hintze, 1975; McNeill, 1982; Modelski, 1972; North & Thomas, 1973). A primary, if not the primary, imperative of state building has been the suppression of internal rivals and the defeat of external enemies. To remain in power at home and competitive abroad, military preparations have been essential and increasingly costly as military technology has improved. To pay for these seemingly ever-rising military costs, rulers have felt

compelled to extract more and more resources from their populations. To collect and manage the increasing scale of these resource extractions, rulers have been forced or encouraged to create and expand their state's bureaucratic-administration apparatus as well.

This version of the death-and-taxes cycle has not been unaffected by change. Economic growth in general, and industrial development in particular, have made tax collections easier and more remunerative, but these same developments have also contributed to the likelihood of more intensive and costlier wars. The idea of national debt has been invented to overcome shortfalls in tax revenues, especially in time of war, but the institutionalization of state debts also implies long-term debt accumulation and predictable debt interest payment schedules (increasing organizational fixed costs). Much more recently, and hardly in isolation from other ongoing developments, the scope of governmental intervention in society and economy has also expanded. Yet although these developments may have modified the processes of state building and expansion, it is doubtful that they have been so radically transformed that we need to look for entirely new patterns. Interstate competition, wars, and the increasing costs of military preparations have not disappeared, nor has the need to pay for these activities. It seems reasonable to suggest, therefore, that the historical connection between war making and state making and expansion is a persistent one.

The differences between this historical perspective on the growth of the state and the Peacock-Wiseman interpretation can be viewed as a matter of degree and emphasis. Peacock and Wiseman stress an image of contemporary state decision makers, with a perceived need to expand the scope of governmental activities, taking advantage of periods of crisis and war to expand their revenue base. The longer view does not preclude this inherently opportunistic possibility, but it chooses to emphasize instead a broader conceptualization of states owing their very organizational existence and *raison d'être* to the need to survive, and to prevail, during the periods of warfare. Confronted with very real threats, decision makers are compelled to mobilize human and material resources at their disposal in the interest of state security and military victory. At war's end, successful and unsuccessful states demobilize, but there is no reason to assume or to expect that the demobilization will be complete, except perhaps in cases of absolute defeat. Nor is it likely that state decision makers will insist on the strict restoration of the state's prewar role in national society. To the contrary, the realities of wartime change will make turning back the political clock extremely difficult and unlikely:

²We are reluctant to argue that the twentieth century is an era generally characterized by more governmental sensitivity to taxpayer preference schedules. It is true that taxpayer revolts, Proposition 13 notwithstanding, were once more common than they are currently. But whether this generalization suggests greater governmental sensitivity or greater taxpayer passivity, resignation, acquiescence, or all of the above, remains debatable. Nevertheless, we are suggesting that contemporary analysts are much more conditioned to emphasize popular constraints on governmental expansion than they were likely to before the twentieth century. In a similar vein, Wagner's Law's complete avoidance of the subject of war betrays a late-nineteenth-century flavor. As Bird (1971, p. 4) notes, Wagner's late-nineteenth-century generation did not expect wars to be very common in the future.

New sources of revenue will have been created and old sources will have been expanded, embellished, and perhaps made more efficient.

New social problems (e.g., price controls, provision for the homeless and refugees, concerns over inadequate diets and education for the draft-eligible portions of the population, the need to suppress racial tensions at home and in combat units, reconstruction) will emerge and old problems will receive greater political attention than before.

New domestic political coalitions may emerge or have an improved opportunity to emerge as their constituencies' contribution to the war effort becomes more highly valued. The general resistance to social and political change, in any event, is more likely to be either weakened or even overwhelmed by the need to respond to the demands of the war effort.

New bureaucratic organizations will emerge to deal with novel war-related problems. Old governmental agencies will be expanded not only to deal with the increase in management problems, but also because the opportunity to invoke security-related justifications for bureaucratic expansion will be expanded as well.³

The significance of some of these war-induced changes may be eliminated or subsequently diminished in the postwar era, but it is most unlikely that all of the war-induced changes in expenditures, revenues, number of governmental agencies and personnel, salience of social issues and problems, and the nature and identity of political coalitions will disappear or even fade away. Wars, especially major wars, thus induce direct domestic changes in the short run and also serve as catalysts and facilitators for direct and indirect domestic changes in the long run. A respectable proportion of the war-induced growth of the state is thus a not-altogether-planned by-product of making war.

Yet there is no reason to believe that all wars will have the same impact on state-building processes. Some wars may bring about very little in the way of change, whereas others may be associated with profound and wide-ranging impacts. Modelski's (1978, 1981, 1982) long cycle of world leadership perspective is crucial in this regard because it supplies a theoretical rationale for anticipating that one category of warfare, the global war, will be more likely than others to be associated with significant societal impacts. Global wars are iden-

tified as the decisive contests fought over the issue of succession to world leadership and, therefore, demarcate the transition from one long cycle of leadership to the next. During and immediately after each global war, one state emerges in a leading capability position. The eventual erosion of this preponderance leads to leadership challenges mounted by rivals and another succession struggle to be resolved in global war.

Although global wars are not defined in terms of scope, participation, or costs, they tend to be unusually extensive in terms of geography encompassed and the number of major actors involved. Global wars tend also to be among the most costly wars in terms of both lives lost and resources expended. Consequently, it seems reasonable to hypothesize that the most important wars, the deadly turning points in modern political history for the global political system, should have the most important and significant impacts on the state-building processes of the participants. In contrast, non-global interstate wars generally tend to be much less extensive and intensive affairs and, therefore, are relatively less likely to exert significant impacts.

By focusing on the two categories of warfare as separate types of impact-producing interventions, our version of the war making-state making approach differs from the Peacock and Wiseman interpretation in several respects. First, Peacock and Wiseman restrict the temporal scope of their displacement argument to late-nineteenth- and early-twentieth-century British attitudinal and philosophical shifts toward greater state involvement in social policy arenas. Alternatively expressed, the two most recent world wars facilitated the expansion of the British state into new areas of governmental intervention. However, the distinction between global and interstate war applies as far back as the initially crude emergence of the global political system in 1494, thereby encompassing nearly 500 years of state building and expansion.

Peacock and Wiseman (1961, p. 38) had noted that their emphasis on changing governmental attitudes would have been less tenable if crisis-related expenditure displacements had occurred before 1900. They then proceeded to demonstrate the absence of a permanent displacement effect on British spending after the Napoleonic Wars. If they are correct, the validity of our own historical argument is open to some question unless the British pattern is exceptional. To ascertain what is a deviation from the norm will of course require an expansion of the number of states examined. However, we also believe that Peacock and Wiseman misinterpreted the early nineteenth-century British data. In order to explore this claim and to assess better the longer view on state building, the

³On this point, see Porter's (1980) argument that American governmental agencies learned to use national defense rationalizations to justify expanded budgets to legislators even when they were not directly related to the ongoing World War II effort.

present examination will need to develop data series that extend back in time as far as is practical. The development of reasonably lengthy time series will also facilitate the employment of impact assessment models that measure the displacement impact of war in as systematic a manner as is possible.

Although the global war making perspective expands the temporal scope of the displacement explanation, the focus on the intervention agents is being narrowed considerably. Despite the fact that their own examination was restricted to the twentieth century's two world wars, Peacock and Wiseman equated their conceptualization of the opportunities for displacement with "social disturbances" or "national crises." These broad categories imply the need to investigate the possible impacts of a relatively large list of candidate events. Non-war crises such as severe economic depressions may indeed lead to the expansion of the state but we doubt that any single type of crisis can compare over the long run to the ostensibly consistent, historical impact of global war.

Implicit in our approach is the assumption that seeking information on war impacts for all states is not likely to prove to be a very efficacious strategy. Instead, given the disparities in types of states and historical experiences, a more selective and categorically homogenous state sample is desirable. Peacock's and Wiseman's focus on a single state, of course, could be said to represent the extreme interpretation of this preference. But there is a middle course between the extremes of single state case studies and large sample aggregations. The logic of our hypothesis development suggests an actor focus on states that have participated in both global and interstate wars. Yet all war participants do not participate fully or in the same way. For example, we might expect to find some differences in comparisons between strong and weak states or between persistent combatants and states defeated and occupied early in a war. On the other hand, we need to avoid eliminating all of the variance. Consequently, we will confine our sample construction to the premise that the hypothesized displacement effects of global war should be most noticeable in the expenditures of those states that are most acutely involved in the succession struggles as contenders for systemic power. Ideally, then, our examination would focus on the nine states that have been identified as the principal contenders (at various points in time) in the long cycle process.⁴

⁴From the long cycle perspective, global wars have taken place in 1494-1516, 1580-1608, 1688-1713, 1792-1815, and 1914-1918/1939-1945. Global powers must satisfy minimum global reach capability (naval power) criteria. The following states qualify for consideration:

Unfortunately, continuous expenditure records from the sixteenth century on are not available for any state. We also need to be careful to control for the possible complications of economic growth. Moreover, we wish to determine whether tax revenues follow the same historical course as do expenditures. In spite of the explanatory emphasis on tax revenues in Peacock's and Wiseman's tax burden argument, these data are only rarely inspected vis-à-vis the displacement hypothesis. Finally, we too wish to check whether any war-induced displacement effect that is uncovered can be explained solely in terms of increased military and war-related spending. A greater emphasis on the historical significance of warfare need not preclude the expansion of non-war-related activities but the extent to which these governmental activities have expanded is probably subject to some degree of evolution.

The need for hard-to-come-by, continuous time series data on central government expenditures, tax revenues, military expenditures, and some measures of national wealth—all of which must encompass one or more global wars—forces some compromises on analysts.⁵ Longitudinal data that meet the requirements outlined immediately above are readily accessible for only a very few states. Accordingly, we will concentrate our analyses on four global powers: Great Britain (1700-1980), the United States (1792-1980), France (1815-1979), and Japan (1878-1980), and the sets of global and interstate wars listed in Table 1.⁶

Portugal (1494-1580), Spain (1494-1815), England/Great Britain (1484-1945), France (1494-1945), Netherlands (1579-1815), Russia/Soviet Union (1714-present), United States (1816-present), Germany (1871-1945), and Japan (1875-1945). Other assessments of the impacts of global wars on wholesale prices, economic growth, and public debts may be found in Thompson and Zuk (1982) and Rasler and Thompson (1983, in press).

⁵By dividing expenditures and tax revenues by GNP, we are not overlooking the probability that government spending and the economy as a whole are likely to be subject to different rates of inflation. Specialized deflationary indexes, however are not normally available for long time series.

⁶Gathering data on governmental expenditures and revenues is subject to a host of pitfalls—some of which, no doubt, we have failed to avoid. Whenever possible, we have attempted to use closed accounts as reported by governmental agencies or secondary and tertiary sources based on governmental abstracts. We have occasionally been forced to deviate from a reliance on official accounts in cases of overtly understated military spending figures. We have also attempted to exclude from the expenditure and revenue series spending for, and receipts from, public enterprises in order to enhance their longitudinal comparability. The data sources utilized are for France, Sudre (1883), de Kaufmann

Table 1. Interstate and Global Wars

Interstate Wars		Global Wars	
France (1815-1979)			
Franco-Spanish	1823	World War I	1914-1918
Roman Republic	1849	World War II	1939-1945
Crimean	1854-1856		
Italian unification	1859		
Franco-Mexican	1862-1867		
Franco-Prussian	1870-1871		
Sino-French	1884-1885		
Franco-Thai	1940-1941		
Korean	1951-1953		
Sinai	1956		
Great Britain (1700-1980)			
Anglo-Swedish	1715-1719	Spanish succession	1701-1713
Quadruple alliance	1718-1720	French Revolutionary	1793-1802
Anglo-Spanish	1726-1729	Napoleonic	1803-1815
Austrian succession	1739-1748	World War I	1914-1918
Seven years	1756-1763	World War II	1939-1945
American independence	1778-1783		
Anglo-American	1812-1814		
Crimean	1854-1856		
Anglo-Persian	1856-1857		
Korean	1950-1953		
Sinai	1956		
Japan (1878-1980)			
Sino-Japanese	1894-1895	World War I	1914-1918
Russo-Japanese	1904-1905	World War II	1941-1945
Manchurian	1931-1933		
Sino-Japanese	1937-1941		
United States (1792-1980)			
Anglo-American	1812-1814	World War I	1917-1918
Mexican-American	1846-1848	World War II	1941-1945
Spanish-American	1898		
Korean	1950-1953		
Vietnam	1965-1973		

(1884), Marion (1914), Jeze (1927), Mallez (1927), Ministere des Finances (1946), Marczewski (1961), Mitchell (1975, 1981), Ministere de l'Economie (various years), OECD (1980, 1981); for Great Britain, Deane (1955, 1968), Deane and Cole (1962), Mitchell (1962, 1975, 1981), Mitchell and Jones (1971), Feinstein (1972), Central Statistical Office (various years), Cole (1981), OECD (1980, 1981); for Japan, Emi (1963, 1979) Ohkawa and Rosovsky (1973), Mitchell (1982), Japan Statistical Yearbook (various volumes); and for the United States, U.S. Department of Commerce (1975), Berry (1978), U.S. Office of the President (1982, 1983). The identification of the wars has been guided by information found in Wright (1965), Singer and Small (1972), Dupuy and Dupuy (1977), and Small and Singer (1982).

Time Series Problems and the Utility of Box-Tiao Impact Assessment Models

Previous studies concerned with estimating the impact of war on various processes such as economic growth and the increase in national and social welfare expenditures can be divided into two groups. The first group basically relies on visual evaluations of the differences between pre- and post-intervention series in an attempt to assess war impact (Emi, 1963; Mahar & Rezende, 1975; Peacock & Wiseman, 1961; Reddy, 1970; Rosenfeld, 1973). As a preliminary step or in instances where an impact is acutely obvious, the

eyeball technique may suffice and certainly is always helpful. As the number of interventions and national series to be examined increases, however, visual plots quickly become cumbersome analytical and communication devices, especially if space is limited and if not all of the impacts are readily discernible. In general, but particularly in these circumstances, more objective tests are definitely desirable.

The second group, for the most part, uses regression techniques to estimate the changes in the level of prewar and postwar intervention series (Andre & Delorme, 1978; Barbera, 1973; Bonin et al., 1967; Gupta, 1967; Kaufman, 1983; Nagaran, 1979; Organski & Kugler, 1980; Pryor, 1968; Stohl, 1976; Tussing & Henning, 1974; Wheeler, 1980). The ultimate validity of many of these regression-based findings, we feel, is handicapped by certain statistical problems related to an overemphasis on data fitting operations and an underemphasis on controlling for autocorrelation and trend.

Most of the earlier expenditure displacement analyses exclude war period data from their analyses in an effort to assess long-term effects without the results being unduly influenced by the war outliers. In the process, analysts have selected various time periods ranging anywhere from 3 to 70 years or more for the ad hoc prewar and postwar data intervals. In order to fit a least squares model to the data, prewar and postwar contortions in the original series that might reduce the degree of fit tend to be ignored.

In the presence of autocorrelation, a problem commonly encountered in time series and frequently ignored, the data will track for a period away from the equilibrium point. This tracking phenomenon results in unbiased but inefficient estimates of the parameters of a model. In linear regression, OLS estimates of β will be unbiased while the variances of the estimators will be understated. In addition, the error variance of the regression model will be minimized and, if autocorrelation is ignored, the model will appear to provide a much better fit to the empirical data than is actually the case. Inferences based on sample t and F -statistics will be misleading owing to the deflation of the true variance of β and the regression model (Hibbs, 1974).

Trend poses still another obstacle in time series analysis. It produces a systematic change in the level of the series in such a way that it is difficult to interpret the short- and long-term effects of an intervention. The conventional method of removing trend has been to subtract a least squares trend line from the data. However, it can be argued that the OLS parameters cannot be estimated with much accuracy owing to their sensitivity to outliers as well as their somewhat static dependence

on the positions of the first and last observations of the observed series.⁷

Perhaps an even more central issue with OLS detrending methods is the presumption by researchers that a time series is influenced by deterministic trend when it may be characterized by stochastic drift. A time series that is modeled as a fixed function of time (deterministically) when in fact the values vary in a probabilistic manner (stochastically) will result in errors in assessing the magnitude of an intervention's impact (McCleary & Hay, 1980). In addition, because a time series can drift upward or downward for long periods of time owing only to random forces, it is not always obvious whether a progressive change in the level of a series is due to deterministic trend or stochastic drift.

To address the problems of fitting, autocorrelation, and trend, we use impact assessment models developed by Box and Tiao (1975). These models avoid the fitting problem because they use all of the data in a time series to estimate parameters that are likely to have generated the series. Consequently, the analyst has the opportunity to avoid excluding selected data points arbitrarily. Another advantage of Box-Tiao models is that they provide a parsimonious way to control for the effects of autocorrelation. An ARIMA scheme is used to model a noise component which includes the random and deterministic effects of drift, trend, and autocorrelation. A linear filter(s) is then applied to transform the observed time series into white noise before estimating the actual effect of the

⁷To subtract a least squares trend line from a series, a researcher regresses that Y_t series on time ($X_t = 1, 2, 3, \dots, N$), estimates the least squares slope of the model, and then calculates the "detrended" Y_t series. In this case, the Y series is a function of the $Y_t = b_0 + b_1(\text{time}) + e_t$ equation where b_0 is the intercept and represents the mean level of the Y_t series and $b_1(\text{time})$ represents the slope of the trend line or the expected change in the level of Y_t from one observation to the next. However, b_0 and b_1 are derived by minimizing the sums of squares function

$$\Sigma(Y_t - \hat{Y}_t)^2 = \Sigma[b_0 - \hat{b}_0 + (b_1 - \hat{b}_1)t]^2.$$

As the independent variable t (time) increases monotonically, the first and last observations of Y_t usually make the greatest contribution to the sums of squares function. Consequently, the OLS estimates of b_0 and b_1 are estimated so that the OLS trend line generally passes through Y_t and Y_n regardless of how well the middle observations ($Y_{t+1} \dots Y_{n-1}$) fit. This procedure represents a rather static approach to dynamic phenomena (McCleary & Hay, 1980). Moreover, studies that do not attempt to remove trend may be subject to many of the same problems in comparing pre-intervention and post-intervention slopes.

intervention.⁸ Moreover, the Box-Tiao models neutralize the problems associated with trend and drift, without a priori distinctions, through the use of difference equation models that can be used to differentiate between trend and drift and to provide dynamic estimates of trend.⁹

Types of Intervention Effects. The intervention component of the Box-Tiao models is represented by two transfer function parameters, ω_0 and δ , which estimate the initial impact of an event (I_t) on the observed Y_t series and the rate of growth or decay in the level of the time series after the impact. While the omega parameter (ω_0) reflects the initial impact of the intervention, an estimate of the difference between the pre-intervention and postintervention levels of Y_t , the delta parameter (δ) captures the dynamic response. Delta is a rate parameter in the sense that it specifies how quickly the postintervention series level continues to change (increasing or decreasing) by smaller and smaller increments (or decrements). The size of δ , which is constrained between -1 and $+1$, indicates how quickly (or slowly) the postintervention series reaches equilibrium. When δ approximates 1, the postintervention series returns to equilibrium very slowly. Conversely, when δ approximates 0, the postintervention series returns to equilibrium very quickly.

ω_0 and δ provide the tools to assess whether an intervention has an initial abrupt or gradual effect on Y_t and whether it is associated with a temporary or permanent change in the level of Y_t . The behavior of a time series generated from the impact of an event will take the form of two basic patterns, a step or pulse model of intervention.

⁸A white noise series describes a set of independent and random observations that are normally distributed about a zero mean and constant variance.

⁹If a time series is the realization of an integrated process (i.e., a random walk due to drift), it can be modeled by simply differencing that Y_t series. If the differenced series has a nonzero mean, it is characterized by linear trend which is represented in the following equation as

$$Y_t = Y_{t-1} + \theta_0 + a_t,$$

where θ_0 is estimated as the mean of the differenced series. To distinguish between drift and trend, θ_0 is subjected to a t -test. If θ_0 is not statistically different from zero, the series is considered to be drifting rather than trending. Whereas OLS detrending methods estimate linear trend using a time counter as the independent variable, the difference equation model uses the values of Y_{t-1} as the independent variable. Since all of the Y_{t-1} values influence the estimate of θ_0 (the constant of the difference equation), a dynamic estimate of linear trend is produced (see Box & Jenkins, 1976; McCleary & Hay, 1980).

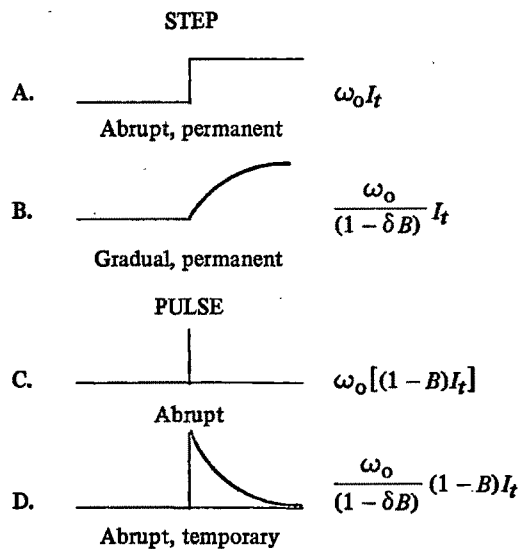
These models and their appropriate Box-Tiao intervention components are displayed in Figure 1.

Step model A represents an abrupt, permanent impact pattern where the event (I_t) is associated with a significant shift in the level of the Y_t time series from pre-intervention to postintervention. Model B characterizes a gradual, permanent impact. The onset of an event is accompanied by a significant initial change in the level of the post-intervention series. However, the full impact of the event is not realized immediately. Instead, it has a gradual effect over a number of subsequent observations (or time periods) until the post-intervention series reaches a new equilibrium.

Alternatively, Model C illustrates a situation in which the event under study has a significant initial impact on a Y_t series at one period—the onset of the event—and has no residual effects on the rest of the observations. Finally, Model D represents the case when an event is associated with an abrupt but temporary change in the level of the postintervention series.

Summarily, the strategy of this approach is to identify the noise model (ARIMA structure) of the Y_t series with the pre-intervention data. Then, an intervention component from Figure 1 is

Figure 1. Models of Intervention and Impact



ω_0 = An estimate of the difference between the pre- and post-intervention process levels.

I_t = 1 during a war and 0 during nonwar years.

δ = Rate of decay or increase.

B = A backshift operator which may be interpreted as $B(Y_t) = Y_{t-1}$.

selected based on theoretical notions about the type of impact that I_t will have on the observed time series.¹⁰ These parameters, in addition to the ARIMA parameter(s), are estimated in a full model through iterative, nonlinear estimation procedures. In most cases, parameters lacking statistical significance are removed from the equation, and the full model is reestimated. An important exception to this rule is provided by cases involving deltas with values greater than one or where the confidence intervals associated with the deltas are not constrained within the ± 1 range. This warning of model misspecification reflects what is referred to as "explosive growth" (also known as a ramp effect) after the impact of the intervention. Intuitively, such an outcome is more likely to be interpreted as providing support for a step function, permanent change than it is for a delta-less, temporary, pulse function model.¹¹ In any event, an intervention model's ultimate goodness-of-fit is judged by the degree of correspondence between the observed and predicted output series values, the residual mean squared error, the statistical significance of the model parameters, and the white noise or random character of the residuals.

Data Analysis

Our principal hypothesis states that for certain states, global wars are more likely to displace governmental expenditures and revenues than are interstate wars. In the Box-Tiao context, we would then expect to find significant, permanent impacts associated with the intervention of global wars and either temporary or nonsignificant impacts registered by interstate wars. This expected outcome is very close to what is found and reported in Table 2. All of the interstate war im-

pacts reflect abrupt, temporary models, and only half of the omegas are significant. All of the global war omegas are significant, and all but one are associated with abrupt, permanent models.¹² The sole exception is the Japanese expenditures/GNP series, which requires the imposition of an abrupt, temporary model.

In contrast to the evident British, French, and American displacements owing to global war, World War I had no visible impact on Japanese expenditures and revenues, especially in comparison to the disturbance in the expenditure series caused by the 1904-1905 interstate war and the temporary expenditure spike during World War II. If it is fair to dismiss the first Japanese global war as a case of marginal participation (Singer and Small (1982) report 400 Japanese battle deaths for World War I), the second case suggests that defeat and occupation may interrupt the displacement effect, or at least alter the direction of displacement. Interestingly, however, Japanese revenues/GNP are permanently displaced by World War II even though expenditures/GNP are not.

Nevertheless, in three of the four expenditure cases and in all four revenue cases, the intervention outcomes are markedly different. The intervention impact of the global wars are permanent or sustained while the impacts of the interstate wars are either insignificant or transitory. It is of course possible to be more specific about the nature of these impacts. We could, for example, translate each of the logged omega parameters into estimates of percentage change before and after the interventions. The largest omega in Table 2, for instance, is the 1.01 parameter associated with the impact of global wars on the United States expenditure/GNP series. In percentage

¹⁰The intervention (I_t) is represented as a discontinuous variable that ranges between zero (denoting the absence of the event) and one (denoting the presence of the event). Although the effects of war are not always restricted to the actual duration of warfare, we will code as one only those years of actual warfare. To do otherwise could easily bias the attempt to assess the impact of war by capturing other, postwar influences on expenditures and revenues and thus incorrectly exaggerating or minimizing the war impacts.

¹¹McCleary and Hay (1980) suggest that when $\delta \geq 1.00$, the level of the series changes by the quantity ω_0 in each postintervention moment. In other words, before the intervention the series is trendless. After the intervention, the series follows a trend with the parameter ω_0 interpreted as the slope. Such a radical change (from a state of equilibrium to a state of growth) is more than likely due to a postintervention time series which is too short to encompass the equilibrium state of the process.

¹²Significant deltas approximating or exceeding the value of one were estimated in each of Table 2's global war intervention components (with the exception of the Japanese expenditure/GNP case). Substantively, this outcome may suggest that new expansionary influences were introduced in the aftermath of war. For our immediate purposes, however, the important thing to note is that the postwar levels in French, British, and American spending and revenue series (and the Japanese revenue series) did not return to the prewar levels, thus indicating abrupt, permanent changes. More precisely, what tends to occur is a very large temporary spike, especially in the spending series, that recedes to a level higher than that found in the prewar era. Efforts to estimate compound models (incorporating both temporary and permanent changes) were unsuccessful, we believe, due to the substantial parameter collinearity encountered and the often overwhelming size of the temporary spike. While Box-Tiao models offer certain statistical advantages, they also clearly possess some limitations in analyzing global war-induced changes.

Table 2. The Impact of War on Logged Expenditures/GNP and Tax Revenues/GNP^a

State	Interstate Wars ^b		Global Wars	Noise Model ^c	
	ω_0^d	δ	ω_0	N_t	X^2
Logged Expenditures/GNP					
Great Britain (1700-1980)	.13* (4.8)	.72* (6.9)	.39* (7.8)	white noise	18.6
United States (1792-1980)	.15 (1.8)		1.01* (7.6)	white noise	20.1
France (1815-1979)	.07* (2.4)		.50* (6.7)	white noise	18.4
Japan (1879-1980)	.27* (3.4)		.65* (5.2)	white noise	12.4
Logged Revenues/GNP					
Great Britain (1700-1980)	.03* (2.6)	.87* (6.2)	.08* (2.6)	white noise	12.0
United States (1792-1980)	-.06 (-1.2)		.30* (2.4)	white noise	19.4
France (1815-1979)	-.02 (-1.2)		.27* ^c (5.7)	white noise	12.4
Japan (1878-1980)	.05 (1.3)		.20* (2.3)	$\theta_1 = .41^*$ (4.3)	15.2

Note: No statistically significant trend parameters were encountered; *t*-values are reported in parentheses.

*Statistical significance at the .05 level.

^aAll data have been logged in order to achieve variance stationarity.

^bThe impact of interstate wars was abrupt and temporary, whereas the impact of global wars was abrupt and permanent.

^cThe first seven noise models are white noise, first differenced series (0,1,1). The Japanese revenue noise model is a first differenced, moving average process (0,1,1). $X^2 \leq .05$ where H_0 : residuals are white noise.

^dThe reported omega is based on a five-year lag for World War I.

change terms, the 1.01 parameter can be interpreted as an approximately 175% change in the expenditures-to-gross-national-product ratio. This figure can be compared to the insignificant and temporary 16% change ($\omega_0 = .15$) associated with the expenditure/GNP impact of the United States participation in interstate wars. Yet it should be kept in mind that we are modelling multiple interventions over fairly long periods of time and that the omega parameters and percentage changes reflect average, as opposed to cumulative, effects. Calculating percentage changes in this context, therefore, does not really provide us with a more concrete numerical handle for interpreting and comparing the specific outcomes. Only the metric of the parameter is changed. Although such changes are helpful when the impact of a specific war is being analyzed, the interpretation advantage when the average impact

of wars is being assessed collectively seems quite marginal.¹³

This observation seems all the more true when the outcome is as clearcut as the findings sum-

¹³For those readers who wish to pursue the percentage change conversion procedure further, McCleary and Hay (1980, pp. 171-185) advance the following formula for translating omega parameters in log metric into percentage change intervention estimates:

$$\text{percentage change} = (e^{\omega_0} - 1)100,$$

where e^{ω_0} is the ratio of post-intervention to pre-intervention equilibrium. However, in models with significant delta parameters, the incremental post-intervention movement of the process requires the analyst to exponentiate the asymptotic change ($\omega_0/1-\delta$), in the process level.

marized in Table 2 indicate. The persistent effects of global war on both the expenditures and revenues of the states examined are simply of a different order of magnitude than the impacts of interstate war. The logged parameter values also permit us to point out an interesting difference between expenditure and revenue outcomes. In the aggregate, global war tends to exert similar impacts on both types of series—that is, permanent and statistically significant. But wartime displacements of revenues are far less dramatic than for expenditures. In fact, revenue increases during wartime tend to lag behind expenditure increases in terms of magnitude and timing. Revenue levels only begin to match expenditure levels toward the end, or after the end, of a war and only after expenditure levels have begun to decline.

This tendency could be viewed as somewhat contrary to what one might expect, based on Peacock's and Wiseman's argument that wars increase tax burden tolerances. Alternatively, it could be argued that dramatic expenditure increases drag revenues upwards and that the extent to which this occurs is dependent on the extent to which postwar expenditures decline and not the other way around. Unfortunately, there is only so far one can push this counter-argument with aggregate data. We need more detailed case studies of how wars bring about both temporary and permanent governmental expansion (see, for example, Burke, 1982; Marwick, 1974; Skowronek, 1982; Stein, 1980; Strickland, 1983).

Several data problems prevent us from examining many pre-twentieth century global wars. Although the limited French data that are available do indicate some, but less than expected, expenditure displacement between 1688-1713 and 1792-1815, our current empirical evidence is re-

stricted to the eighteenth- and nineteenth-century British record. Fortunately, however, these data encompass the period used by Peacock and Wiseman to reject the presence of permanent pre-twentieth-century displacements. Our analysis, whether all British global wars are lumped together or examined separately, suggest that British participation in global wars, at least since 1700, have brought about permanent displacements.¹⁴ How then do we account for the more general discrepancy between our findings and those of Peacock and Wiseman?

Table 3 lists Peacock's and Wiseman's 1792-1890 data on expenditures/GNP for selected years and contrasts it with our own corresponding data. Clearly, the numbers we are using are not exactly identical with those utilized by our predecessors, but the direction of movement is sufficiently similar to regard them as being in the same ballpark. However, there are two important differences. First, our series begins in 1700. This enables us to see a permanent displacement occurring between 1700 and 1720 (5 to 9% which roughly persists in peacetime through the rest of the century), an impact that probably would be

¹⁴This statement is not entirely true. When British global wars are examined separately (as opposed to being treated as a class of warfare as in Table 2), the impact of the 1701-1713 Spanish Succession war on expenditures/GNP is not statistically significant, even though the same war has a significant impact on revenues/GNP. We do not regard the 1701-1713 deviation as a meaningful exception, however, because we have only one year (1700) of prewar GNP data. Moreover, the 1701-1713 warfare constituted only the second phase of the 1688-1713 global war.

Table 3. British Expenditures, GNP, and Expenditures/GNP

	Expenditures/GNP (%)		Expenditures	GNP
	Peacock-Wiseman	Rasler-Thompson		
1700		5	4	70
1720		9	8	67
1792	11	9	16	182
1800	24	17	28	166
1814	29	29	61	209
1822	19	15	51	350
1831	16	12	48	413
1841	11	11	47	428
1850	12	10	61	596
1860	11	10	65	621
1870	9	7	65	1013
1880	10	6	84	1446
1890	9	6	113	1890

Note. British expenditures and GNP are expressed in 1913 pounds (000.000 omitted).

even more impressive if our data could be extended back through the 1680s.

The second difference is seen in Peacock's and Wiseman's expenditure/GNP level returning to the 1792 level by 1841. Our series suggests that this does not occur until some 20 years later in the 1860s. Nevertheless, both series indicate an eventual return to the pre-French Revolutionary/Napoleonic Wars equilibrium level thereby suggesting a less-than-permanent effect. But how long is "permanent"?¹⁵ The disturbance to our series does not subside for some 45 years, and even Peacock's and Wiseman's evidence indicates a 25-year effect, or nearly a generation in length. Regardless of how permanence is defined, it is equally clear that Great Britain enjoyed remarkable economic growth rates during the period in question. Between 1700 and 1792, the British economy expanded by roughly a factor of 2.5, although much of this growth was offset by population growth. In marked contrast, the size of the nineteenth-century British economy doubled approximately every 20 to 25 years. Any British government might have been hard pressed to expand their budgets at this same rate—particularly in an era consistently characterized (through the 1870s) by a conservative approach to governmental spending.

Thus, Peacock and Wiseman are not incorrect to treat the 1793-1815 displacement as less than permanent, but the nature of the evidence also requires careful consideration. In absolute terms or constant British pounds (see Table 3), there is no doubt that British governmental expenditures were displaced upward. In relation to economic growth, this upward displacement ultimately proved to be temporary. How long the effect took to be regarded as temporary depends upon whose data are used, but it is also evident that the "temporary" status is as much, if not more, dependent upon the intervention of the Industrial Revolution as it is dependent upon austerity budgets. Considered in conjunction with the permanent impact (on revenues/GNP at least) of the second half of the 1689-1713 global war, we should be most reluctant to restrict permanent expenditure displacement to the twentieth century and the evolution of contemporary ideas about the appropriate scope of governmental activities.

Our final question centers on the extent to which the permanent expenditure/revenue impacts of global war can be traced to military spending. The long view of state making stresses

the role of military and war expenditures and preparations as the primary impetus to the growth of the state organization. Yet we do not infer from this emphasis that the growth of the state is predicated solely upon the growth of military spending. On the contrary, preparing for war and interstate competition has had a number of spillover consequences, ranging from the need for a permanent bureaucracy to collect taxes in order to pay for sporadic warfare to the war-related growth of social services administered by the state. Titmuss (1969), for example, has argued for a close link between the ascending intensity of war and the evolution of British social policy. As an increasing proportion of the population has become involved in the warfare of the past few centuries, the state has been forced to expand its efforts to improve the health and morale of, initially, men available for combat, later, the next generation of recruits (especially children) and, ultimately, to the whole population as they have all become important to war efforts.

There seems little need to restrict this argument to the British experience for it is indeed a point of view that is quite compatible with what we are referring to as the broader view of the war making-state making linkage. At the same time, it also means that we have little basis—aside from the distinction about relevant crises—for expecting findings different from those advanced by Peacock and Wiseman. Global wars presumably should influence both military and nonmilitary expenditures.

In this examination, we will initially pursue a minimal definition of military spending by restricting the concept to those expenditures consumed directly by the state's armed forces. Accordingly, we will confine our empirical attention to the differential impacts of interstate and global war on military and nonmilitary expenditures/GNP, as reported in Table 4 for the four global powers.

The outcome reported in Table 4 is not quite as clearcut as the findings put forward in Table 2. With the exception of the American case, both interstate and global wars exert abrupt, temporary impacts on military spending. Only in the American case is there a temporary/permanent split between the impacts of the two types of war. Further analysis, however, suggests that the categorical approach to war impacts requires some qualification, depending on which global wars are involved. When the impact of each global war is examined separately, it is the pre-1939 global wars that are associated with temporary impacts. World War II, however, led to significant and permanent increases in British, American, and French military expenditures/GNP. Not too surprisingly, the Japanese experience requires the

¹⁵The effect lasts sufficiently long to be able to model the impact of the French Revolutionary/Napoleonic wars as exerting a permanent effect on the British series.

construction of a more complicated model—a temporary pulse model for war years followed by an abrupt, permanent model for the dramatic postwar decrease.

In contrast, the war-type distinction is operative for nonmilitary expenditures/GNP. The impact of interstate wars is nonsignificant for all four countries, whereas abrupt, permanent models are found to be applicable in the British, American, and French global war cases. The unusual Japanese case takes a gradual, permanent model in this instance, reflecting increases in nonmilitary spending after both twentieth-century world wars. Ironically then, it is nonmilitary, and not military, spending that is more systematically influenced by periodic global warfare. Nor is this phenomenon an innovation of the twentieth century.

Still, dividing expenditures into military and nonmilitary spending categories constitutes a limited test of the extent to which global war expands nonwar-related allocations. We can take this analysis one step further by removing some more of the war-related expenditures from the nonmilitary side of the ledger. Two prime budgetary

candidates for removal as explicitly war-related are interest payments on the national debt and veteran pensions. Series pertaining to the first item are relatively available for all four global powers. Data concerning veteran pensions, unfortunately, are somewhat less accessible for our long periods of time, but we were able to extract them for the Japanese and American cases.

Table 5 reports selected observations on two proportional series: 1) nonmilitary expenditures/GNP (nonmilitary expenditures equal total expenditures minus military expenditures, as analyzed in Table 4) and 2) nonwar expenditures/GNP (nonwar expenditures equal total expenditures minus (military expenditures plus debt interest payments plus veteran pensions) for the United States and Japan and minus (military expenditures plus debt interest payments) for Great Britain and France). The point of the exercise is not to compare the magnitude of the two sets of observations (the nonwar proportions must be smaller than the nonmilitary ones, by definition) but rather to check whether the global war changes in the nonwar series appear to behave

Table 4. The Impact of War on Logged Military and Nonmilitary Expenditures/GNP^a

	Interstate Wars ^b		Impact Type	Global Wars			Noise Model ^c	
	ω_0	δ		ω_0	δ	ω_0	N_t	X^2
Logged Military Expenditures/GNP								
Great Britain (1700-1980)	.32* (4.0)	.69* (4.9)	abrupt, temporary	1.60* (9.6)			white noise	11.9
United States (1792-1980)	.32* (2.9)	.71* (3.7)	abrupt, permanent	1.49* (8.3)			white noise	11.9
France (1815-1979)	.13* (2.3)		abrupt, temporary	1.10* (8.9)	.47* (5.8)		white noise	28.8
Japan (1878-1980)	.30* (2.1)		abrupt, temporary	2.32* (8.0)			white noise	29.9
Logged Nonmilitary Expenditures/GNP								
Great Britain (1700-1980)	.00 (-.5)		abrupt, permanent	.33* (4.7)			$\theta_1 = .50*$ (10.0)	15.7
United States (1792-1980)	-.04 (1.8)		abrupt, permanent	.52* (4.7)			white noise	18.4
France (1815-1979)	-.03 (-.6)		abrupt, permanent	.27* (3.2)			$\theta_1 = -.11*$ (2.2)	21.0
Japan (1878-1980)	.08 (1.9)		gradual, permanent	.35* (3.5)		.86* (9.5)	$\phi = -.35*$ (-3.5)	32.8

Note: No statistically significant trend parameters were encountered; *t*-values are reported in parentheses.

*Statistical significance at the .05 level.

^aAll data have been logged in order to achieve variance stationarity.

^bThe impact of the interstate was abrupt and temporary.

^cFive of the first six noise models are white noise, first differenced series (0,1,0). The British and French nonmilitary noise models are first differenced, moving average processes (0,1,1) whereas the Japanese nonmilitary noise model is a first differenced, autoregressive process (1,1,0).

Table 5. Selected Data on Nonmilitary and Nonwar Expenditures as a Proportion of GNP

	Great Britain		France		United States		Japan	
	Non-military	Nonwar	Non-military	Nonwar	Non-military	Nonwar	Non-military	Nonwar
1780	.057	.013						
1790	.070	.013						
1820	.110	.027						
1830	.088	.021						
1840	.071	.019						
1850	.076	.022						
1860	.064	.023						
1870	.045	.019						
1880	.040	.020						
1890	.038	.023	.096	.047	.019	.008	.063	.049
1900	.035	.024	.085	.046	.018	.008	.074	.062
1913	.042	.035	.061	.037	.010	.005	.083	.052
1920	.145	.092	.150		.045	.031	.054	.038
1930	.147	.084	.112	.055	.027	.018	.081	.051
1938	.132	.094	.174	.136	.065	.050	.173	.058
1950	.192	.155	.219	.207	.104	.076	.160	.158
1960	.175	.141	.181	.168	.096	.071	.108	.099
1970	.227	.202	.235	.212	.119	.094	.103	.096
1980	.285	.247	.272 ^a	.234 ^a	.172	.143	.176	.146

Note: The United States and Japan nonwar proportions exclude veteran pensions, whereas the British and French nonwar proportions are restricted to the common base (total expenditures - (military + debt interest spending)/GNP).

^a1979 values.

much differently from the previously analyzed nonmilitary series.

The nonwar spending series do seem to exhibit the same patterns of discontinuous growth and permanent changes before and after global wars observed in the more broadly defined nonmilitary spending series. Equally worth noting is the 1790-1820 doubling of the proportion of GNP devoted to nonwar purposes in the British case. The proportions admittedly are quite small, in accordance with pre-twentieth century austerity policies, and decline between 1820 and 1840 but, unlike the nonmilitary series, the upward shift in the nonwar proportion reflects a permanent change. Thus, Table 5 reinforces the support for the contention that the displacement phenomenon should not be dismissed simply as an artifact of postwar increases in war-related spending.

Conclusion

We have attempted to achieve several goals in the present study. First, we have sought to reduce some of the empirical ambiguities about, and disputes over, the existence of a war-induced expenditure displacement phenomenon. In contrast to the divergence of opinion in the literature, the relationship between global war and permanent

spending shifts in some of the world system's most important states (Great Britain, the United States, France, and Japan) is statistically and substantively significant. Moreover, the displacement that takes place is only partially a function of war-related expenditures. Nonwar-related spending is affected as well. Global war must therefore be considered one of the more important sources of the growth and expansion of the modern state.

Second, we have also attempted to make a distinction between Peacock's and Wiseman's hypothesized displacement effect and their tax tolerance explanation of the relationship. Demonstrating the existence of the displacements does little to confirm, or to disconfirm for that matter, the relatively independent idea that national crises provide opportunities for overcoming taxpayer resistance to greater revenue extraction efforts. Rather than seeking ways to compare taxpayer attitudes during war and peacetime, something about which we know very little (see Goetz, 1977), our own interpretive preferences are to stress the broader, historical context of the reciprocal interactions between war and state-building processes. For the older and most powerful states at least, an appreciation for the persistent role of war is central to explaining the growth and expansion of the state, whether or not governmental leaders choose

to pay much attention to citizen preferences on tax burdens.

Last but not necessarily least, the outcomes of the data analyses underscore the theoretical significance of a special category of warfare—the global war—that is far less appreciated than it deserves. There is no need to overstate our case by insisting that global wars are the only external events to have significant internal impacts. Other types of events, including the more frequent non-global wars, can and do have domestic repercussions,¹⁶ but we have been able to demonstrate that the spending and tax revenue patterns and, more generally, the organizational expansion of some of the system's leading states are more likely to be displaced and displaced permanently by global wars than they are by interstate wars. We suspect that we are only beginning to tap, in a systematic way, the domestic implications of the global war as both a catalyst and as an agent of socio-economic and political change.¹⁷

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¹⁶Intensive civil wars may also have a permanent impact on expenditures and revenues that is comparable to global war as occurred in the United States in the nineteenth century.

¹⁷Although an emphasis on global wars may not tell us much about the growth of states made independent since 1945, our findings should at least encourage other analysts of the ongoing expansion of public expenditures and revenues in industrialized states (e.g., Cameron, 1978; Gould, 1983; Kohl, 1983; Schmidt, 1983; and Taylor, 1981) to consider incorporating the effects of global wars in their models. In the 1983 conference version of this article, we extended our global war impact analysis to the twentieth-century expenditure and revenue experience of 22 states other than those examined here. Although these data are characterized by a number of missing values that hamper serial examination, the permanent impacts of global war do not appear to be restricted to the major or global contenders for systemic power. The expenditures and revenues of other war participants (including minor power active belligerents and occupied states) as well as some non-participating states that were located near the combat theaters appear to have been affected as well. However, the examination of these data introduce several analytical complications that cannot be discussed adequately in the space presently available and will have to be postponed for separate treatment in the future.

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Communications

Comment on Katz, Mahler, & Franz (Vol. 77, December 1983, pp. 871-886)

The empirical study by Katz, Mahler, and Franz (1983) of the impact of taxes on the distribution of income, investment, and economic growth is both simple and powerful. Unfortunately, it is also somewhat misleading. Katz et al. have shown that those aspects of taxes that they considered have no impact on investment and growth, whereas some aspects have an effect on the distribution of income. The problem is that other aspects of taxes may still have an effect on investment and growth. What is worse, Katz et al. neglected to consider precisely those aspects of taxes that should be expected to influence investment in the light of economic theory.

Nevertheless, the conclusions of Katz et al. that neither total taxes, nor the totals received from taxes on incomes, profits, and capital gains, nor sales and value added taxes, nor social security contributions have a significant effect on capital formation is of interest independently of the other ways in which taxes may influence investment. Katz et al. imply that there is no necessary trade-off between growth and income redistribution, no "big trade-off" between efficiency and equality. They open room for the possibility that governments can pursue growth-oriented policies under a reasonably egalitarian distribution of income. The results of their analysis are consistent, therefore, with the empirical studies that conclude that partisan control does matter for the distribution of income, levels of welfare spending, or both (Cameron, 1978; Castles, 1982; Hicks & Swank, 1983; Shalev, 1983) but not for the rate of capital formation (Cameron, 1983).

Our purpose here is 1) to present the correct model, according to neoclassical economics, of the effects of profit taxes on investment, 2) to provide an explanation for the finding by Katz et al. that the total revenue collected from taxes on profits is to a large extent independent of the rate of investment, and 3) to show that this result is not in contradiction with the empirical studies by economists (Almon & Barbera, 1980; Bischoff, 1971; Hendershott & Hu, 1981; Jorgenson, 1971), which conclude that the rate of taxation of profits and the rate of tax relief for investment do affect the rate of investment.

A Model of Investment with Taxes on Profits

According to the standard theory of investment, firms choose that rate of investment which

maximizes the present value of current and future profits, where profits are measured after taxes and net of the cost of investment. A recent and useful review of the literature on taxes and investment by Auerbach (1983) presents the following specification of the firm's investment decision:

$$\max_{I(t)} \int_0^{\infty} e^{-rt} [R(t) - I(t) - T(t)] dt, \text{ where} \quad (1)$$

$$R(t) = R[K(t)], \text{ with } R' > 0 \text{ and } R'' < 0, \quad (2)$$

$$I(t) = K'(t) + \delta K(t), \text{ and} \quad (3)$$

$$T(t) = \tau R(t) - (k + \tau Z)I(t). \quad (4)$$

Reading equation (1) from left to right: r is the real rate of interest, $R(t)$ is the before-tax revenues or cash flow received by the firm, $I(t)$ is gross investment, and $T(t)$ is the total tax liability of the firm, all in period t . By equation (2), the revenues of the firm are a function of the capital stock in place at time t with additions to capital yielding positive but diminishing marginal returns. By equation (3), gross investment is equal to the net changes in the capital stock, $K'(t)$, plus depreciation, $\delta K(t)$. Finally, by equation (4), the total tax liability equals the tax rate τ assessed on total revenues $R(t)$ minus investment tax credits of $kI(t)$ and minus the present value of deductions for depreciation $\tau ZI(t)$. The term k is the tax credit given per unit of investment while Z represents the present value of the depreciation allowance. Assuming for notational simplicity that relative prices are constant, the solution to the maximization problem (equation (1)) consists of an initial investment to attain the optimal (profit-maximizing) level of capital and future investment sufficient to maintain the stock of capital at its optimal level. In mathematical notation, the profit-maximizing firm will choose the path of investment given by:

$$I(0) = K^* - (1 - \delta)K(0), \text{ and} \quad (5)$$

$$I(t) = \delta K^*, \quad t > 0, \quad (6)$$

where K^* , the optimal level of capital, is given implicitly by the Euler equation:

$$R'(K^*) = (r + \delta) \left[\frac{1 - k - \tau Z}{1 - \tau} \right]. \quad (7)$$

The effects of profit taxes on investment are entirely captured by the ratio $(1 - k - \tau Z)/(1 - \tau)$

in equation (7). This is the ratio of the marginal cost of investment divided by the marginal gain from additional revenue. As this ratio falls, the marginal revenue product of capital must decrease which implies, by the property of diminishing returns, that the profit-maximizing level of capital stock, K^* , is higher. Note that it is not the variable measured by Katz et al., total profit taxes, $T(t)$, nor the effective profit tax rate, $T(t)/R(t)$, which influence investment, but the combination of the nominal profit tax rate, τ , the rate of investment tax credit, k , and the depreciation allowance, Z , found in equation (7).

Investment and Income Distribution

The fact that it is not total profit taxes, $T(t)$, but the ratio $(1 - k - \tau Z)/(1 - \tau)$ which influences investment leaves open the possibility of separating, to some extent, investment policy from fiscal policy. For example, consider the tax reform favored by the Reagan administration and other believers in the free market: the abolition of the corporate profit tax as well as investment tax credits. This amounts to a tax schedule with $\tau = k = 0$, in which case the market will produce the level of capital stock given by:

$$R'(K^*) = (r + \delta). \quad (8)$$

However, it is apparent that exactly the same result can be obtained from any tax schedule that satisfies the condition: $\tau = k + \tau Z$. This condition is equivalent to levying a tax on uninvested profits only, or $T(t) = \tau[R(t) - I(t)]$. The Reagan program is to tax uninvested profits at the rate of zero, but whether the tax rate τ equals zero or approaches unity has no effect on the profit-maximizing level of capital stock nor, therefore, on investment. Of course, the effects on tax receipts of varying the tax rate can be substantial.

The example can be generalized by letting the government choose a target level of capital, denoted $K^*(\Theta)$, where Θ is defined by

$$R'(K^*) = (r + \delta)\Theta. \quad (9)$$

The parameter Θ would differ from unity if the government thought that the real interest rate was not the appropriate social discount rate. Note that $dK^*/d\Theta < 0$, because a decrease in Θ implies a decrease in the marginal revenue product of capital which, by diminishing returns, implies an increase in K^* . The government, in order to attain its target $K^*(\Theta)$, must choose a tax schedule such that:

$$(1 - \tau)\Theta = (1 - k - \tau Z). \quad (10)$$

Again, any tax schedule which satisfies equation (10) will have identical effects on investment. The total taxes collected from profits will equal:

$$T(t) = \tau R(t) - [1 - \Theta(1 - \tau)]I(t). \quad (11)$$

With Θ fixed by the government's target level of capital stock, the total taxes collected and, thus, available for income redistribution depends entirely on τ . Letting τ take any value greater than or equal to 0 and less than unity, there is associated with every target level of capital a range of possible total tax receipts:¹

$$-(1 - \Theta)I(t) \leq T(t) < R(t) - I(t). \quad (12)$$

Note that if $\Theta < 1$, a tax schedule with a low nominal tax rate, (with $\tau \rightarrow 0$), implies a subsidy to firms. Thus the choice of investment policy is independent of the choice of fiscal policy within the bounds of equation (12).

The analysis is not quite complete, however, because the bounds on tax collections are not independent of the target level of capital stock. Both $R(t)$ and $I(t)$ depend on the government choice of Θ . It can be demonstrated that as the target level of capital increases, the lower bound on tax receipts in any period declines (or the upper bound on subsidies to firms increases). More interesting is the impact of the government's choice of investment target on the upper bound of tax revenues, $[R(t) - I(t)]$, for it is the upper bound that limits the possible redistribution that can be achieved through profit taxes. This can be read from the following derivatives:

$$\begin{aligned} d(R - I)/d\Theta &= -(dK^*/d\Theta) > 0 \text{ when} \\ t &= 0, \text{ and} \end{aligned} \quad (13)$$

$$\begin{aligned} d(R - I)/d\Theta &= [R'(K^*) - \delta](dK^*/d\Theta) \\ &< 0 \text{ if } R'(K^*) > \delta \text{ when } t > 0. \end{aligned} \quad (14)$$

In the initial period there is always a trade-off between the target level of capital stock, or equivalently, the target level of investment, and the maximum tax receipts from profits. In subsequent periods, however, there is no such trade-off. As

¹This assumes that $-(1 - \Theta)I(t) < R(t) - I(t)$, or $\Theta I(t) < R(t)$. This is always true for $t > 0$: $R(K^*)/K^* > R'(K^*) = (r + \delta)\Theta > \delta\Theta$ which implies that $R(K^*) > \Theta\delta K^* = \Theta I$. It need not be true, however, for $t = 0$ if a firm initially invests more than its after-tax profits by borrowing. What would happen in such a case depends on whether or not the firm can sell unused tax deductions. It is possible that a higher τ would increase the value of the unused tax deductions and thus improve the firm's profits in the first period.

long as $R'(K^*) > \delta$, the government can simultaneously induce a larger amount of capital formation and increase the taxes collected from profits.²

Katz et al. ask "Do lower marginal tax rates on . . . profits in fact spur sufficient private investment to promote steady, continuous growth? . . . Is there, indeed, necessarily a trade-off between the capitalist requirement for productive incentives and the democratic demand for a more egalitarian income distribution?" (1983, p. 873). The answers provided by the neoclassical theory of investment are yes to the first question and no to the second. Lower nominal tax rates on profits, higher rates of investment tax credits, and more generous depreciation allowances do induce greater private investment.³ But this does not mean that greater investment must be purchased with reduced tax collections from profits and, hence, reduced financial support for government programs or transfers. A high rate of tax on profit together with high rates of investment tax relief can be as effective in inducing a rapid rate of capital formation as a low rate of tax on profit, and the former policy will yield substantially higher tax revenues. The social democratic program of using the tax system to induce firms both to invest and to help finance welfare programs is as compatible with neoclassical economic theory as Reagan's proposal to eliminate taxes on profits and allow the rate of investment to be set by the market alone.

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Reply to Douglas D. Rose (Vol. 78, March 1984, pp. 204-205)

With his cordial comment on my article (Vol. 76, June 1982, pp. 239-258), Professor Douglas D. Rose has welcomed those findings that confirm "our prior expectations" and rejected those that do not. Thus, his analysis quietly discounts my thesis that politicians do not endorse rules of the game as reliably as is implied by traditional constitutional commentaries or by modern democratic theory. Although I readily accept parts of his criticism, and I would like to believe that politicians are indeed praetorian guards of representative government, I'm afraid that both my judgment and my data still better suit the cynicism of Lord Denning than the optimism of V. O. Key.

The British Constitution is unwritten, unprotected by judicial review, and exists mainly in the minds of the politicians. But my survey instrument found that the politicians were of two minds and held two quite different constitutional inter-

²If $R'(K^*)$ reflects the social as well as the private return from capital, it is irrational in any economic system to invest to such an extent that $R'(K^*) < \delta$. This would imply that the cost of maintaining the capital stock, the cost of replacing deteriorated plant and equipment, exceeds the marginal gains in production such a level of capital stock makes possible. Aggregate output, both present and future, could be increased by disinvesting. The equality $R'(K^*) = \delta$, in fact, is the golden rule of growth economics when the population is stable: This is the level of capital which yields the highest sustainable level of aggregate consumption.

³To be more exact, lower nominal tax rates on profits induce greater investment as long as $k + Z < 1$.

pretations. This, I believe, is the target of Professor Rose's comment. The first and older interpretation, the deliberative, is strongly supported by Conservatives and appreciated by Labour. The other and newer interpretation, the representational, is admired more exclusively, although not entirely so, by Labour alone. Thus, constitutional dissensus seems to occur mainly with the newer representational view. And it is measures of these doctrines that Professor Rose finds flawed. If I understand him correctly, he holds that the inter-party dissensus here is the article's news, and that, if it is discredited successfully, we are left with "prior expectations," that is, with comparative consensus behind the deliberative interpretation. This is a little misleading, for the comparative consensus behind the deliberative interpretation isn't so very consensual at all. Inter-party disagreement on two of its three core beliefs is 20 and 29% (Table 2). Furthermore, intra-party disagreements among ideological groups run up to 50% on some of the attitudes (Table 3). Finally, orientations toward this supposedly consensual deliberative interpretation swim in a sea of political values such as strong government and social equality (Table 4). In short, even if Rose were to discredit his items successfully, the remaining data would still sustain the argument that the contemporary force of political bias in politicians' thinking about rules of the game has been underestimated.

I'm not convinced, however, that the items he singles out must be banished. They may be weak, but they are not at all as invalid as he claims. These measures, like the others, were constructed carefully with the help of British colleagues who specialize in the study of Parliament. They were also pre-tested with former MPs who were not included in the study's sample. Certainly they could be improved. Yet, despite their problems, they seem to me to shed considerable light on how politicians think about constitutional topics. Rose's objections to these items are twofold: one, they are not clearly connected to doctrines they purport to measure; two, they suffer from ambiguity. I agree with Professor Rose that, from a constitutional perspective, they have their ambiguities, but I think I can clear up some of the connections with the doctrines.

Part of the confusion may arise from the fact that there was no space in the article for interpretations of each item, and that rationales for the ones Professor Rose questions may not be so obvious because they have much to do with complaints and controversies at the time of the interviews. The three measures concerning "role of the electorate," for example, are not direct measures of a precise convention. Indeed, the convention itself has never been precisely defined. What is

clear, however, is that a basic principle of the contemporary Constitution is that the public's views should be heard and should have a significant impact upon public policy. Thus, when strong and efficient government is overdone (Appendix item 4), as Prime Minister Heath was accused of overdoing it, the snap of decisive action curtails responsiveness to public opinion and dilutes commitments to majority rule. In the same vein, there was much controversy at the time about privacy and secrecy in government (item 5) which, as the Franks Report argued, dries up streams of information needed by citizens for serious participation in formulating public policy. If the constitutional position on the electorate's role is ill-defined, it nevertheless at least prescribes communication with constituents between elections as well as participation in parties and local government. Throughout the 1970s, there was much concern at Westminster over political alienation and lack of public involvement in political life. Hence, rejection of cynicism about increasing citizen control over governmental affairs (item 3) was interpreted as support for the norm. The fact that these three items also intercorrelated in predicted directions should add confidence to the judgment that they are tapping aspects of "role of the electorate" and can legitimately be combined in an index.

Three of the other items Rose criticizes address the doctrine of "individual responsibility" which holds that Ministers are answerable to the House of Commons for their Department's work and can, for serious errors, be compelled to resign. This norm is intended to insure ministerial accountability to Parliament. And there was considerable annoyance at the time over actions of some Ministers who, using various subterfuges, seemed to be eroding the doctrine. To buttress this principle and defend their own positions, MPs from all parties sought greater opportunities to question Ministers properly (item 8). They also demanded more information which could be used to examine the competence of Ministers and their Departments (item 9). It was further argued that individual responsibility was being undermined by the Prime Minister's rigorous approach to party discipline and by his ever-growing influence (item 10). As the balance of power seemed to be tilting towards the Executive, those most concerned about individual responsibility fought to preserve the few weapons they still possessed. Again, Rose is correct that none of the three items measures the literal doctrine. But each seems to me to assess significant aspects of dispositions toward it, and as these items were likewise correlated in predicted directions, they too were combined in an index. Moreover, their performance in the analysis is much the same as that of others which might be

taken as more literal statements of constitutional norms (e.g., parliamentarism): both are viewed by politicians through lenses deeply tinted with political bias.

I hadn't supposed the findings were so very controversial. After all, they simply support criticism found in recent books, bills, early day motions, speeches and letters to *The Times*. Perhaps Professor Rose will be assuaged with the assurance that the data have already done their bit for "prior expectations" in another article where, consistent with the Dahl-Key theory, politicians' attitudes toward rules of the game were found to be much more robust than the public's. The present article merely adds that these attitudes are, at the same time, pulled and pushed about by political forces shaping politicians' outlooks. Yet British representative government stands firm. Apparently it can tolerate from its leaders more dissensus on fundamentals than had been thought.

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Erratum

In "The Political Bases of Citizen Contacting: A Cross-National Analysis" by Alan S. Zucker-
man and Darrell M. West (Volume 79, pp.
117-131), the last paragraph on p. 119 should
read: "In order to test these models of citizen con-
tacting, we examined data found in the seven-
nation study conducted by Verba et al. These sur-
veys contain a wealth of information about con-
tacting as well as other forms of participation."

Forthcoming in September

The following articles have been scheduled for publication in the September 1985 issue.

- Richard Arneson, "Marxism and Secular Faith: Was Marx a Bad Public Choice Theorist?"
- Larry M. Bartels, "Expectations and Preferences in Nominating Campaigns"
- Jonathan Bendor and Terry Moe, "An Adaptive Model of Bureaucratic Politics"
- James Ceaser, "Alexis de Tocqueville on Political Science, Political Culture, and the Role of the Intellectual"
- Robert Goodin, "Vulnerabilities and Responsibilities: An Ethical Defense of the Welfare State"
- Arthur S. Goldberg and John R. Wright, "Risk and Uncertainty as Factors in the Durability of Political Coalitions"
- Joseph Greenberg and Shlomo Weber, "Multi-party Equilibria under Proportional Representation"
- Jonathan Kelley, Ian McAllister, and Anthony Mughan, "The Decline of Class Revisited: Class and Party in England, 1964-1979"
- Amnon Rapoport and Esther Golan, "Assessment of Political Power in the Israeli Knesset"
- Steven B. Smith, "Althusser's Marxism without a Knowing Subject"
- Karen Strom, "Party Goals and Government Performance in Parliamentary Democracies"
- Eric Uslaner and M. Margaret Conway, "The Responsible Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974"

BOOK REVIEWS

Review Essays

Development and the Politics of Administrative Reform: Lessons from Latin America. By Linn A. Hammergren. (Boulder, Colo.: Westview Press, 1983. Pp. xvi + 213. \$19.00, paper.)

No Shortcuts to Progress: African Development Management in Perspective. By Goran Hyden. (Berkeley: University of California Press, 1983. Pp. xv + 223. \$35.00, cloth; \$9.50, paper.)

Development Projects as Policy Experiments: An Adaptive Approach to Development Administration. By Dennis A. Rondinelli. (New York: Methuen, 1983. Pp. ix + 167. \$21.00, cloth; \$9.95, paper.)

The themes of the three books under review deal with the problems and opportunities that exist for the field of development administration and management as this academic and practical field enters its fourth decade. These three books also provide distinct but complementary views of the history of development administration. Taken as a whole, the three works contribute to a better understanding of past experience, of the present, and of what might be the future environmental and institutional constraints to development, both globally as well as in specific regions of the world.

The descriptions, analyses, and prescriptions of these authors vary given their academic backgrounds and experiences. At the same time, there are interdisciplinary thrusts that the authors recognize and attempt to employ.

Dennis Rondinelli's work is more global than the works of Linn Hammergren and Goran Hyden. Rondinelli concentrates on development policies as they have affected development projects with the intention of reorienting development administration. On the other hand, Linn Hammergren tackles the issue of administrative reform within the context of Latin America, concentrating on three cases: Peru, Venezuela, and Colombia. Goran Hyden focuses upon still another theme in development administration: the contextual or environmental issue relating to Africa, in particular, sub-Saharan Africa. All three authors tend to have a pessimistic view of both the past and present, but also attempt to provide an optimistic future if certain conditions—particularly political and social—are met by both lending agencies and recipient countries.

Rondinelli's approach/focus is on a larger scale (in terms of issues dealt with) than the other two books. *Development Projects as Policy Experiments: An Adaptive Approach to Development Administration* is part of a Methuen and Company series on development and underdevelopment edited by Ray Bromley and Gavin Kitching. Rondinelli is a well-known and widely published development planner and has substantial consulting experience with international and bilateral lending agencies. His task, as he describes it, is to "explore . . . the growing divergence between the nature of development problems and the principles of development administration . . . as they have been defined by international assistance agencies and the governments of many developing countries over the past two decades" (p. 2). He contends that a major dilemma in this field is that planners and policymakers are control-oriented, and that these controls "inhibit the kinds of analyses and planning that are most appropriate" to dealing with the "increasing uncertainty and complexity of development problems" (p. 2).

After providing a critique of the experience with development since its post-World War II beginnings (chap. 1), emphasizing the control of development activities rather than facilitating the encouragement of "flexibility, experimentation, and social learning . . . essential to implementing development projects successfully" (p. 2), Rondinelli examines development policies as social experiments (chap. 2), the design of development projects (chap. 3), the implementation of development projects as policy experiments (chap. 4), and the need to reorient development administration (chap. 5).

Chapter 5 outlines the case he wants to make. Rondinelli rejects both the "transfer of administrative procedures and techniques from industrial countries" (the "tool-oriented" approach) and the transformation and modernization of political processes and administrative structures (the "Weberian model" approach) of the 1950s and 1960s (p. 116). His alternative approach for the future is "adaptive administration" (p. 120). This approach involves adjusting planning and administrative procedures to the political environment of public policymaking. It also involves increasing the responsiveness of organizations (through participation and market surrogate

arrangements), using a learning based approach to planning and administration, increasing administrative capacity, decentralizing authority (for development planning and administration), building local organizational capacity and institutional service delivery networking, utilizing "adjunctive" and strategic planning, simplifying planning and management procedures, encouraging error detection and correction, and, finally, creating incentives for innovative management. Rondinelli's concluding note emphasizes his desire to make development planning and administration adaptive. "Let good managers manage . . . [and] . . . reward them for their efforts and results" (p. 148).

Rondinelli's contribution to the field is basically one of simplifying and synthesizing the issues involved in development project planning and implementation. Scholars familiar with the field will recognize his description of past and present development policies, but may disagree with both his prescriptions for the future and the extent to which all of his suggestions could be effectively incorporated by lending agencies and recipient countries. Regardless of one's possible disagreement with this book, it is well documented, and its bibliography and index enhance its usefulness in a classroom setting.

"Administrative reform, a central element of development strategies twenty years ago is today in so much disrepute that it has all but disappeared from the vocabulary of development theory" states Linn Hammergren in her introductory chapter (p. 15). In examining the three middle-income countries of Latin America—Peru, Venezuela, and Colombia—she traces their reform movements and attempts to generalize from their experiences. Several common themes for administrative reform success and failure are developed in the three case studies. In Peru, the reasons for failure include the counterproductivity of the experts' monopoly on the reform movement; the lack of political skills of the reform experts; and the isolation of the organization responsible for reform, which did not possess the power to act on its own. In Venezuela ("The Reformer's Reform" as Hammergren subtitles chapter 5), some change did occur, but the extent to which the majority of the population became aware of this was limited because the reforms did not directly affect "efficiency and efficacy at the point of contact with the bureaucracy" (p. 125). The Colombian reform experience (subtitled "Institutionalized Reform") was not dominated by experts as were the other two cases. The Colombian reforms were less ambitious and limited to reorganization of individual bureaucratic entities "rather than the bureaucracy as a whole" (p. 134). In addition, the Colombian reforms were

drawn up by committees of politicians and administrators. The changes were incremental but, nevertheless, accomplished.

Hammergren's conclusions in chapter 7 focus on the policy process, the political variables, and the organizational variables that emerge from her three case studies. These must be related to the ends or objectives of reform. Most importantly, she emphasizes the need to relate the reform of institutions, in the name of efficiency, to the governments' (or regimes') desires to improve the effectiveness of government programs. The smooth internal functioning of a bureaucracy is no substitute for the timely, effective delivery of goods and services.

Development and the Politics of Administrative Reform presents documented, scholarly, and practically oriented material that buttresses what many in the field have begun to recognize: Administrative reform (as conceptualized in the 1950s and 1960s) is no universal panacea for the problems of public program design and implementation.

Goran Hyden presents still another theme in development management: the contextual, environmental issue. Although *No Shortcuts to Progress* was originally to be a textbook on African public administration in the mid-1970s, the focus changed to "a critical look at the context in which administration and related development management activities are being conducted" (p. ix). The book's content deals with "the historical macro context in which African management is being managed" (p. ix).

Hyden's chapters deal with African economy and society, governance and politics, policy-making and administration, decentralization and parastatals, participation and accountability, and local capacity. His final two chapters (7 and 8) present implications for donor agencies and prescriptions for change (titled "No Shortcuts to Progress").

Hyden's approach has elements of economic determinism. In the Introduction he states, "This book is based on a simple thesis, originally developed by Marx that the economic element is, in the broadest sense, determining human progress" (p. xiv). He concludes his work by stating, "The point made in this book is a very simple one, fetched straight out of a Marxist textbook . . . a society is the captive of its own history and that history has its own phases determined by the material conditions prevailing at any given time. . ." (p. 213). What is required, Hyden maintains, is for African countries to effectively institutionalize "structure which presuppose macro development" (p. 213). Interestingly, he is certain that Africa will not undergo socialist revolutionary transformations. He is also con-

vinced that accelerated development will also not occur until African countries first establish the structures necessary for such development.

This is a broad ranging work that will not necessarily find a warm reception among certain African scholars or among those who advocate either the "tool-oriented" or Weberian theories of development administration. Both theories assume that structures or institutions predisposed to development do, in fact, exist. Hyden's analysis maintains that the structural development fundamental to the transformation of Africa from its pre-capitalist stage is absent.

Rondinelli and Hyden, and to a lesser extent Hammergren, deal with the fundamental problems confronting development administration and management, problems that have been recognized but not resolved by organizations such as the World Bank and USAID. (See, in particular, the *World Development Report, 1983*, Part 2, "Management in Development," Oxford University Press, 1983, pp. 41-127.) Reconceptualizing and refocusing development administration, as envisioned by these authors, is exceedingly difficult.

Currently, the Organization for Economic Cooperation and Development (OECD) countries, along with the Organization of Petroleum Exporting Countries (OPEC), are providing approximately \$34 billion annually in Official Development Assistance (ODA) to the less-developed countries of the world. But each year, it becomes increasingly difficult to secure these funds. Consequently, the changes proposed for recipient countries in these three books are worth consideration. Internal resource mobilization and administrative changes in development planning and implementation are agenda items that the world's poorest countries must consider and act upon.

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Media Politics: The News Strategies of Presidential Campaigns. By F. Christopher Arterton. (Lexington, Mass.: D.C. Heath, 1984. Pp. xiv + 220. \$27.00.)

Channels of Power: The Impact of Television on American Politics. By Austin Ranney. (New York: Basic Books, 1983. Pp. x + 207. \$14.95.)

Over the Wire and On TV: CBS and UPI in Campaign '80. By Michael J. Robinson and Margaret A. Sheehan. (New York: Russell Sage Foundation, 1983. Pp. xv + 332. \$24.95, cloth; \$8.95, paper.)

Students of media and politics have taken giant strides since their early efforts to describe their

interrelationship. Generally, early scholars thought of the media as an outside influence on politics, and the conclusion was soon drawn that any influence involved was minor at best, despite arguments by Walter Lippmann and Douglass Cater. But contemporary scholars now view media as essential, not secondary, elements of the political system, and we are on the verge of integrating them into studies of the political process. So long as the impact of the media was seen as idiosyncratic and as essentially nonpolitical, much as the impact of weather on voting turnout, we could draw only an incomplete (and misleading) picture of the operation of the political system.

A more complete understanding of politics will only be possible when we have succeeded in incorporating all the basic elements into one theoretical scheme. But the incorporation of the media into a theory of politics has proven difficult: They are neither similar to interest groups, nor are they a "fourth branch of government." In the last decade, a spate of books and articles has dealt with the impact of media coverage on politics, arguing that it is not the event, but the report of the event, which shapes the political process, with Watergate and the Vietnam War frequently cited as examples. Further studies have examined the influence of the media's own preferences and goals on their news reporting, preferences and goals that make the media themselves political actors. The authors of the books reviewed here all take a somewhat different, and I think worthwhile, perspective: Media coverage changes the nature of the political process.

Arterton examines the impact of news reporting practice on the conduct of political campaigns and attempts to assess the consequences of this impact in *Media Politics*. His evidence comes from extensive interviews with campaign personnel and journalists during the 1976 and 1980 presidential campaigns. He finds that journalists and campaigners "interact on three conceptually distinct levels—personal, organizational, and substantive" (p. 37), each of which he treats in a separate chapter. He concludes that presidential campaigning in the late twentieth century differs markedly from that of earlier eras to the detriment of representation and coalition-building. Correcting these disadvantages to media politics, Arterton says, "will occur only when initiated by journalists" (p. 208).

Arterton proposes a theoretical framework for examining the media as an integral part of the political process. He accords the media a participant status in campaigns: The strategies and tactics of campaigners are directly affected by the media's practices. Organizational considerations of both the campaign organization and the media organizations exert a significant and reciprocal in-

fluence on each other. Campaigners treat the press as "more than simply a surrogate for the electorate" (p. 198); Arterton points out that "relations with journalists . . . shape what campaigners actually do in their attempts to mobilize electoral support" (p. 201). In contrast to the journalists' conception of themselves as outside the campaign process, campaigners view journalists as an essential element in campaigning. Readers will find the figures on pages 22 and 23 especially insightful on this point.

Robinson and Sheehan set a monumental task for themselves, "a twelve-month 'press watch' " (p. 10), to evaluate journalistic performance and to determine what differences, if any, exist between the new electronic media and more traditional print journalism in covering presidential campaigns. They find, generally, that both media adhere relatively closely to their explicit professional standards of fairness, objectivity, and access. But they point out major differences between these media as well: greater concentration on the presidency for television, more thematic coverage for network news, and a tendency for television to interpret even official presidential actions only in the context of the campaign. They conclude that "the commercial media did a better job in 1980 than most critics allowed, albeit a worse job than the media themselves pretended" (p. 304).

Robinson and Sheehan painstakingly characterize the differences in campaign coverage between the two media, noting that, compared with print, network journalism "is more inferential and hostile [and] shades markedly toward the didactic" (p. 216). Television news focuses more on the president than do newspapers, and it "politicizes news much more" (p. 212) than does print. These and other conclusions lead the authors to "speculate about politics under a new system of political reporting" (p. xiv). Their concluding section, titled appropriately enough, "Conclusions and Implications," is the most interesting part of *Over the Wire and On TV*, because here the authors treat media coverage not as an outside influence but as a major determinant of political events. Robinson and Sheehan stop short of formulating a theory of politics that explicitly incorporates the media (not, after all, their purpose), but they point out that "network journalism has played a part in changing the tenor of American mass politics" (p. 262).

Channels of Power goes beyond electoral politics; Ranney's attention is focused, as his subtitle suggests, on the impact of television on American politics. That impact is a result of the fact that for most of us, "televised political reality is 'real reality' " (p. 30), and that television (as well as other media) has an unavoidable structural bias that has

a systematic impact on American politics. In contrast to the other two works, Ranney's monograph does not report research findings. Instead, it is composed of revised lectures delivered in 1981 at the University of Michigan School of Law. The lecture format gives Ranney the opportunity to explore the impact of television's portrayal of politics, and it leads him to ask a central question, "what is political reality in the 1980s?" (p. 5). It is because voters respond to political reality as portrayed on television that its patterns become significant; it is because politicians must act in the context created by that coverage that its portrayal becomes important.

The bulk of Ranney's analysis is devoted to speculating about television's influence, detailing changes in the American political culture, in the types of politicians who seek public office, and in the process of governing—all traceable in his view to the emergence of television as a dominant medium. He argues that television raises expectations about presidential action and about prompt resolution of complex crises, and that the "glare of television's attention has helped significantly to weaken the ability of presidents and congressmen to govern" (p. 154). He concludes with a call "to preserve and renew those values and institutions that we believe have been dangerously weakened in the television age" (p. 182).

Channels of Power is, in its scope, the broadest of the three books under review. Its main points are thought provoking (although personally I feel that the impact of television is overstated). Clearly, however, Ranney presents an attempt to bring the study of media politics within the purview of the science of politics. His conception of political reality has some promise of providing a focus for further research by political scientists interested in the media. The concept plays an important role in Arterton's analysis; campaigners necessarily worry about the political reality presented by journalists, and the campaign news reported by the media depends in large part on the activities of campaigners hoping to influence that presentation. And Robinson and Sheehan conclude that television and wire services present different pictures of political reality, traditional print treating national politics "descriptively and officially," and television news "analytically and critically" (p. 290).

Treating these works as representatives of a developing trend in media analysis, we can see clearly that the traditional question concerning the impact of the media on politics is mistaken; it is as misguided as asking what the impact of interest groups on politics is. The point is that the media are integral parts of the political system, not outside influences on it, and as such must be studied within that context. The political theories

that explain, or purport to explain, political phenomenon must also be able to account for media influence. A careful reader of these books cannot help but conclude that research that treats the media in isolation from the political context misses the boat. Arterton's careful analysis demonstrates the centrality of media in at least

one aspect of the political process; future researchers will, I am sure, discover complex inter-relationships between policymakers and press in other areas as well.

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American Government and Politics

Inside the Legislative Process: The Passage of the Foreign Service Act of 1980. By William I. Bacchus. (Boulder, Colo.: Westview Press, 1984. Pp. xxii + 148. \$15.00.)

William Bacchus continues to contribute to our knowledge of the processes involved in organizing for foreign policy which he first elucidated in an essay in the *American Political Science Review* (1974, 68, 736-748). Additionally, we gain insights into the nature of relations among bureaucrats, congressional actors, and White House officials in the formulation of legislation. The specific bill discussed is the first sweeping revision of Foreign Service legislation since the Foreign Service Act of 1946. Bacchus's book will be of interest to personnelists, foreign policy students, and congressional scholars.

Bacchus starts with the premise that political scientists should examine low-visibility legislation because it is the norm for the legislative process. He develops a piece of participant-observer work of the finest sort aided by his service as one of the two main congressional liaison officers in the Department of State responsible for passage of the bill. Bacchus nicely takes into account both formal and informal aspects of the legislative process.

He traces the development of the legislation from the passage of the Foreign Service Act of 1946 to the implementation of the Foreign Service Act of 1980 in 1981. Bacchus begins by presenting a chronology of the Foreign Service Act of 1980 and then lucidly elaborates on the difficulties of achieving an "appropriate personnel 'structure' for the conduct of the nation's foreign affairs" (p. 6). For example, how does one utilize civil service personnel overseas who have specialized skills needed for the conduct of foreign policy

without slighting foreign service personnel or incurring the wrath of unions such as the American Foreign Service Association and the American Federation of Government Employees, and how does one classify individuals whose work influences foreign policy, but who never work outside of the United States?

Bacchus presents major elements of the personnel plan that began to emerge from within the State Department in 1979. There was an effort to complement the Civil Service Reform Act of 1978 and to emulate its successful passage. Curiously, although President Carter was a strong advocate of the Civil Service Reform Act, he was indifferent about this legislation. It would have been useful if Bacchus had discussed in more detail why this was the case. Key components of the plan included a reduction in the number of Foreign Service categories, the exclusion of domestic-only employees from the Foreign Service system, and the creation of a new Senior Foreign Service.

The book is laid out in a systematic fashion. Chapter titles include: "Hearings," "Developing a Bill," "The Floor," "Conference," and "Enactment and Implementation." Although Bacchus does examine formal procedures in explicit detail as implied by the foregoing, his writing style is not dry and he highlights idiosyncratic factors that can influence the development of legislation, such as the ambivalent stance of Senator Claiborne Pell toward the bill and the dispute surrounding provisions protecting the interests of divorced spouses of foreign service personnel. Pell, a former foreign service officer and an advocate of reform of the foreign service personnel system, was opposed to performance pay and overtime compensation for foreign service officers. He deemed such payment demeaning because the assumption should be made that such

elite individuals always perform at peak capacity. Attitudes of congressional negotiators toward the interests of the divorced spouses of foreign service officers were primarily determined by the friendliness or acrimony of committee members' own divorces.

In conclusion, *Inside the Legislative Process* is well executed and worth reading.

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Religion and Politics in the South: Mass and Elite Perspectives. Edited by Tod A. Baker, Robert P. Steed, and Lawrence Moreland. (New York: Praeger Publishers, 1983. Pp. xiv + 191. \$23.95.)

Religion and Politics in the South is the third volume on Southern politics edited by Baker, Steed, and Moreland, and all three volumes are based on papers presented at the Citadel Conferences on Southern Politics. Unfortunately, this collection does not compare favorably to the two previous books in this series, *Party Politics in the South* (Praeger Publishers, 1980) and *Contemporary Southern Political Attitudes and Behavior* (Praeger Publishers, 1982). Although the first two volumes in this series provided a wealth of new data and insights into southern political parties and political behavior, *Religion and Politics in the South* tells us little that is new.

The book is divided into two sections, with the first containing four chapters concerning mass perspectives on religion and politics and the second, four chapters on elite perspectives. One problem concerns the blurred distinctions between southern masses and elites. Although elites in several cases are defined as party activists, many such activists obviously do not possess the economic, educational, and other characteristics frequently associated with the concept of elites.

In the first chapter, Michael Mezey tries to distinguish the attitudinal issues that distinguish southerners from persons who live in the rest of the nation. Mezey finds that racial issues most clearly delineate the South from the rest of the nation, but he finds less distinctiveness on moral and religious issues. Rural, white southerners are found to be the most distinct group, whereas urban, white southerners are most like the rest of the nation. Two other essays in the section of the book on mass perspectives deal with southern evangelicals. Unfortunately, the authors use slightly different measures in defining evangelicals. Corwin Smidt concludes that southern evangelicals are not the monolithic group that they are

usually portrayed as. Less than one-third of southerners were classified as evangelicals, and only 30% of these classified themselves as conservatives. Jerry Perkins, Donald Fairchild, and Murray Havens compare evangelicalism among southern whites and blacks. Besides finding blacks much more likely to be evangelicals than whites (43% versus 25%), they find that race and not religion remains the dominant factor for blacks in the political realm.

Two of the four essays on elite perspectives are by the book's editors. In the first, they examine fundamentalism among party activists in two southern states and five not in the south, and in the second, they discuss political attitudes of party activists in Virginia and South Carolina. Steed and his colleagues discover no difference between party activists who are or are not southern in terms of social characteristics, but southern activists who are fundamentalists are more likely to be Republicans and conservatives. Among southern party activists, Democrats are more likely to take a "nonsouthern" issue position on military and minority rights issues, whereas Republicans more closely resemble their southern constituents.

The major finding from the essays is that evangelicals are not a monolithic group and that their influence on southern politics has been greatly exaggerated. Unfortunately, most of the findings are neither new nor noteworthy. I found myself concurring with one of the authors, Michael Mezey, who observes that his conclusions "will be obvious to those who have studied the South more persistently than I have" (p. 21).

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Sectionalism and American Political Development, 1880-1980. By Richard Franklin Bensel. (Madison: University of Wisconsin Press, 1984. Pp. xx + 494. \$35.00.)

This book swims against a growing tide of literature that describes the United States as increasingly homogeneous politically, or nationalized. As Bensel notes, sectionalism is generally described as a "fading anachronism" (p. xix). Focusing on roll call voting in the House of Representatives from 1880 to 1980, Bensel documents the history of sectionalism and its impact on political development. His conclusion is that sectionalism endures in American politics and constitutes a fundamental force in structuring the party system, political ideologies, and the norms of political institutions.

One of the strengths of this impressive book is its thorough theoretical grounding. The concept

of sectionalism is based on Immanuel Wallerstein's core-periphery distinction and then placed within a model of political development and change. Importantly, Bensel recognizes that sectional boundaries will change over time, and hence avoids the simplistic approach of dividing the nation into two fixed regions—the old slave states and the rest of the country. The empirical criteria used to establish the units of analysis are valid and display substantial creativity.

The bulk of the book traces the roll call voting behavior of trade areas (the units composing the core and periphery) over the last century to establish the outlines of political development as structured by sectional conflict. This effort has its strengths and weaknesses. The votes analyzed are selected on the basis of Bensel's measure of sectional stress which indicates trade area cohesion and conflict. Some political historians will doubt the representativeness of these votes for certain periods. The votes are then placed into a number of general policy categories. These categories serve as the basis for understanding the major political conflicts of the particular period. At this point, the skeptical reader may well cry circularity. That is, votes which manifest high levels of sectional stress are used to document the importance of sectionalism. To this end, a more systematic comparative approach (e.g., party versus sectional conflict) would have been helpful. If, however, Bensel's aim is more modest—to simply show the role of sectionalism in influencing politics—then this criticism misses the mark. These chapters provide a very well-researched and well-reasoned account of the way in which sectional conflict affected the agenda and political conflict in the House from 1880 to 1980. One of the strongest points of these chapters is the analysis of the changing nature of sectional conflict and how these changes altered the political agenda of the House and the structure of conflict.

Even in this regard, however, some problems remain with the book. The most important shortcoming is occasional reductionism, that is, all political conflict is explained by the divergent economic interests of the core and periphery. For example, racial and ethnic differences are considered as secondary or even ignored at some points, even though numerous analyses find these factors to be crucial during the late nineteenth and early twentieth centuries. However, Bensel does occasionally examine the *exceptions* to his predictions in order to produce a fuller explanation of political conflict.

These criticisms should not obscure the value of this study. Its theoretical grounding, thorough research, and focus on change result in an invaluable study on a topic that has received little thoughtful research. In particular, the historical

treatment employed by Bensel allows him to trace the dynamic processes in his model and thus better develop plausible explanations of political development. The two chapters that attempt to draw the links between sectional conflict, institutional norms, the party system, and political ideology to explain political development are more suggestive than definitive, but they raise interesting and important questions for further research. This book is a very important contribution to the literature on political development and change.

PATRICK A. PIERCE

Saint Mary's College

History of the Supreme Court of the United States: Vol. 9, The Judiciary and Responsible Government, 1910-21. By Alexander M. Bickel and Benno C. Schmidt, Jr. (New York: Macmillan Publishing, 1984. Pp. xiv + 1041. \$75.00.)

This magnificent volume, the most recent installment of the Oliver Wendell Holmes Devise History of the Supreme Court, constitutes a brilliant and comprehensive account of the inner workings, politics, personalities, and policies of the Court during the chief justiceship of Edward Douglass White. No one who has studied this often-neglected era has approached the erudition or mastery displayed in this book, and no one is likely to do so in the near future. Based on archival searches all around the nation for letters, documents, opinions, and briefs—not to mention painstaking exegesis of the *United States Reports*—the research that Bickel and Schmidt have poured into this work is truly impressive. In the larger scheme of things, the books of the Holmes Devise provide an important and needed benchmark for scholars of the Court—a fine beginning of what we can hope will be a careful examination of the development of the Supreme Court as a political institution.

Bickel died in 1974, leaving seven essentially finished chapters, and his former student, Schmidt, then took over the chore of research for and writing of the final three. I cast no aspersions on Schmidt when I say that I found Bickel's chapters, especially 1 ("Mr. Taft Rehabilitates the Court") and 4 ("Appointment Cycles"), particularly graceful and compelling. For students of courts as small groups, Bickel retells the familiar story of President and later Chief Justice Taft's unparalleled, compulsive interest and role in nominations and appointments to the federal courts. Of course we have known the general outlines of Mr. Taft's exploits for many years, but

Bickel has unearthed a great deal of fascinating and often new material, much of which he published in the *Yale Law Journal* a decade ago. These days few law professors or political scientists can construct a lively and fluent narrative. Yet Bickel shows a fine flair for storytelling at many junctures in this book.

For those who like myself normally focus on the constitutional docket of the Court, chapters 2 ("The Rule of Reason") and 7 ("Federal Administration and the Federal Specialties") provide a refreshing and painlessly educational change of pace. In the former, Bickel leads us through a detailed analysis of the vagaries of the law and politics of antitrust before, during, and after the White Court. In the latter, he takes brief but helpful forays into such topics as the Interstate Commerce Commission, the Federal Trade Commission, public lands and water rights, Indian affairs, patents and copyrights, and bankruptcy. Although the Court by the 1930s had become primarily a constitutional court, Bickel reminds us that during White's years in the middle chair, "the federal specialties still loomed quite large, and the Supreme Court had by no means ceased to be a common law court" (p. 650).

In Part 2, which includes chapters on Jim Crow, the Peonage Cases, and black voting rights, Schmidt quite persuasively argues that the "Supreme Court's race relations decisions between 1910 . . . and 1921 . . . constitute one of the Progressive era's most notable, and in some ways surprising, constitutional developments. Each of the Civil War amendments was given unprecedented application. . . . The fact of the matter is that the White Court breathed life into Reconstruction principles that had been left for dead by the Waite and Fuller Courts for three decades" (pp. 725, 727). In these decisions, the White Court sowed many seeds, the fruit of which Earl Warren and his brethren reaped nearly 40 years later.

At the risk of seeming overly defensive of our own discipline's *bona fides*, I do think that Bickel and Schmidt could have benefitted, especially in chapters 1 and 4, from some exposure to the better work of political scientists. That the text should reflect no contact with Walter F. Murphy's *Elements of Judicial Strategy* (University of Chicago Press, 1964) and David J. Danelski's work on the chief justiceship in coverage of internal politics, or with Henry J. Abraham's *Justices and Presidents* (Oxford University Press, 1974) and Danelski's *A Supreme Court Justice Is Appointed* (Random House, 1964) on the politics of appointments, simply mystifies me. Similarly, I suspect that our colleagues in history will cast a jaundiced eye on the failure of Bickel and Schmidt to take account of most of their previous efforts. For the most part, Bickel and Schmidt use

historical scholarship to provide authentication for this or that fact or date rather than to place their researches in the broader context of historiography on the Supreme Court and American society. We read chapter and after chapter and find no attempts to create even the most rudimentary generalizations about the roles of the Court and the justices. Instead of interpreting these rich sources in the light of social science or of assigning historical meaning to the White Court and its decisions during the Progressive Era, Bickel and Schmidt have built a record, a veritable wealth of materials for other scholars. For even that much, we should give thanks.

GREGORY A. CALDEIRA

The University of Iowa

Protest Is Not Enough: The Struggle of Blacks and Hispanics for Equality in Urban Politics. By Rufus P. Browning, Dale Rogers Marshall, and David H. Tabb. (Berkeley: University of California Press, 1984. Pp. xvi + 317. \$27.50.)

Protest Is Not Enough is an important contribution to the study of urban and minority group politics because it explains the relationships among minority protest politics, mobilization efforts for local public office, incorporation into the dominant liberal governing coalition, and the policy responsiveness of cities. Because of the nature of benchmarks used to measure policy responsiveness and the reliance primarily on factors internal to the cities, the book is less successful in demonstrating that, where minority incorporation has occurred, minorities have done significantly better than nonminority residents of these cities in policy responsiveness, or that the policy gains compared to those in cities where minority incorporation has not taken place are significant enough to warrant the praises of democratic openness the authors make of cities where minority incorporation has occurred.

In the first half of the book the authors do an excellent job of describing levels of black and Hispanic incorporation and explaining reasons for the levels of minority incorporation found between 1960 and 1980 in 10 northern California, nonpartisan, reform cities. Incorporation is defined as the extent to which blacks or Hispanics or both are represented in coalitions that dominate city policymaking when they decide minority related issues. They find that by 1978, minority incorporation had occurred in the following cities listed in declining order of degree: Berkeley, Oakland, San Francisco, Richmond, and Sacramento. Stockton and San Jose had a few blacks and Hispanics on city council. They had limited

influence because they were not part of a dominant government coalition. In Hayward, Daly City, and Vallejo, minorities were without significant influence in local politics.

Important findings include the following: minority protest and demand politics have no long-term negative effects on minority incorporation; minority incorporation was caused by white liberal support (as measured by voting on Proposition 14) and minority group efforts to gain public office, rather than size of black population in a city alone, the national civil rights movement, or relative deprivation of blacks compared whites in a city. Another important contribution is the complex, and at times brilliant, comparison of levels of incorporation and causation between blacks and Hispanics. Generally, blacks have been far more successful politically than Hispanics and have helped clear the way for Hispanic influence in several of the cities.

In the second half of the book, which focuses on the policy responsiveness of cities, the authors conclude that levels of incorporation, internal city factors, are significantly related to a city's policy responsiveness to minorities.

This book makes a contribution to our knowledge of federalism or intergovernmental relations. The authors conclude that the long-term effects of the federal government programs of the 1960s aided minority incorporation by providing resources to minority groups and white liberal supporters and issues around which minorities could be mobilized. Federal government programs of the 1970s overall resulted in less aid pinpointed to minorities. However, in the black-incorporation cities in the 1970s there was less of a decline in redistribution to minorities than in cities in which minority incorporation did not take place. Finally the authors find that bureaucratic implementation did not have an independent effect separate from the nature of the dominant governing coalition on a city's policy responsiveness to minorities.

The major problem with *Protest Is Not Enough* is the choice of standards of evaluation. Movement in the direction of increasing minority access to government, a process standard, is used as a benchmark along with policy responsiveness, differences among the 10 cities in support of civilian review boards, black hiring, the 1960s Model Cities, poverty, and urban renewal programs and responsiveness to federal programs in the 1970s. These benchmarks result in the impression that cities with black incorporation did far more for minorities, redistributively, than cities with no incorporation, and that local politics are important to the life chances of minorities.

These benchmarks lead to an overstatement of the positive effects of incorporation. Would it not

be more valid to measure the result of incorporation on policies important to *both* minority and nonminority citizens? I wonder what the authors might have found if they investigated whether incorporation significantly changed policies central to the benefit-tax ratios of citizens, minorities and nonminorities, that is, redistributive and developmental policies such as tax policy or local economic support of physical development policies. These benchmarks, used along with those on process norms and specific policies discussed in this study, would more effectively demonstrate the independent effect and significance of local political context on urban policy, an important objective of this book. It also would add to our knowledge of the degree to which external economic versus internal and external factors affect the policy responsiveness of our cities to minorities, a major concern of modern scholarly inquiry.

RONALD KAHN

Oberlin College

Implementation of Civil Rights Policy. Edited by Charles S. Bullock, III and Charles M. Lamb. (Monterey, Calif.: Brooks/Cole Publishing, 1984. Pp. x + 223. \$14.25, paper.)

Relying on existing literature on policy implementation, Bullock and Lamb identify 10 factors that may play a role in effectively carrying out civil rights policy.

Implementation might be aided, the authors suggest, if (1) policy goals are clearly stated, (2) precise standards are specified for measuring compliance, (3) a mechanism for monitoring compliance exists, (4) an implementation agency is established, (5) enforcement personnel are committed to civil rights, (6) superiors support enforcement personnel, (7) the beneficiaries of civil rights policy are organized and cohesively support implementation, (8) enforcement agencies coordinate their efforts, (9) cost/benefit ratios favor compliance, and (10) the federal government is active on behalf of civil rights.

Five essays examine seven areas of civil rights policy. From these, Bullock concludes that only federal involvement, specific compliance standards, commitment by agency personnel, support from superior officers, and favorable cost/benefit ratios are important for the implementation of civil rights policies. The clarity of policy goals adds somewhat to implementation, and the other factors are unimportant.

The implementation of voting rights has been largely successful, argue James Schur and Richard Button. Federal government involvement occurs under the Fifteenth Amendment, and especially

the Voting Rights Act of 1965. The act provides specific standards for compliance and clarifies policy goals. Monitoring is relatively straightforward, measured by the extent of registration and voting by blacks. There has usually been enforcement by the Justice Department and strong support by the president, Congress, and courts.

Desegregation in higher education, according to Q. Whitfield Ayres, has not been effectively implemented. No federal policy specifically applies to higher education; colleges fall under other, broader policies. The Office of Civil Rights sometimes attempted enforcement, but other leaders have shown little interest. Blacks are torn between strengthening historically black institutions and desegregating formerly segregated white colleges. Student enrollment choices and the differing academic programs of institutions muddy the measures of desegregation in higher education.

Other essays show relatively successful civil rights implementation in elementary and secondary education (Charles Bullock), moderate success in fair employment practices (Harrell Rodgers, Jr.) and bilingual education (Bullock), and little success in housing desegregation (Lamb) or in combatting "second-generation discrimination" (various authors).

The authors candidly acknowledge analytical problems. Are the tested variables inclusive? What is the ordering of importance of these implementation variables? Do they work more strongly in certain combinations than separately? Are some variables surrogates for others? For example, is support for enforcement by elected officials actually a reflection of public attitudes toward enforcement?

Notwithstanding these reservations, this volume is generally useful. First, the authors identify variables that might be related to policy assessment and test them in specific policy areas. Second, each chapter is a helpful essay on a specific aspect of racial discrimination and civil rights policy in the United States.

DAVID ADAMANY

Wayne State University

The Reapportionment Puzzle. By Bruce Cain. (Berkeley: University of California Press, 1984. Pp. xii + 202. \$24.50.)

Bruce Cain argues that reapportionment is an inherently political process and that reapportionment done by legislatures is compatible with pluralist foundations of American government. He challenges the aim of good government reformers for a nonpartisan, nonconflictual reap-

portionment process. According to Cain, good government fair plans include criteria such as equal population, compactness, contiguousness, respect for city and county lines, observing communities of interest, avoiding racial gerrymanders, and creating competitive districts. But as Cain shows, not all reasonable people agree on the value of these criteria, which makes them political; conflict exists among them, and they cannot be ordered when there is conflict. Especially noteworthy is that even the aesthetic consideration of compactness is not necessarily always in the public interest, particularly when it clashes with affirmative action concerns.

Part 1 of the book is devoted to the centrality of politics and representation to reapportionment. Part 2 is a case study of the 1981 redistricting process in the California legislature, where Cain served as a reapportionment consultant to the Assembly Elections and Reapportionment Committee. His work consists of a solid, logical line of argument. He models the process at each point and presents real-world examples to support his case.

Although reapportionment may be inherently political, the implementation of partisan and bipartisan gerrymanders is more difficult than their critics assume according to Cain. Tension especially exists between individual incumbents and party leadership in pursuit of a partisan gerrymander because of divergent incentives. Incumbents seek a risk-averse strategy involving the maximization of the safety and familiarity of their districts. Party leadership seeks efficiency. This means lessening the number of wasted votes that may involve making previously safe seats riskier, thus producing a clash of individual and party incentives. Cain views incumbents' perspectives on the reapportionment process as a reversal of the Downsian model. Rather than voters choosing candidates closest to their own interests, candidates "compare voters and choose the ones closest to them, subject to a strong displacement constraint" (p. 117).

For the majority party to undertake a partisan gerrymander it must consider whether it has the votes in its legislative caucus to pass a reapportionment bill without minority party support and has the resources to fight in the more efficient seats it would be creating, and it must take into account its estimate of long-range political and demographic trends.

Bipartisan gerrymanders reduce the number of efficient seats in both parties, protecting as many incumbents as possible in doing so. Such redistricting is undertaken when conditions exist opposite to those favoring a partisan plan. The majority party does not have sufficient votes for the adoption of a partisan plan, party resources

are nearly equal, and uncertain political and demographic trends exist.

Values important to Cain—affirmative action, political representation, and maintaining continuity or the status quo—make the legislature the appropriate implementor of redistricting. Large legislative size provides descriptive representation, its makeup is politically proportionate, and legislators' exercise of conflicts of interests promote caution and resistance to change. But Cain also makes suggestions for improvement of the process by making it more public, rational, and timely.

Cain has done an excellent job of weaving together theoretical concerns and description of one reapportionment process. His treatment of the clashes involved in the process is well argued and provides a strong challenge to good government reformers. He has made a thorough contribution to the reapportionment literature.

BARBARA BURRELL

State University of New York at Albany

The State and the Poor in the 1980s. Edited by Manuel Carballo and Mary Jo Bane. (Boston: Auburn House Publishing, 1984. Pp. xxiv + 328. \$24.95, cloth; \$16.00, paper.)

Recent reductions in federal spending on social welfare programs have resulted in new demands placed on state governments to respond to the needs of the poor. Manuel Carballo and Mary Jo Bane's edited volume, *The State and the Poor in the 1980s*, is therefore a very timely book. The state in this case is Massachusetts, and the "poor" are those who live in households with incomes below the poverty line, as determined by the federal Office of Management and Budget. The various chapters, most of which have been written by scholars affiliated with the John F. Kennedy School of Government at Harvard University, contain careful yet critical evaluations of that state's response to the conditions and needs of its poor residents.

Although the contributors are critical, even at times sharply critical, potential readers should be aware of what this book is not. It is not a negative commentary on American society generally, nor on our economic and political systems more specifically. The contributors propose no radical restructuring of institutions, nor any dramatic redistribution of wealth. Indeed, Carballo states in the Introduction that the volume does not address the *causes* of poverty, except to note that they are multiple, and therefore the government's response must be multidimensional as well. Neither is the book an attack on the Reagan administration. Although I strongly suspect that

most of the contributors would argue with Ronald Reagan over who should be considered "truly needy," the book is not used as a medium through which to express disapprobation. Rather, fiscal retrenchment at the federal level is accepted as a fact of political life, and a more sensitive and effective state-level response to the poor becomes the focus of concern. Finally, this book is in no way a progeny of the state policy output literature within political science. No effort is made to explain the programmatic choices made in Massachusetts, or to account for the different levels of funding across various programs. To the contrary, state policy output studies by political scientists have been completely ignored in this volume (probably with minimal, if any, harm).

Each chapter of the volume addresses a specific dimension of Massachusetts's programmatic commitment toward the poor. Areas covered include education, health care, job training, income assistance, housing, transportation, and crime. Additional chapters evaluate the taxation policies of the state (specifically the progressive and regressive nature of the state's different revenue-raising measures), and the role that centralized planning plays (or more accurately, does not play) in the state's efforts to respond to the needs of the poor. In each chapter existing programs are described, their impacts assessed, and recommendations for programmatic changes are offered. Although a reader who does not share the contributors' particular interest in Massachusetts may find the state-specific details rather tedious at times, almost all of the chapters report a wealth of informative data within distinctive yet heuristically useful conceptual frameworks. Among the best are the chapters on health care by David Blumenthal and David Calkins, income transfers by Bane, and transportation by Jose Gomez-Ibanez (who argues quite persuasively that subsidizing automobile ownership among the poor would be a more effective and efficient response to their transportation needs than the state's present subsidy for mass transit).

The authors are all appropriately cautious about generalizing beyond Massachusetts, recognizing that the poor in Massachusetts, compared to many other states, are disproportionately white (more than 80%; racial discrimination, as a consequence, receives major attention only in John Yinger's chapter on housing policies), and also disproportionately residents of metropolitan areas (close to 90%). Therefore, their recommendations, like their data, tend to be state-specific. But several phenomena that have been increasingly important across states are highlighted in this volume and should be of interest to a wide audience. One is the so-called "tax revolt" and the impact that it has had on programs assisting the

poor. In 1980, Massachusetts's voters adopted Proposition 2½, which mandated local governments to limit property taxes to no more than 2.5% of property values. The consequences that this has had on services benefiting the poor, and on the redirection of attention toward the state as an alternative source of support for these services, are impressively demonstrated by Helen Ladd and Herman Leonard in their chapter on taxation.

Another topic of growing concern is the increasing "feminization of poverty." Bane, in an initial chapter on the demographics of Massachusetts's poverty problem, reports that by 1980 almost one-half of the poor in Massachusetts were members of female-headed households. In a later chapter on income security programs, she argues convincingly that it is this group among the poor that is least likely to be lifted out of poverty by current state and federal programs. Indeed, among the many needed policy changes identified in this volume, policy innovations in response to this group appear to be the most immediate challenge confronting all levels of government in this country.

The State and the Poor in the 1980s will not be exciting reading for very many, but few should fail to be enlightened by it. The various chapters are only minimally integrated, so students of specific policy areas can benefit from reading selectively among the contributions.

RICHARD L. ENGSTROM

University of New Orleans

The Spot: The Rise of Political Advertising on Television. By Edwin Diamond and Stephen Bates. (Cambridge, Mass.: MIT Press, 1984. Pp. xiv + 416. \$17.50.)

Those worried that political advertising on television is devouring democracy can relax, but not too much. After reading this book we learn that our fears are neither entirely imaginary nor real. The authors—one a distinguished media-watcher for many years, the other a relative newcomer—alarm us with disturbing illustrations, but reassure us with reports of contrary evidence and findings.

Generally the bad news is followed by the good news.

At various points Diamond and Bates disturb our democratic hopes. We learn that national presidential and megastate elections increasingly are dominated by political commercials (or polispots) that are not only expensive, but can also transform and distort our perception of the world.

We learn that radio listeners of the first Kennedy-Nixon debate thought Nixon had won, whereas television viewers awarded victory to the

more photogenic Kennedy. We learn that their campaign advisers told Dewey in 1948 and Humphrey in 1968 that additional advertising in crucial states probably would have changed the outcome of those elections. We learn that a rich but unknown New York Republican seized the Republican-Conservative nomination for governor with the help of almost \$10 million largely spent on aggressive polispots.

Diamond and Bates, however, inform us that candidates fail whose behavior departs from their television image, that polispots tend to reinforce attitudes rather than convert, that incumbency and voter partisanship explain more election results than political advertising, that advertising spreads candidates' messages widely and efficiently without manipulating, that free media is more important than paid media, and that, anyway, the media wizards themselves are uncertain about what works and what doesn't.

However, the authors offer only limited hope, for they point to the development of political advertising as a menace of considerable proportions. Politics may become a kind of spectator sport with voting "just one more activity being commended to us by television purveyors of goods and services" (p. 385). But Diamond and Bates spend virtually no time exploring this situation despite labeling it as "probably more serious" than the more publicized fears of media mercenaries selling candidates like bars of soap (p. xii).

Diamond and Bates are at their best in Parts 3 and 4 of their book, where they analyze media techniques and the effects of polispots. Their evaluation is precise, amusing, enlightening, but incomplete (the bad news and the good news are not sufficiently related to each other). Parts 1 and 2 involve a brief description of the Glenn campaign for the 1984 presidential nomination and a long description of the evolution of political advertising that includes much that is fascinating, but also so much detail that some readers may long for an edited version. Until it appears, such readers may wish to consult the authors' "The Political Pitch" (*Psychology Today*, November, 1984, 18, 22-32).

This book is the best available study of political advertising on television. That's the good news. The bad news is that it easily could have been much better.

GENE WEINSTEIN

San Francisco State University

American Political Theology: Historical Perspective and Theoretical Analysis. Edited by Charles W. Dunn. (New York: Praeger Publishers, 1984. Pp. iii + 195. \$12.95, paper.)

Dunn boldly sets out to consider the ways in which conflicting theological perspectives in the past and present have influenced politics in the United States. He focuses on three periods of past, the founding era, the decades around the Civil War, and the age of the New Deal, and then in a much longer section, on our own age. His basic thesis is that most of the divisions in American religion as it has affected our politics have revolved around the enduring conflict between what he terms liberal and conservative theologies. Dunn feels that what each has stood for has varied throughout the rich history of Christianity in America. Indeed, Dunn's sensitivity to changing and evolving theological perspectives in American Christianity is a welcome positive feature of his analysis. He argues that what has not changed, however, is the tension between theological conservatives and liberals, and he makes a good case that this division is at least one angle from which we might view conflict in the major religions in the United States. In the process he constructs an interesting analysis.

Dunn's major goal, of course, is to go on to show that this division has consistently had serious implications for public policy. Today we know this is no easy task, as serious students of American politics find themselves forced to confront theologies as diverse as the multitude of religious groups involved in American politics who espouse one or another theme. It is obviously even more challenging a task for eras in our past. The distance from a group's religious ideals to its political practice, much less to what role its action plays in the formulation of public policy, is enormous. Dunn knows this very well, and sometimes he has more evidence for theological impact than at other times. But his major effort is toward suggesting a framework for the long-run influence of clashing theologies on our politics, and in this he succeeds surprisingly well in so brief a work.

Dunn supplements his theoretical and analytic work with a generous selection of relevant documents and writings from American constitutional, political, and religious history. This makes his book doubly valuable, because it offers both a framework for analysis and plunges one into the primary sources that are essential to his argument. His selections show a great deal of care, insight, and balance. In the contemporary period, for example, he ranges from the *Humanist Manifesto* to Jerry Falwell, from the radical black theologian, James Cone, to Roman Catholic theology on the bomb among other perspectives.

For the contemporary era, as well as for the others, any scholar would have a personal list of crucial documents rather than that which Dunn presents. But I suspect that such a list would be small, because Dunn has chosen well. Some readers may be ambivalent about the frame of reference used in making the selections, one that coincides with Dunn's conservative/liberal theological division, but that is only to be expected, and his net is attractively wide. There is no doubt that the selections from primary sources are a great strength of *American Political Theology*.

In *American Political Theology*, Dunn emerges as one of the stimulating pioneers of the rapidly developing subfield of religion and American politics. His work deserves a close reading.

ROBERT BOOTH FOWLER

University of Wisconsin-Madison

Relief or Reform? Reagan's Regulatory Dilemma.

By George C. Eads and Michael Fix. (Washington, D.C.: Urban Institute Press, 1984. Pp. ix + 285. \$19.95, cloth; \$12.95, paper.)

In their compelling work *Relief or Reform? Reagan's Regulatory Dilemma*, George C. Eads and Michael Fix offer a detailed empirical analysis of the Reagan administration's regulatory relief promise and the factors affecting its ultimate decision to pursue more limited regulatory reform. The book provides insight into the Reagan regulatory strategies and offers a lengthy critical assessment of why the Reagan regulatory relief effort achieved few of its original goals. This analysis reveals that the president cannot merely rely on administrative changes through politically motivated appointments, budgetary cutbacks of regulatory agencies, agency reorganizations, limited enforcement of existing statutes, and the transfer of regulatory responsibilities to states and localities. If lasting regulatory change is to be implemented, the president needs the substantive legislative support of Congress.

The historical context for the Reagan administration's regulatory approach is established early in the book. Eads and Fix provide an overview of the Ford and Carter regulatory strategies and contend that these administrations, with the help of Congress, offered impressive economic regulatory reductions in the airlines and the trucking, railroad, and banking industries. Yet both administrations failed to achieve similar substantive legislative success in the social regulatory arena.

In 1981, the Reagan administration offered

sweeping regulatory reforms and received widespread support from business interests, economists, and libertarians who agreed that excess economic and social regulation had threatened government efficiency and economic vitality. Yet despite this apparent support, the Reagan regulatory relief effort failed to forge the necessary congressional coalition that would make regulatory relief a reality. Instead, social regulation under the Reagan administration became a political albatross with the appointments of controversial pro-industry administrators James Watt and Anne Gorsuch Burford. Fix and Eads argue persuasively that these blatantly political appointments, coupled with the administration's inability to perceive the importance of securing legislative reform, ultimately stymied the Reagan regulatory relief efforts.

In the absence of congressional approval, the Reagan administration attempted to reform existing regulations through the administrative process. Opponents of the Reagan efforts appealed to the judicial system, and the Supreme Court handed the Reagan administration several key setbacks. Eads and Fix detail the proposed rule changes and court challenges in several cases: the National Highway Traffic Safety Administration's decision to rescind the passive-restraint standard; the Department of the Treasury's decision to rescind liquor labeling standards; the EPA's attempt to alter the Clean Air Act's definition of "stationary source"; and the Department of Labor's decision to alter the Davis-Bacon regulations.

Although the authors are largely critical of the Reagan administration's regulatory strategy, they contend that Reagan accomplished meaningful legislative changes in intergovernmental relations and grant reform. The Reagan initiatives fostered state and local government discretion, justified reductions in federal grants, and legitimated rules changes as a part of the broader regulatory relief effort.

An excellent concluding chapter assesses the Reagan regulatory relief program critically. Eads and Fix suggest that the administration did achieve some economic success in the regulatory arena, but they are quick to point out that this success is not nearly as sweeping as Reagan had intended. The administration's inability to consider the important role played by Congress, the media, and the Supreme Court in the regulatory arena proved to thwart meaningful relief efforts. This well-researched study reveals that our checks and balances system will impede a president who attempts to bypass Congress and ignores the Court's key power of judicial review. Given these constraints built into the American policymaking process, it is not surprising that President Reagan

was forced to settle for more limited regulatory reform.

CRAIG A. RIMMERMAN

College of Charleston

Western Public Lands: The Management of Natural Resources in a Time of Declining Federalism. Edited by John G. Francis and Richard Ganzel. (Totowa, N.J.: Rowman & Allanheld, Publishers, 1984. Pp. 312. \$39.50.)

This edited collection of 14 selections nicely focuses on the rich diversity of policy issues involved in natural resources in the American West. The recent past, the present, and some future prospects are observed and analyzed. Some prominent themes of changing federalism, roles of national and state governments, and the perspectives of private and public interests are woven together into the cloth of natural resources management. The frequent clashes between these themes and actors are an issue that runs through the essays.

This monograph is neatly divided into two sections. Part 1 examines "The Public Lands" including such elements as the Sagebrush Rebellion, privatization, the Bureau of Land Management, and state interests in federal and Indian lands. Part 2 then addresses "Selected Natural Resources Issues" including federalism and nuclear wastes, state models of water management, and regulatory management of multistate energy projects. Francis and Ganzel supply a good introduction to set the stage for the essays in *Western Public Lands*.

Refreshingly free of jargon or overspecialized analytical approaches, these essays offer an excellent overview of significant issues confronting western policymakers grappling with the competing facets of public lands and natural resources choices. Although not particularly written for the general reader in political science, *Western Public Lands* is a rich source of background and ideas for policymakers and scholars more specifically in this area. Much of this work would be suitable as readings for students of political science, public administration, and policy studies at the graduate and upper-division undergraduate levels for courses on natural resources and perhaps courses on intergovernmental relations or decision making.

For example, the chapter by Henry P. Caulfield, Jr. on U.S. water resources development policy and intergovernmental relations is an example of what the book accomplishes. Using a historical framework, Caulfield highlights public policy of the last century and then leads it to the

present (1977 to 1983). He delves into such themes as federalism, the ebb and flow of national government dominance, and political policymaking. The essay then probes the future utilizing several contrasting scenarios to provoke thought on where this subject may be leading.

The contributions to *Western Public Lands* are logically sequenced, in most respects well organized, and supported with copious notes and sources. Two minor objections apply: Most of these contributors are well known in their own specializations, but a brief listing of author's field and current affiliation would be beneficial for some readers. A more severe difficulty with some of the essays is their dated quality. Although most of the research and findings are relatively timely, I find a slight tendency for a timewarp from the late 1970s to the early 1980s. The Sagebrush Rebellion, once so well known, seems now almost to have slipped from sight.

In an arena where action evolves so dynamically, this problem is not wholly solvable. However, it is challenging and rewarding whenever possible to emphasize where we may be going from the base of the past and present into the partly uncharted terrain of the next decade and generation. To political scientists and others involved with the future of western lands and natural resources, *Western Public Lands* is an essential source of information and issues.

DANIEL SLOAN

University of Colorado at Boulder

Congressional Women: Their Recruitment, Treatment, and Behavior. By Irwin N. Gertzog. (New York: Praeger Publishers, 1984. Pp. viii + 291. \$29.95.)

Congressional Women is a significant contribution to the burgeoning literature on the House of Representatives. More particularly, it fills a considerable void by concentrating on the women who have taken their place in the 95% to 99% male-dominated House from 1916 to 1982.

The book is divided into five parts. The introduction and explanation of the content of each section is followed by congresswomen's recruitment patterns. Part 3 concerns women's treatment by their colleagues. Changing legislative goals and methods for attaining them is the focus of Part 4. The concluding section presents a typology of congresswomen and a discussion of their probable future. A succinct appendix contains lists of congresswomen, committee assignments,

and a bibliography.

Part 2 has a tremendously valuable empirical presentation and analysis of widows who become members of Congress. Contrary to popular belief, most congressmen's widows (84%) chose not to run for their husband's seats. Also, southern widows have a greater chance for party nomination first time around, whereas widows outside the South have had to fight party-backed opposition. By the second term, northerners were likely to be renominated, but not the southerners who were expected to serve only one caretaker term.

The backgrounds of congresswomen show a temporal progression from ascribed status of family wealth and connections to achieved law degrees and prior public service. Is this also the general longitudinal pattern of congressmen's backgrounds? One suspects that it is, and that women's recruitment follows the prerequisites of men's with regard to occupational acceptability and holding prior office.

At first ignored or patronized, congresswomen now are more likely to be treated equally by their male colleagues. Yet they have not been allowed into the informal inner circles of power. Gertzog argues that this exclusion has deleterious effects on getting congresswomen's legislation on the House agenda. Moreover, House leadership positions remain a male-only preserve.

Changing legislative goals, achievements, and strategies are traced over the 66 years women have been members of Congress. Legislative goals have evolved from reinforcement of traditional roles, to egalitarian concerns, to measures to assist women such as victims of domestic violence. The Congressional Caucus for Women's Issues has emerged as a successful instrument for achievement of current legislative objectives.

Chapter 12 discusses changing gender and legislative roles. In conclusion, Gertzog is cautiously sanguine about future increases in numbers of congresswomen. Although he has cogent arguments, it is quite possible to reason otherwise. The fault lies not in gender roles that do not vary much in the western democracies, but in the constraints of our single-member district system. European nations with multimember systems—even Ireland and Portugal, to say nothing of the Scandinavian countries—have much larger women's representation in parliament than obtains in the United States. The day when these women will approach equal numbers with men in their parliaments is in reach within their lifetimes. The evidence is otherwise for the United States.

Congressional Women is the definitive work on the subject. It would be useful as a supplementary text for introductory political science or a class on Congress. Women and politics courses will find it meritorious as a text or supplementary volume. It

also is highly recommended reading for specialists in American politics.

WILMA RULE

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Processing the News: How People Tame the Information Tide. By Doris A. Graber. (New York: Longman, 1984. Pp. xiii + 239. \$27.50, cloth; \$15.95, paper.)

No study of public opinion is without its major frustrations. The marketplace of ideas, the macro world of opinion analysis, remains as ephemeral as ever, based as it is on the need to observe and measure the activities of both producers and consumers who are largely beyond the scope of the tools at hand. Attempts to use statistical correlations to interpret past events provoke disputes about validity, methodology, and the applicability of the interpretation to other similar events.

Fundamental to these differences about the impact of opinion lies a vast ignorance about the nature of the cognitive processes through which individual opinions are formed. Micro analysis attempts to create a clinical world in which some of the external manifestations of thought processes can be examined, that is, individuals are tested on what they have learned and asked to report on how they think they have learned. Unfortunately, there are no set standards for clinical conditions. Reviewers of findings may have participated in similar studies in which an entirely different methodology was employed or, more likely, never participated in any such study. Consequently, a reviewer of the particular techniques described in a report may be tempted to quibble, argue, and nitpick at points that gained easy acceptance among those who conducted the study. Some readers may have such difficulties with Doris Graber's useful and interesting study describing her observation of a panel of participants as they absorb the cacophony produced by the media, or as she puts it, *How People Tame the Information Tide*.

What this book provides in the way of informative discussion and theorizing is more important than the faults to be found with it. Graber analyzes the impact of the media by focusing on the subjective reaction of its consumers. An analysis of the process by which the news is made, as well as the theoretical devices that are used in the process of transmission, requires that we gauge the reactions of those who receive the transmission. Content analysis in the form of categorizing

the words and symbols transmitted by the media provides a great deal of useful information about the media, but not necessarily about the media's impact.

The setting for this study was Evanston, Illinois, where 200 respondents were interviewed and categorized until a final group of 21 panelists were selected as continuing participants. They were followed through 1976 in a series of interviews in which they were asked open-ended questions about what had appeared in the media. Efforts were made to ascertain that participation in the study did not induce significant changes in respondents' behavior by comparing the observed habits with those reported in initial interviews. The media sources that the respondents used were analyzed by the interviewer so that she could compare a panelist's version with her own.

What was found was that the model of the media as agenda setter for the public does not apply to those areas where an individual is able to examine a report by analogy to his or her own experiences, beliefs, observations, or what they have previously learned from the media. One interesting example of the latter was the reaction of several panelists to reports of a 1976 ceasefire in Lebanon. "Rather than processing the story as part of a schema on problems in the Middle East, the panelists called on their knowledge of ceasefires in Northern Ireland. Since these had not worked well, the panelists reasoned that ceasefires never worked and that they would, therefore, fail in Lebanon, too" (p. 131).

Graber asserts that an individual's schema explains the processing of news, that is, that knowledge abstracted from experience processes new information and makes it possible to retrieve stored information (p. 23). Anything that doesn't fit the schema tends to be disregarded. This tends to complement the notion of opinion lag as developed in the 1950s and 60s by Robert Lane, David O. Sears, and others.

There are problems concerning the anecdotal evidence used to illustrate the book's empirical theory. For example, Graber's four categories are based on high and low interest in politics and easy and difficult access to information sources. Yet in the portraits of the panelists and in the discussions of their reports throughout the book, individuals seem to move among categories; for example, people with low access to information sources knew more than they should have if the ideal typology fit.

In the final analysis, what *Processing the News* indicates is that we need many more broadly based studies of how individuals respond to the news, a conclusion that Graber shares. In an era of sophisticated methodologies and abstract models, strong empirical studies of this sort pro-

vide evidence with the explanatory power to build a strong theory of public opinion.

MICHAEL BARUCH GROSSMAN

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The Uncertain Triumph: Federal Education Policy in the Kennedy and Johnson Years. By Hugh Davis Graham. (Chapel Hill: University of North Carolina Press, 1984. Pp. xi + 280. \$22.00.)

One of the most important legacies of Lyndon Johnson's Great Society was the creation of a major federal role in education. Federal initiatives launched during this period financed programs that stretched from preschool to graduate school. The creation of these efforts is the subject of Hugh Davis Graham's *The Uncertain Triumph: Federal Education Policy in the Kennedy and Johnson Years*.

Graham begins by recounting the inability of the Kennedy administration to enact a general aid to education law, a failure he attributes to a lack of commitment. But, just weeks after Kennedy's assassination, Congress enacted both the Vocational Educational Act and the Higher Education Facilities Act and broke the legislative logjam.

The great expansion of federal aid took place in the Johnson administration when education was a key component of the War on Poverty. Much of the education policy development was done by external task forces brought together by White House aides Bill Moyers and Joe Califano. Graham focuses on these groups and their role in generating ideas that became legislative proposals and, sometimes, legislative enactments. The most successful education task force was John Gardner's 1964 group, which articulated the priorities that were codified in the Elementary and Secondary Education Act of 1965 (ESEA). Other major education task forces of this era included Frederick Hovde's task force on education (1961), Joseph Hunt's task force on child development (1967), and William Friday's task force on education (1967). These external groups were supplemented by dozens of interagency task forces composed largely of federal officials.

Graham's detailed analysis of the task forces as an instrument of policy formulation is a welcome contribution to the literature on the Great Society. The volume illustrates both how much government has changed in the last two decades—and how little. On the one hand, some of the key issues in today's education policy debates—merit pay, whether to create a U.S. Department of Education, the quality of teacher education programs, how to improve education in urban areas

—were also debated in the 1960s. On the other hand, some of the changes are striking: The legitimacy of federal aid to schools is no longer questioned. The changes in the political process are even more noticeable. The Johnson task forces operated in secret, and there was no outside review of their proposals. Today's emphasis on open meetings and instant analysis provides a far different environment.

Although the volume is very useful, there are two important limitations. First, enactment of federal education legislation was, to a large extent, a triumph of political brokering. Long-standing controversies—aid to religious schools, aid to segregated schools, and fears about possible federal control of education—had to be settled before any federal programs could be authorized. The legislative histories of federal education aid describe these issues in some detail. However, because executive branch policy planners were not always concerned with political calculus, these issues do not assume as prominent a role in this volume as they did in reality. This will not be a problem for those familiar with the political history of federal aid to education, but it may mislead the reader who lacks such knowledge.

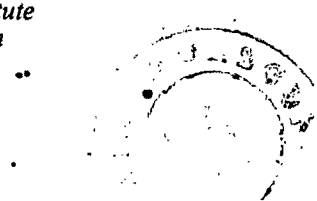
A second problem stems from the author's effort to summarize the policy impact of the federal programs. Graham argues that the programs failed to achieve their goals and were an administrative burden for state and local governments. There is considerable research to support that position, and there is no doubt that advocates of federal aid promised more than the schools could deliver. But there is just as much evidence to suggest that the federal efforts increased access to educational services, improved educational achievement, and imposed only a modest burden on state and local officials. Graham, however, devotes very little attention to the research that illustrates the more positive consequences.

Part of the difficulty facing anyone who attempts to describe the impact of federal education spending is that the topic involves a great deal of evidence, much of it contradictory. An effort to briefly summarize this evidence is bound to suffer; this is no exception.

These shortcomings are relatively minor problems. Graham's well-written and thoroughly documented book is an important addition to the literature on the creation of the federal role in education and will be a valuable source for those concerned with that issue.

TERRY W. HARTLE

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Productivity and Public Policy. Edited by Marc Holzer and Stuart S. Nagel. (Beverly Hills, Calif.: Sage Publications, 1984. Pp. 296. \$28.00, cloth; \$14.00, paper.)

American political rhetoric is as subject to fads as most other aspects of life. A current fad is an almost obsessive concern for economic productivity. This trend manifests itself in such agenda items as industrial policy, international trade, privatization initiatives, tax reform, and budget-cutting. This volume, which is the twelfth in the Sage Yearbooks in Politics and Public Policy series, offers sober and sobering assessments of the interaction between productivity and public policy.

Political discussions of productivity tend to engage in, if not be dominated by, finger-pointing. Greedy unions, lazy workers, bottom-line-oriented managers, and government regulations are the most common targets of the critics. However, this book contains 11 analyses which, while acknowledging these popular targets, examine the concept of productivity and the multitude of related public policy issues from a variety of analytic perspectives and in a wide variety of practical applications. Because of this combination of characteristics, the book is best described as a heuristic device rather than as a polemic. It maintains an intellectual suppleness and eclecticism throughout.

The 11 analyses tend to focus around three themes: the conceptualization of productivity, specific public policies and productivity, and environmental constraints on productivity-oriented policies. Ellen Rosen's opening chapter on the concept of productivity sets the framework for the remainder of the volume. She carefully examines the difficulties involved in measuring productivity within the context of a broader discussion of the qualitative and quantitative dimensions of this issue. She does an excellent job of connecting methodological concerns to substantive policy issues. The sensitive approaches to the concept of productivity, and its measurement, contained in the 10 chapters that follow, explicitly or implicitly, address the issues raised by Rosen.

Specific public policies are examined in many of the chapters. Raymond Duch examines economic subsidy programs in France, the United Kingdom, and Japan. He concludes that a highly selective subsidy strategy has the best chance of success. He also includes a fascinating discussion of the differences between new industries, mature industries, and high-growth competitive industries. Howard Leichter examines the impact of public policy strategies on national productivity by focusing on the Japanese, Western European, and British experiences. He concludes by contending,

"It may very well be that the United States must first recognize, as our Western European competitors have, that there is a positive relationship between socially supportive policies and increased productivity" (pp. 65-66). Eli Noam contributes a rigorous analysis of the public policy implications of the characteristics of the cable television industry in the United States.

The larger environmental constraints on attempts to use public policy to improve private- or public-sector productivity are examined throughout the volume. Holzer offers a particularly provocative discussion of the impact of public attitudes toward public-sector administrators. Howard Leichter identifies cultural variables that are distinctive to the United States. Daniel Mitchell analyzes the impact of macroeconomic policies and economic growth cycles on productivity performance. Finally, Rupert Chisholm contributes a description and discussion of the "sociotechnical system" (STS) approach to productivity. This is a very ambitious and comprehensive attempt to capture the major environmental forces impinging on productivity in organizations. Furthermore, it is an attempt to reconceptualize productivity in light of the movement from an industrial to a postindustrial society.

This sampling of the main themes of the book cannot do justice to the richness of its discussion of productivity and public policy. This is a volume that both documents the present heightened concern for productivity and points the way toward innovative analytic perspectives and practical strategies.

CRAIG RAMSAY

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Legislating Bureaucratic Change: The Civil Service Reform Act of 1978. Edited by Patricia W. Ingraham and Carolyn Ban. (Albany: State University of New York Press, 1984. Pp. vii + 405. \$14.95, paper.)

The editors of this volume have passed judgment on the reform that was the centerpiece of the Carter administration: The results did not match the objectives. What went wrong? Is it just the lack of resources essential to the implementation process, as most of the contributors tell us? Any understanding of the implementation problem requires a much more sophisticated model than those suggested in this volume. The literature on public policy implementation has increased since Aaron Wildavsky and Jeffrey Pressman blazed the trail. George Edwards has suggested three other controlling variables that could make the

difference: leadership, communication, and psychological disposition of implementers. Paul Sabatier and Daniel Mazmanian have added problem tractability, continuing support of the sovereign, and changing socioeconomic and political conditions to the list. Referring to these scholars' works and adapting some of their concepts would undoubtedly sharpen our focus and enrich our understanding of this important policy area. Nevertheless, Ingraham and Ban have put together an excellent piece of scholarship, authentic in originality, fresh in insights, and provocative in observations.

Incrementalists have a penchant for evaluating the impact of public policy early enough in the implementation game for purposes of midcourse correction. Through trial and error, we learn from mistakes and profit from experience. But mistakes can also be deceiving, for they sometimes send the wrong signals for remedial action. Some policies have sleeper effects, like Roosevelt's land reform program during the New Deal. The positive effect of this program was delayed until 30 years later in the form of a viable black middle class that became the back bone of the civil rights movement in the 1960s.

The establishment of the Senior Executive Service is a rejection of the Wilsonian doctrine of the politics-administration dichotomy. But scholars are now having second thoughts on the wisdom of a symbiotic relationship between the two. A senior executive in the federal public service is no longer a source of independent judgment about the substance and thrust of public policy. Before jumping "for the jellybean (bonuses)" (p. 31), Frederick C. Thayer feels that the executive must now obey the orders of a political superior, dance to the tune of a different musician, and march to the beat of a different drummer. Will the New Public Administration now stop blaming Wilson for all the urban uprisings of the late sixties because bureaucrats were neutrals in the public policy process instead of advocates?

The bull that Carter took by the horn has now become a sacrificial lamb in the Reagan corral. Gone are the hopes of motivating career administrators with bonuses and rewarding mid-career executives with merit pay. As Carolyn Ban writes (p. 54), the "hundred flowers" that were supposed to bloom from the Carter reforms got caught in the quicksand of budget cuts, policy terminations, and personal retrenchments. What a disappointment it must have been to that young man who promised in testimony in 1978 before the House Post Office and Civil Service Committee that the Senior Executive Service would be a spark plug that could turn on the best and the brightest to their highest level of performance.

What is really the philosophy behind all these

public personnel administration reforms? Is it not to democratize public administration by making bureaucracy more accountable, responsible, and responsive? If that is the end result, Frederick Mosher has raised this question: Is civil service reform the way to enshrine democracy in a bureaucracy? Would not broadening, lengthening, and humanizing the educational preparations of career administrators in the super-grades be a better way? And external and internal controls another way? If civil service reformers were less than sanguine about this aspect of public personnel management, it is because they were more than obsessed with the idea of Theory X and its transferability from the business world into the public sector. Wallace Sayre was right: Technique has again triumphed over purpose.

A. B. VILLANUEVA

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Public Policy and the Aging. By William W. Lammers. (Washington, D.C.: Congressional Quarterly Press, 1983. Pp. xv + 265. \$7.95, paper.)

Lammers, of the Andrus Gerontology Center at the University of Southern California, has written a succinct overview of American public policy toward the aging during the last two decades. The brevity and bland evenhandedness of this volume will be a strength for some potential readers and a weakness for others.

In separate chapters, the author gives a brief history of government policy toward the aged and surveys the arena within which public policy toward the aged is made. Demographic and voting behavior data on the aged are also surveyed. Much of the remaining portion of the book is given to analysis of specific policy areas that have impact on the aged. Each of these chapters, which focus on Social Security, retirement, health, and housing respectively, begins with a description of the environment and outcome of recent policy discussions followed by a discussion of what Lammers views as the chief policy problems associated with that topic.

Lammers's discussion of Social Security would have been improved by the use of more sophisticated tables. His treatment of the impact of reductions of the minimum benefit seems particularly weak, as does his treatment of the likely utilization and impact of independent retirement accounts. On the other hand, the treatment in this book of changing retirement patterns and alternative housing options for the aged is quite informative.

Perhaps the chief failing of *Public Policy and*

the Aging is its failure to acknowledge that the "problems" of aging in the United States today are essentially the product of earlier successes. Although laws preventing mandatory retirement rules preserve continued employment as an option for many elderly persons, more and more workers are retiring at a younger age, and fewer than was once the case are quitting work due to problems of health. Although Lammers does call for an increase in the eligibility age for Social Security benefits, most of his policy agenda items call for additional federal spending or regulation. But as he admits, the politically active middle- and upper-middle class retirees and the groups to which they belong are hardly likely to seek the federal programs and regulations that will be of the most benefit to the poorest and most frail among the elderly. As one alternative, perhaps a study of the causes of the successes mentioned previously might yield a more feasible policy agenda that might better meet the needs of the most deserving elderly.

GAYLE R. AVANT

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Health Politics and Policy. Edited by Theodor J. Litman and Leonard S. Robins. (New York: John Wiley & Sons, 1984. Pp. xvii + 403. \$23.95.)

This is a collection of 16 essays by practitioners and academicians in the field of health care policy. The editors who solicited and assembled these essays intended that the volume provide a "comprehensive overview of the politics of health" (p. xiii). This is a bold objective but one well worth undertaking for two reasons. First, since the enactment of Medicare 20 years ago, the federal government has been searching for ways to cope with the ever-expanding fiscal demands of that landmark health care financing program. This quest has added great complexity to the mix of health policies and to the character of health politics at the federal level, making this policy arena far more opaque than casual observers would suspect. Second, for the most part the opacity has been compounded, rather than reduced, by the social science literature on the subject, much of which is conceptually muddled and surprisingly little of which, until relatively recently, was the product of political scientists.

The contributions to this volume have been organized into five parts of unequal weight and scope. The essays in Part 1—led by Litman's

introduction that nicely summarizes some of the lessons of governmental involvement in the health care system to date—aim to provide a general overview of health politics and policy. Particularly noteworthy is a thoughtful essay by Roger Battistella and James Begun examining the influence on the health policy environment of different political and economic orientations whose influence has varied over time. Their review of what they call the normative, rationalist, neoconservative, and neo-Marxist approaches is an instructive survey of the intellectual landscape of this policy field.

The eight essays that make up parts 2 and 3 focus on the major political actors and institutional mechanics relevant to an understanding of health policymaking. Here we find discussions of (among other things) the fragmented structure of programmatic control within the executive branch, the consequences for health policy of reforms in the structure and process of congressional decision making, the problems of implementation, the role of the states in health policymaking and health services delivery, the relative success of professional provider groups in improving their monopolistic positions through legislation, and the limits and possibilities of citizen action in reforming the health care system. Although there is nothing very original in these largely descriptive (and, in some cases, pedestrian) essays, together they provide a reasonable guide to the institutional and political environment of health policymaking. What is generally lacking, however, is any insight into the ways in which policymaking structures and processes vary from one health program area to another.

The balance of the book consists of four chapters: a brief history of the evolution of public health activities in the United States by Milton Roemer; a nicely crafted overview of the politics of health care regulation by Thomas Bice; a curious flogging of this policy arena's dead horse, national health insurance, by Theodore Marmor; and a lone essay devoted to understanding health politics from a cross-national perspective by William Glaser.

The field of health policy is littered with two kinds of debris: narrow and tedious work by economists and health care administrators on the one hand, and overgeneralization by medical sociologists on the other. There is little of either in this book. That alone makes it praiseworthy. (It also contains an exceptionally fine bibliography.) Nevertheless, specialists in the field of health politics and policy will find little new information here nor much in the way of analytically distinctive slants on the subject. The primary virtue of *Health Politics and Policy* is its potential for use as a background text in health politics courses

—no small virtue in light of the persistent dearth of such books.

JOHN T. TIERNEY

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Direct Legislation: Voting on Ballot Propositions in the United States. By David B. Magleby. (Baltimore: Johns Hopkins University Press, 1984. Pp. xi + 270. \$27.50.)

David B. Magleby has written a book that is both interesting and important. It is interesting because of the craftsmanship with which he manipulates a number of survey and aggregate data sets (more than a dozen in all, including the Michigan data, four state data sources, and two national surveys that Magleby conducted himself) in a straightforward statistical manner with a view to building a composite portrait of a typical participant in initiative and referendum elections not bound by issue areas, locations, and time. It is important because it makes a solid contribution, in my opinion, to the voting literature in general. Much of what we know about voting behavior derives from the partisan/candidate context, and Magleby's work by definition goes beyond that context. In addition, his attempt could and should easily provoke further thoughts on the classical tensions between the competing models of mass and representative democracy.

Most of Magleby's findings can be grouped in terms of the assumptions made for the argument favoring adoption or expansion of direct legislation: increased citizen participation, improved representation of voter preferences, informed voter decision based on increased interest and information, a less ambiguous interpretation of individual voting preferences, and an effective check against special interests and party machines. Most of the main features of these assumptions are, however, rejected by Magleby's analysis. Direct legislation does not increase turnout except on salient issues such as California's Proposition 13. Citizens who vote on ballot measures are much less representative of the general electorate than those who vote in candidate races because of the heightened level of knowledge, sophistication, and sense of efficacy that it takes to understand and survive the complicated issues, ballots, and the excessive number of voting decisions. Voters are confused over the meaning of the vote because they are no better informed about the propositions than they are about candidates, except again on salient propositions such as California's nuclear and rents control initiative and the Massachusetts vote on graduated income tax. The decision concerning which

outcome ought to be read as a popular mandate is often difficult because of the low participation rate, voter irrationality, and emotional reaction to the media campaign. The overall influence of special interest groups remains strong in the direct legislation process because of their heavy involvement in referendum politics.

All of this constitutes an important body of findings. Its significance goes beyond that of a mere description in that it expands and adds different dimensions to the contents of the existing general literature on voting, notably the Michigan studies and the work by Raymond E. Wolfinger and Steven J. Rosenstone. One could quarrel with Magleby's use of different comparative referent in arriving at the negative conclusions about those who participate in direct legislation. Given what we know about voting behavior in general, it does not seem appropriate to compare voters in candidate elections with those in initiative and referendum elections. Thus, comparing the "dropoff" rate (the proportion of voters who cast ballots but who do not vote in a particular candidate race or on a proposition) with the concept of "rolloff" or "falloff" rate (the proportion of voters who vote for the office listed first on the ballot but do not vote in a subsequent candidate race) as a measure of participation on candidate elections is a rather harsh way of evaluating the assumption about the level of participation on propositions. Besides, Magleby does confirm even a higher rate of participation when voters decide on propositions in primary elections than for about one-half to two-thirds of the candidate races. In addition, salience of issues involved visibly improves both the participation rate and the information level of the voters.

There are other minor objections to be sure. Linking the direct legislation process somehow with the atrophying party system seems at best to be an exercise in building a spurious relationship, especially when Magleby demonstrates some highly charged partisan involvement in direct legislation resulting in unmistakably partisan division of votes later. Conversely, it borders on a tautological statement to say that most of the apartisan or nonpartisan referendum elections generate a situation in which the voters act without partisan cues.

Finally, when discussing direct legislation and its relevance to the theory of mass democracy, the book might have taken on even a greater theoretical significance with a perhaps more thoughtful analysis of the Progressive reform ideology as the background factor and the so-called Swiss model that, in part, defines the voting universe in terms other than a simple numerical size. After all, Magleby's point about whether "all voters participate in deciding ballot propositions" (p.

34) is a misreading of the model of mass democracy. For one thing, one may view as an encouraging indication the fact that direct legislation, at this stage of its development, improves certain attributes of particular sectors of the general electorate including the issue public.

These reservations aside, Magleby has made a significant foray into an important issue area in political science today and has added much to the developing literature on the subject of direct legislation, most importantly using it as a linkage to a greater body of empirical findings about voters in general and the normative literature on democracy.

JONG O. RA

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The American Elections of 1982. Edited by Thomas E. Mann and Norman J. Ornstein. (Washington, D.C.: American Enterprise Institute, 1983. Pp. xi + 203. \$16.95, cloth; \$8.95, paper.)

Add this book to the small but growing list of works on midterm elections. Like the American Enterprise Institute's book on the 1980 elections, this volume emphasizes description over analysis and provides basic facts and background on the congressional campaigns, political action committees (PACs), redistricting, and state elections. The book recounts a nice summary of the 1982 elections, but it provides little in terms of placing these elections in historical context.

Albert Hunt's chapter on national politics and the congressional elections covers the issues and major factors that influenced these campaigns quite well, but none of this information is new. Hunt gets down to specifics occasionally, detailing how some of these factors affected individual races. This is a valuable contribution because most works on national elections rely on aggregate data.

Curiously, Hunt gives little attention to a phenomenon that received a great deal of attention at the time—the so-called gender gap. Hunt discusses the gender gap strictly in terms of attitudes toward President Reagan, and then writes, "There was little doubt that this problem was affecting other Republicans" (p. 21). In fact, there is some evidence to suggest that the gender gap had little impact on the outcome of the 1982 races.

The effects of reapportionment are examined by Alan Ehrenhalt, who demonstrates how federal courts and the Voting Rights Act helped to protect Democrats and minorities in spite of

anticipated Republican gains. John Bibby presents data on gubernatorial and state legislative races and provides some brief analysis and speculation on what the results entail for the future.

Larry Sabato assesses the role of PACs, independent groups, and parties on the 1982 elections and concludes that, due largely to the efforts of the Republican National Committee, we may be entering an era ripe for a party-responsibility system. Sabato may well be correct about this, but in concentrating on the details of 1982 campaign contributions, he ignores several major questions about PACs, including the targeting of contributions to members of specific committees and the way many incumbents build up massive war chests in order to discourage credible opponents. Sabato's optimism about party resurgence is offset by the comments of the editors in their chapter on voters and Congress. Mann and Ornstein point out that few Republicans ran as Republicans and few Democrats as Democrats. They write, "The use of party in campaigns in 1982 strongly underscores the notion that dealignment, rather than realignment, is the prevailing trend in American politics" (p. 148).

The editors have provided a handy summary of the 1982 elections, including useful appendixes, that will be of value to those who study elections. It is good to see a chapter on state races included, as these are often slighted in election studies. Those who wish to place the events of 1982 into a more theoretical context have several other works to turn to, notably Barbara Hinckley's *Congressional Elections* (Congressional Quarterly Press, 1981) and Gary Jacobson's *The Politics of Congressional Elections* (Little, Brown, 1983).

PAT DUNHAM

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Special Interest Groups in American Politics. By Stephen Miller. (New Brunswick, N.J.: Transaction Books, 1983. Pp. xiii + 160. \$19.95.)

A clear understanding of how interest groups fit into the landscape of American politics is not easily achieved. Scholarly literature on the topic is disappointingly slim, and media attention is frequent but often sensational or exaggerated. As a result, a debate has ensued, without reference to a sound theoretical base, as to whether interest groups are beneficial to the achievement of the public interest or whether they are detrimental to good public policy. Stephen Miller's book is a significant contribution that sheds light on this complex aspect of American political behavior by providing a needed theoretical statement that helps explain how faction and special interest are incor-

porated into the whole of the American political experience.

The central theme of this work is a description of how the framers of the United States Constitution perceived the problem of balancing private interests with the public interest, and how, through institutional arrangements in a federal form of government, they addressed the problem. The book consists of three main parts: the intellectual and historical influences that shaped the rationales of the Constitution's framers; how *The Federalist* dealt with special interests; and how three distinct periods of American political history have tested the solution to special interests proposed by the Framers.

A valuable feature of this book is Miller's careful tracing of the intellectual currents that shaped the thinking of people like Alexander Hamilton, George Washington, Thomas Jefferson, and especially that of James Madison. Relying on the works of political theorists like Hobbes, Hume, Burke, Montesquieu, Bolingbroke, and Pope, and, accounting for the political and cultural environment of colonial America, Miller provides insight into how "interests" should be regarded, and how government can accommodate them.

The substantial assessment of *The Federalist* and the genius Miller ascribes to Publius may be overstated, but it does offer a model for gauging the place interest groups occupy in American politics. Clearly, the problem is not with special interests, Miller argues, but with electing to public office, especially the national legislature, "enlightened men" who will render special interest subservient to the public good. The threat to the public interest is not special interest, but public opinion fanned by emotion. The 1790s, the first of three American history periods examined, is an example of how emotional factions—the Federalists led by Hamilton and the Republicans led by Madison—nearly destroyed the Union.

The second period studied to test the Publius model for regulating special interests is that of the 1870s and 1880s. There the politics of disinterest practiced by reformers considered to be intellectuals and "the best men" threatened the common interest because of the scorn they had for popular opinion and individual interests. Professional politicians who exercise a politics of pragmatism and accommodation more closely approximate the Publius model by heeding to popular opinion and channeling special interests through political parties and local politics.

The emergence of public interest groups in the 1970s is the final period Miller evaluates in light of the Publius model. As expected, Common Cause and Public Citizen are identified as "disinterested" groups that failed the Publius test because they are judged to be self-righteous, prescriptive,

nonresponsive to public opinion, and to exercise patriotic rhetoric. Such a scathing review of the 1970s makes one wonder what Miller would say, and why he did not, of the political behavior of the New Right, especially the religious right, in the late 1970s and early 1980s.

Miller's critique of the theoretical problems associated with special interests in American politics is well done. However, his conclusion—that special interests are not as influential in policy formation as publicly perceived—is a generalization woefully in need of empirical documentation. Based on that generalization is the identification of the central problem of special interests, namely the misconception of the public regarding interest group influence. I am doubtful that that is the central problem, and I find Miller's recommendations for solving the perception problem to be both weak and unimaginative. I feel that the Publius idea of "regulating" special interests takes more than changing legislators and their environment.

TIMOTHY J. O'BRIEN

Marquette University

Public Sector Performance: A Conceptual Turning Point. Edited by Trudi C. Miller. (Baltimore: Johns Hopkins University Press, 1984. Pp. ix + 276. \$27.50, cloth; \$12.95, paper.)

This collection of nine essays evolved from a review of the literature commissioned by the National Science Foundation, but most of the authors move well beyond review to critical analysis. Although some of the essays hew more closely to the book's topic than others, between them they cover many important aspects of public-sector performance.

For example, J. Richard Aronson and Eli Schwartz ably evaluate a number of public finance tools for measuring and improving government performance. Ronald W. Johnson and Arie Y. Lewin examine performance measurements for service delivery. They note that the development of a single measure of overall organizational performance is not in sight, but they point out a number of promising developments. In a chapter titled "Information Systems and Intergovernmental Relations," John Leslie King and Kenneth L. Kraemer find considerable progress in applying information technology to very simple governmental operations such as billing and accounting. However they conclude that "it is difficult to find advanced [information] systems for planning and management that significantly affect how planning and management are done" (p. 122).

Thomas J. Anton argues that most of the broad conceptual literature about intergovernmental change in the United States manifests a "disdain for data" and uses "politically loaded symbolism" (p. 32). He maintains that empirical analyses implicitly refute much of the conventional wisdom. For example, despite frequent charges that the current intergovernmental system is becoming too complex, "only 100 of the grant programs provided fully 95 percent of total grant funds available, hardly an impossible number to comprehend. At the state and local level, meanwhile, the intergovernmental system has become significantly *less* complicated over the last three decades" because of the elimination of a large number of townships and school districts (p. 40). Similarly, Anton presents evidence to refute charges that federal grants greatly distort local priorities, and he argues that grant-financed programs have been much more successful than usually perceived.

Patrick D. Larkey, Chandler Stolp, and Mark Winer evaluate the literature on "Why Does Government Grow?" Like Anton, they find that a normative (antigovernment) bias distorts many of the analyses. They conclude that "in this literature, as in many social science literatures, the inductive work lacks theoretical rigor and the deductive work lacks empirical relevance" (p. 90). Among a series of quite useful suggestions for further research in this field, they propose that academia reward the unglamorous work of data gathering more highly and provide fewer rewards for "theorizing about concepts that cannot be represented empirically and devising increasingly arcane methods for analyzing fundamentally bad data. . ." (p. 92).

John J. Kirlin argues for a greater emphasis on the political aspects of government performance. He reviews the literature on policy implementation and on federalism, and, premising his analysis on the work of Charles Lindblom and especially Aaron Wildavsky, he calls for designing governmental systems to encourage a more flexible "strategic" rationality rather than synoptic rationality.

Trudi C. Miller's concluding chapter asserts that we have reached a turning point in practice and thought about government performance, in large part because previous work on this topic has been based on false "natural science" assumptions. The natural science model assumes that "(1) the laws that govern human behavior exist independently of human control, and (2) the units

ences as physics, and more on engineering oriented sciences such as computer science, because political institutions are to a large extent man-made and manipulable—more like computers than like stars. Parts of Miller's proposed "design science" bear a strong resemblance to decision and management sciences. Her short essay understandably does not detail the exact shape of the new design science. Nonetheless, like a number of the chapters in this collection, it raises interesting issues for future consideration.

JAMES E. SWISS

North Carolina State University

Communists in Harlem during the Depression. By Mark Naison. (Urbana: University of Illinois Press, 1983. Pp. xxi + 355. \$22.95.)

Mark Naison makes an important scholarly contribution in this volume. His objectives are to describe and analyze the interrelationships between blacks and the Communist party in Harlem during the Depression, to provide an understanding of how those interrelationships were forged and how they evolved over a relatively brief span of years, and to assess the impact of communist influence on the political and civil rights protest efforts of blacks in Harlem.

Naison judiciously uses primary materials to assemble the information base of his study, including interviews with many of the leading black communists of the 1930s and documentary resources.

The book's 12 chapters are grouped into three parts. Part 1, which is organized chronologically, traces the growth of the Harlem Communist party from its formation in the early 1920s to the founding of the National Negro Congress in 1936. This section focuses on the tensions that resulted from the largely successful efforts of the early black recruits to the Communist party to transform the Harlem Communist party from an organization dominated by the problems and perspectives of foreign-born members (largely European ethnics) to an organization more committed to addressing the problems of American blacks.

Part 2 examines in thematic format the Harlem Communist party during the period known as the Popular Front, a period characterized by a new strategy of the Communist party that called for the incorporation of the entire black community

leadership to improve its ties with black organizations and to increase its influence within the Harlem black community in general.

Part 3 examines the erosion of the Party influence in Harlem between the signing of the Nazi-Soviet Pact and the United States entry in World War II. The significant development in this section is the emergence of an indigenous protest leadership in Harlem capable of executing mass protest operations without assistance from the Communist party.

Perhaps the principal strength of the book is that it provides, for the first time, a scholarly, mature account and analysis of an important development in the twentieth-century history of urban racial/class mass protest movements. Naison has no difficulty in placing his research in a stream of previous scholarship that examines the influence of the Communist party in black protest movements in the United States, which indicates that his study has a fairly rich theoretical base. Naison's findings on the personal dimension of black membership in the Party and the evolution of the Party's racial policies are very clear: The Communist party, including some black communists, did play a major role in mass protest movements in Harlem between the late 1920s and early 1940s. Naison is equally as clear in his position that the attraction of the Communist party to Harlemites was always stronger among black artists and intellectuals rather than rank and file blacks. Even among those blacks attracted to the Party, the basis for their collaboration was not an embrace of the communist ideology, but rather a calculated recognition that the human and financial resources offered by the Party were important to their efforts to use protest strategies to extract political, social, and economic benefits from political and economic elite structures. Another important point that Naison makes is that although many blacks in Harlem appreciated the Party's support of black causes, even at the height of Communist influence in Harlem in 1938 the Harlem branch of the Communist party had less than 1,000 black members.

Other strengths of the book include the author's detailed description of relevant developments, meticulous analysis, careful documentation, and reasoned conclusions. Regarding the latter, Naison is careful to draw only those conclusions that are well within the limits of the data consulted.

The weaknesses of the book are relatively minor. For example, although Naison thoroughly accounts for the early difficulties the Communist party had in winning influence among black Harlemites in the early 1920s because of the relative prosperity of the decade—and the Party's subsequent success in attracting support among

blacks in the 1930s because of the economic and social malaise of that decade—he does not provide an explanation as to why blacks were able to develop an independent mass protest capacity in the early 1940s as opposed to an earlier or later time.

A special feature of the book is a chapter in the Appendix that focuses on black-Jewish relationships within the Communist party in Harlem. This is a very enlightening chapter, which provides an important historical perspective to the transformation of the interrelationships between blacks and Jews which has occurred since their collaboration in the civil rights movement in the South in the 1950s and 1960s.

Communists in Harlem during the Depression is an excellent book. I believe that it will stimulate further research into the role of the Communist party in other twentieth-century mass protest movements in the United States.

HUEY L. PERRY

Southern University

Forces of Production: A Social History of Industrial Automation. By David F. Noble. (New York: Alfred A. Knopf, 1984. Pp. 409. \$22.95.)

One of the most intractable challenges facing us in the social sciences is to *explain* technological development. We all tend to objectify technology, to see it, in Noble's words, as "an irreducible brute fact, a given, a first cause" (p. xiii). Even those few who have argued that technology is determined by socioeconomic forces have been unable to support their assertions with detailed and convincing evidence, thereby reinforcing the hegemony of the technological determinists. Given the absence of significant alternative technological approaches among advanced industrial societies (including state socialist ones), antideterminists are left with but one recourse in making their case: to reconstruct the range of options available at the birth of various technologies and to identify the forces propelling the development of one option and the demise of others. It is this "travel down roads not taken" (p. 146) that makes Noble's book such a pathbreaking work.

To make his case against the technological determinists, Noble selects to examine the birth, development, and implementation of automatic control in the machine tool industry. At the end of World War II there were two principal options available for the design of automatic control: "record-playback" (R/P) and "numerical control" (N/C). Each was designed to produce a variety of parts under the instructions of various

programs stored on a permanent medium—in other words, to be “self-acting,” or automatic. Each, therefore, reduced the amount of skilled labor required in the production process. There was, however, an essential conceptual difference between them. Whereas R/P depended on, and thereby acknowledged, the skills of the machinist by recording and making a program of his movements as he put the machine through its paces, which could then be played back indefinitely, much in the manner of the player piano, N/C eliminated “altogether” (p. 84) the need for the machinist by having the program produced by computer programmers working directly from management blueprints.

Noble argues that N/C “won” not because it was necessarily the “best” design in terms of technical efficiency, cost-effectiveness, or commercial viability, but because it dovetailed neatly with the separate and complementary interests of key groups in the society. The military (in particular, the Air Force) and the scientific community (in particular, MIT) play a crucial role in this story. The Air Force, with their requirements for parts fabricated to extremely close dimensional tolerances (necessitated by the development of high-performance aircraft), pushed hard for N/C, which they hoped would produce machining free of human error. The scientists at MIT, already predisposed to prefer abstract, formal, and deterministic solutions to problems and seduced by the largess displayed by the Air Force, were willing accomplices in the promotion of N/C.

Nevertheless, according to Noble, R/P had several commercial advantages over N/C and could well have been attractive to the vast majority of machine tool firms that did not need the capabilities of N/C. Why, despite the relative simplicity and cost-effectiveness of R/P, did it languish? Noble’s answer is straightforward and expresses the central thesis of the book. R/P lost out because it threatened to undermine the existing power relations in the society by lending itself “to programming on the shop floor, and worker and/or union control of the process” (p. 151). N/C won because it “held out the promise of greater management control . . . [and] seemed a step closer to the automatic factory” (p. 167).

Noble has accumulated a great deal of documentation in support of his thesis, much of it of a primary nature. Clearly, there will be disagreements with his interpretations. I, for one, am not persuaded by his reading of the GE experiment in worker participation with N/C, which he claims was abandoned because it threatened management control even though it was profitable. The dichotomy Noble sets up between productivity and profit on the one hand and control and domination on the other seems forced, and

his assertion that capitalism is not and never has been a “system of profit-motivated, efficient production” (p. 321), but is rather a system whose goal is “domination,” is unproductive. What differentiates capitalism from other modes of production is not the goal of class or group domination—all have had that in common—but precisely the means by which the domination is established and preserved.

Disagreements notwithstanding, this is a magnificent achievement in social history. Noble has now pointed the way for all students of technological development, and it would be criminal indeed if the current technological revolution should be accepted passively, mouth agape, without some examination of the multiplicity of roads before us.

LEON GRUNBERG

University of Puget Sound

Citizen Participation in Science Policy. Edited by James C. Peterson. (Amherst: University of Massachusetts Press, 1984. Pp. 244. \$23.50, cloth; \$9.95, paper.)

The role of mass participation in highly complex science and technology policy areas is a topic that interests both the democratic theorist and the public policy analyst. Fortunately, *Citizen Participation in Science Policy* is a collection of essays that might satisfy both audiences. James Peterson has carefully integrated works on democratic theory, participation, and policymaking, all with a science policy focus.

The volume begins with an overview by Peterson that is followed by Dorothy Nelkin’s discussion of science policy and the democratic process. She examines the role of the technical expert and argues cautiously for greater public scrutiny and citizen involvement in science policy. Her discussion precedes four chapters that fall under the heading, “Varieties of Citizen Participation.” They provide the reader with an instructive, straightforward “how-to” guide for potential activists in science policy.

The final section of the book is a series of three case studies of health policy and three case studies of nuclear policy. The health section includes a fascinating look at the “over selling and buying” of the U.S. war on cancer, a useful overview of the role of consumer groups in health planning, and an integrative evaluation of the impact of participation in biomedical policy. In the biomedical policy article, Diane Dutton effectively addresses some issues that characterize the appeal of the book. First, she questions whether participatory democracy is possible in the modern context of

mass organization and technological interdependence (p. 149). She also pointedly asks, "In the present society, do ordinary people—people from all walks of life—really have anything useful to say about technical issues whose intricacies defy even the experts?" (p. 149). Her thoughtful four-case study reviews the constraints on public involvement and disadvantages of mass participation in biomedical policies; however, she concludes that "with confidence faltering in science along with other major institutions" (p. 172), citizen involvement should be encouraged.

The section on nuclear policy begins with an essay by Randy Rydell on points of public interaction in the research and development process in three nuclear power controversies. When dealing with the utility of public participation in science policy, Rydell is less optimistic than Dutton's. He concludes, "There should . . . be no delusions about public participation in the R&D process: at times it will delay needed projects, exacerbate conflicts by rendering issues more difficult to disentangle, legitimize prior decisions, and advance special interests" (p. 194). Jane Kronick, in her article on participation in congressional hearings on nuclear power development, is dismayed by the ineffectiveness of participation in that setting. John Hunt and Neil Katz conclude the section on nuclear policy with a generally positive view of protest participation: Their observations are based on an analysis of a telephone survey of community residents after the Seabrook, New Hampshire Antinuclear Power protest.

Even though the chapters are uneven, I can recommend this book not only in writing style, but also in perspective; many of the authors are practitioners, while the remainder are academicians both inside and outside of political science. There are no pathbreaking contributions or single outstanding conclusions found in the volume; rather this is simply a useful compilation of readings that can introduce the newcomer to some important questions concerning democracy and science policy.

T. WAYNE PARENT

Louisiana State University

The Leadership Question: The Presidency and the American System. By Bert A. Rockman. (New York: Praeger Publishers, 1984. Pp. vii + 253. \$29.95, cloth; \$14.95, paper.)

The ambiguous concept of presidential leadership may be approached in two ways. One ap-

proach (e.g., Barbara Kellerman) emphasizes personal aspects, while the other focuses on the formal powers of the office itself, organizational, or contextual variables. *The Leadership Question* considers both avenues, but ends up arguing that personal factors are not critically important. Rockman helps to flesh out and synthesize research on a concept that has too often been equated simply with the equally ambiguous notion of power. He begins by defining leadership as "the capacity to impart and sustain direction" (p. 6), but later adds two other components: effectiveness and acceptance. He sees no simple relationship among the three components or formula for employing them to assert leadership. However, through such a definition, he avoids the criticism leveled at Richard Neustadt—focusing only on means and not ends.

The Leadership Question is a broader study than it first appears to be. More than any other, this book places the presidency in the larger context of government and society, both in America and elsewhere. This is the best study yet of the overall influences on the president. Political philosophy and comparison are incorporated; thus it is more than a book on leadership or even the American presidency. Although Rockman sees presidential leadership as composed of luck and opportunity, constraints are the dominant characteristic. The focus is mostly on external constraints such as political parties (which seem overemphasized), elections, the media, public opinion, constitutional and historical factors, and political culture. Some attention is given to Congress in the internal governmental structure, but virtually none to the courts, staff, or bureaucracy—elements of leadership potential that Neustadt feels more important. This concentration on Congress (chap. 4) seems a bit forced, perhaps because useful illustrative data are available.

If one can criticize the relative mix of constraints, the comparative focus is not always as systematic as it first appears. Much of the comparison is cross-national, a worthwhile enterprise rarely found in presidency books. Rockman correctly points out that concepts like acceptability and individual liberties are more important in the United States than elsewhere, but more methodical within-nation comparisons are needed as well. Obviously, political culture is a more operative variable when comparing executives cross nationally rather than internally. Rockman discusses several cycles of presidential leadership potential but draws no conclusions as to which are most important and why. Some effort should be made to integrate meta and term cycles with the epochal one. Also, little evidence is provided for the assertion that personal skills are less important

than contextual factors. Surely personal styles account for much of the leadership differences between presidents Carter and Reagan because the constraints were not all that different. Perhaps the seemingly limited influence of presidential skill is attributable to inadequate systematic measures.

Much of my criticism of this book deals not with what is said, but rather with the way in which it is said. Rockman is a careful craftsman, but the book is ponderous at times. It contains a lot of difficult verbiage, and difficult methodological terms are incorporated outside of their normal context. Thus, *The Leadership Question* is essentially a monograph with some appeal in graduate courses on the presidency, American government, and perhaps even political philosophy.

Occasionally, clever turns of phrases seem to be made for their shock value alone, for example, "politicians, being more resourceful than political scientists, rely on other readings [apart from public opinion polls] as well" (p. 117). Generally well edited, the book is relatively error free, except for a few incorrect or incomplete citations. The index could have been more thorough, for example, it fails to mention some authors mentioned in the text and works cited in footnotes.

The data presented often are more illustrative than definitive. Frequently, data are not developed, and we are not always told what the tables reflect. Some tables are incomplete (e.g., year axis in Figure 4.6), and most are quite dated (e.g., Tables 4.1-3). Rockman is better at model-building than in presenting data simply, for example. Finally, one wishes for more consistent and precise uses of policy terminology.

Although much of this review may seem critical, *The Leadership Question* is an important study. It has flashes of brilliance and is a stimulating and thoughtful analysis. Rockman nicely contrasts extant theories of leadership, and if he is not yet able to explain or predict leadership, he describes it very well indeed. In better specifying the components of a model of leadership (if not always telling us which elements are most important), the book is perhaps as close to a theory as political scientists have come. Rockman is quite aware that he has not yet arrived at theory but has begun the necessary linkages.

Although this book contains no explicit hypotheses, and by definition no validation, it does contain interesting assumptions and propositions that are the first steps in theory-building. The difficulty lies in seeing how some of Rockman's assertions could be tested, and the study provides no directions for future research. Rockman is aware that he has not answered the leadership question, but he has given us a better understanding of it. With this study, he has made a definite contribution and has established himself

as one of our most thoughtful writers on the American presidency.

STEVEN A. SHULL

University of New Orleans

Drastic Measures: A History of Wage and Price Controls in the United States. By Hugh Rockoff. (New York: Cambridge University Press, 1984. Pp. xi + 289. \$29.95.)

Economic historian Hugh Rockoff argues that many criticisms of wage and price controls are often based more on anecdotes about black markets and isolated market distortions than on the historical record. His own assessment of the performance of controls during the colonial and revolutionary periods, the two world wars, the Korean conflict, and the Nixon administration persuades him that a system of temporary, across-the-board controls is a viable alternative to the political costs of a recession. He is only marginally successful demonstrating this.

Rockoff's original goal was to adapt the lessons of wartime inflationary crises to the persistent inflation of the post-Vietnam era. His assumption that the latest inflation had reached crisis proportions similar to those of wartime is questionable. But his use of those earlier periods as "natural experiments" typified by "rapid and extensive change" (p. ix) strengthens the comparison. Whether such inflations will again reappear is a good question. But the broader value of Rockoff's book is that it suggests much about the response of the American political economy to crisis. Although his focus is economic, his analysis also highlights the extreme pressures borne by the American political system.

Drastic Measures is both historical and analytical. Each substantive chapter consists of a historical narrative recounting the particular episode and an assessment of the accuracy of four major criticisms of wage/price policies. Although the historical accounts are informative, although sometimes incomplete, it is the evaluation of these episodes that constitutes Rockoff's major thrust.

Two criticisms of controls assessed by Rockoff concern the economic effects of controls. Reviewing the success of controls in slowing inflation, Rockoff finds that across-the-board controls, when supported by restrictive monetary and fiscal policies, work much better than critics allow. Furthermore, he argues, the costs to productive efficiency of controls are often no worse than those that occur in a freely operating market (p. 237). Finally, he says, the protection of fixed-income groups afforded by controls is conducive to public support as well as humane (p. 320).

A third criticism of controls tested by Rockoff concerns the size, costs, and efficiency of the ensuing controls bureaucracy. Unfortunately, he uses an inadequate standard of measurement. We learn little about these characteristics of the controls bureaucracy when they are compared with the postal service. A comparison with an analogous regulatory agency would have been more fruitful. It is also disappointing that there are only suggestions of the role played by the states in the controls program. Despite these flaws, however, one is persuaded that the fear of bureaucratic giantism stemming from controls is somewhat exaggerated.

In evaluating the charge that evasion will be extensive under controls, Rockoff gets to the crux of the controls debate: cooperation. Again, in relying on reported numbers of court cases and sanctions, Rockoff's standard of measurement is inadequate. It overlooks those who got away. Even with the survey data he cites and his documentation of evasion incentives, Rockoff can only tentatively conclude that evasion was not all that pervasive. Without a major national crisis, it will be "politics as usual" (p. 183).

Drastic Measures contains an admirably extensive bibliography. Unfortunately, Rockoff's conclusion fails to offer a new solution to the political problem of controls. His proposed "Board of Stabilization Overseers" (p. 250), intended to facilitate cooperation between monetary, fiscal, and control authorities, does not solve their conflicts but merely institutionalizes them. Although the Reagan administration appears to have made recession a politically survivable inflation policy, *Drastic Measures* will provide valuable information both to policymakers who secretly contemplate controls and to social scientists interested in the response of a market-oriented political economy to crisis-induced regulation.

GORDON P. HENDERSON

Purdue University

PAC Power: Inside the World of Political Action Committees. By Larry J. Sabato. (New York: W.W. Norton, 1984. Pp. 272. \$15.95.)

Larry Sabato has written a fitting companion to his excellent study, *The Rise of Political Consultants* (Basic Books, 1981). Indeed, the growth of political action committees is an integral part of the "new ways of winning elections." The subtitle of *PAC Power, Inside the World of Political Action Committees*, is, however, too narrow: Sabato provides an invaluable description of all aspects of PACs, not just their inner workings. A more accurate subtitle might be "everything you

wanted to know about PACs and were afraid to believe." Sabato helps sweep away the cobwebs that obscure the debate on campaign finance.

The great strength of *PAC Power* is the detailed factual information it contains. The book draws on three skeins of data, each of which would be worth a book on its own. First, Sabato reports on an extensive mail survey of a random sample of 399 multicandidate PACs. Second, some 60 personal interviews with key participants and observers of PACs cover nearly every phase of their operation. And third, *PAC Power* includes a thorough compilation of recent scholarship, journalism, and anecdotes relevant to campaign finance. Sabato stitches these disparate strands of evidence into a rich and stimulating tapestry.

What emerges from Sabato's analysis is the thick "web of politics" that enshrouds PACs. Campaign contributing is only one of the many political activities in which PACs engage. For example, more than three-fourths of PACs surveyed were involved in some other kind of political education program such as voter registration drives. And most large PACs spend as much effort mobilizing their members as currying favor with officials. The influence of money is closely entwined with the power of these other resources.

The web of PAC politics extends beyond their entangled activities to the connections between committees. As a whole, PACs are very diverse in terms of internal organization and interests; even within well-defined sectors such as labor and the New Right, PACs specialize to a bewildering degree. These variegated groups are tied together by complex and subtle networks that share political intelligence and tactics and develop common strategies. In this regard, Sabato sees PACs as a source of party strength and renewal, for even competitive interests must ultimately bind themselves together to be effective, and the major parties still hold the ties that bind best. PACs have power as much because they weave strong coalitions as because of their financial prowess.

Sabato is basically a reformer: "The need for campaign finance reform remains undeniable" (p. 171). But successful reforms must take into account the politics that enmesh PACs. Political money cannot be willed away, Sabato argues, no matter how sincere the will or how severe the way. The object of reform should be to minimize the less desirable aspects of PACs while encouraging their many positive attributes. The spiders that inhabit the political webbing of campaign finance are certainly ugly, occasionally poisonous, but they can be made to catch flies. Towards this end Sabato proposes a number of modest reforms that ought to be taken seriously.

This book has only a few weaknesses. Some readers may find Sabato's use of aggregate data

frustrating: Much of his work begs for individual-level analysis connecting the characteristics of PACs to their behavior. Likewise, students of Congress and parties may find Sabato's interviews and anecdotes a poor substitute for systematic evidence of the patterns he alleges. However, none of these points mars the fabric of the study. Indeed, scholars pursuing these issues will find *PAC Power* indispensable to their work.

JOHN C. GREEN

Furman University

America's Hidden Success: A Reassessment of Twenty Years of Public Policy. By John E. Schwarz. (New York: W.W. Norton, 1983. Pp. 208. \$5.95, paper.)

This is a timely book. John Schwarz's basic objective is to take issue with what has become the conventional political wisdom that during the post-Eisenhower years (1960 to 1980) the American political economy failed to live up to expectations, and that in particular the social and economic programs pursued by the welfare state had failed to alleviate the problems they were designed to address.

Schwarz begins by examining the claim that liberal programs failed to achieve their goals. Scrutinizing the results of liberal governmental programs to reduce poverty; improve housing, health, and nutrition; and to halt and reverse the degradation of the environment, Schwarz produces an impressive array of evidence to demonstrate that government efforts were in fact very successful. For example, Schwarz marshals evidence (pp. 34-38) to show that antipoverty programs reduced poverty at a rate that could not have been achieved by the private sector alone, and the programs did so without significantly reducing the incentive of Americans as a whole to seek work. Similarly, Schwarz shows that "the government's nutritional programs were almost fully effective in reducing flagrant malnutrition among Americans in locales of concentrated poverty across the nation . . ." (p. 45). Similar successes emerge in the areas of housing, health, and the environment. Although government programs never totally eliminated any of these problems, they more successfully alleviated them than is commonly believed. Moreover, the programs achieved this success with relatively little waste. Citing figures drawn from the Republican Study Committee of the House of Representatives and President Reagan's Council on Integrity and Efficiency, Schwarz demonstrates that the amount of

waste in social welfare programs amounts to somewhere between one-half of 1% and 3% (pp. 69-72).

Schwarz then turns to the claim that the American economy was stultified in the 1970s as a result of government regulation, taxation, and support for social programs. Once again, the image that emerges as a result of Schwarz's examination of the record is markedly different from the popular myth of conservative conventional wisdom. "No unbridled growth of the nation's government took place from 1960-80; instead, in most respects, the government grew very slightly in the 1960s and 1970s in relation to the size of the nation's economy. This is the case whether one considers taxation, deficit spending, the size of the bureaucracy, or the amount of regulation . . ." (pp. 80-81). The result was a relatively strong economic performance, one comparable to that of the 1950s.

What then was responsible for the perception that the American political economy had floundered in the post-Eisenhower years and that government was responsible? Cynicism resulting from Watergate and Vietnam cultivated by the new conservative ideology helped perpetuate the myth of government incompetence and responsibility for a failing economy (pp. 117-122). But most importantly, there were demographic and cultural developments that overwhelmed even the relatively strong economy of the 1970s. Specifically, the entrance of the baby boomers into the work force and the increase in the number of women seeking jobs combined to increase the number of new workers seeking jobs (by 40%, or 30 million workers, between 1965 and 1980) at a faster rate than even a strong economy could provide for (pp. 123-124). And this in turn increased aggregate demand to put upward pressure on prices, while many new workers found work in jobs that only marginally increased productivity.

According to Schwarz, this new understanding of the source of America's political-economic troubles requires that we reexamine new conservative policies. Because it is based on a false image of the past (i.e., that welfare state programs did not work and that government actions constrained the efficiency of the market), the conservative agenda threatens not only to undo the successes of the past, but also to result in policies that "will consign many millions of Americans and their families to unemployment for a number of years down the road, and leave more underemployed" (p. 152). In addition, we must reexamine our national investment policy. Schwarz concludes that, "The government could, and should, do more than in the past to channel a larger percentage of the funds available for investment into those areas for which the productivity

payoffs to the country are likely to be at a high level" (p. 155).

Schwarz's argument is an impressive challenge to many of the current interpretations of American political economy. But it is not clear that it refutes the arguments of reindustrialization and deindustrialization theorists. Although Schwarz provides evidence that investment remained stable from 1950 to 1980 (pp. 108-109), he later admits what both deindustrialist theorists and reindustrialists argue, that what is important is the composition of that investment (p. 155).

Finally, according to Schwarz the crowded generation is in some significant part a result of many of those previously excluded from full or equitable participation in the American political economy now realizing the promise of that political-economic ethos. But this seems to imply, as crisis theorists argue, that the welfare state cannot deliver on the promise of advanced liberal society without at the same time producing pressures that undermine the economic performance.

These arguments do not necessarily prove Schwarz's account of the problems wrong, but his argument would have been strengthened by addressing them in detail. Had he done so, the result might have been a truly outstanding book. As it is, the book is still a strong challenge to the now-conservative conventional wisdom that all that is wrong with the United States is attributable to the welfare state.

MICHAEL T. GIBBONS

University of South Florida

The Decline of American Political Parties, 1952-1980. By Martin P. Wattenberg. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xix + 160. \$15.00.)

Few political phenomena have undergone the intense scrutiny that has been focused in recent years on the purported decline of American political parties. Analyses have probed the successive reforms of party rules and procedures, the increased use of primary elections and personal campaign organizations, and the impact of direct access to media for the purpose of reaching voters and mobilizing support. A great deal of attention also has been paid to the intriguing but elusive contours of voter opinions and behavior in light of this apparent institutional degeneration.

Wattenberg's small volume explores the attitudinal profile of American voters in an attempt to "assess the extent of the decline of political partisanship in the electorate, the nature of the decline, and the reasons for it" (p. 5). The author

readily concedes the difficulties of defining the concept of partisanship, but he argues that the attempt is preferable to relying solely on measures of party identification as many scholars have done. In fact, the very vagueness inherent in the term *partisanship* makes it more useful, according to Wattenberg, because it covers a far broader range of public attitudes toward parties. By contrast, the typical party identification scale identifies only the direction and strength of the respondent's affiliation without shedding any light on the underlying voter perceptions of parties or on the reasons for those perceptions. These missing pieces of the attitudinal puzzle are the objects of Wattenberg's search.

As recognized by the author, the task of clearly differentiating partisanship from more traditional measures of party identification is the most difficult step in this analysis. Rather than attempt a stringent, operational definition of partisanship, Wattenberg gives us a set of loosely woven indicators including party identification, party-line voting, split-ticket voting, and—most importantly for his purposes—measures of normative attitudes toward parties as institutions. Using survey data from the Survey Research Center/Center for Political Studies, Wattenberg reexamines conventional diagnoses of the symptoms and causes of declining participation, voter disaffection, and increasing numbers of independents. The result is a useful and clearly comprehensible synthesis of a sizable body of literature on the evolution of American electoral behavior. More importantly, Wattenberg states a significant, new interpretation of the survey research record in the familiar "good news/bad news" format: The good news is that voters have *not* become more negative in their attitudes toward parties; the bad news is that voters *have* become more neutral (read apathetic) toward parties. Furthermore, this decline in positive—and negative—views about parties has been underway for at least three decades, according to Wattenberg.

In the face of this new reading of voter surveys, one is tempted to ask the eternal question, So what? Does it make any practical difference that a prospective voter declines to identify with a political party not because the voter dislikes parties, but because the party seems irrelevant to the voter's world? Without addressing this question directly, Wattenberg does offer a partial answer. He feels that, if anything, voter neutrality would be harder to transform into positive participation than would attitudes of antagonism toward political parties. In his view, the decline in parties has been associated with a secular trend toward voter disinterest and is thus more deeply ingrained and more resistant to change than has been indicated by previous research.

Wattenberg's argument that voters are more neutral than negative is innovative and persuasive. However, his analysis of the causes for voter dispassion covers previously explored territory: candidate-centered politics, loyalty to personalities rather than to parties, campaign finance reform, and media encouragement of these political values. What is new is that all of these factors are examined for their contributions to the increase in voter neutrality toward parties.

One possible weakness in Wattenberg's analysis is the cursory attention he pays to recent literature on the emerging revitalization of party organizations at all levels. Although he briefly notes the work of Malcolm Jewell and David Olson on state party organizations and of Cornelius Cotter and John Bibby on the national level (p. 3), Wattenberg does not consider the possibility that better organized, better managed party organizations could begin to reinvigorate voter interest and affiliation. Republican organizations in particular have shown in recent years that grass-roots organization, technological innovations, and strong ideological themes can translate into electoral successes at all levels of government.

Nevertheless, Wattenberg's study is an important contribution to the ongoing attempt to understand the interplay of voter attitudes and the institutional parties. His book is well-written and highly readable, and its moderate length (131 pages of text) will make it an attractive supplement for courses on American parties or electoral behavior.

ROGER G. BROWN

Iowa State University

Making Campaigns Count: Leadership and Coalition-Building in 1980. By Darrell M. West. (Westport, Conn.: Greenwood Press, 1984. Pp. xii + 198. \$29.95.)

The study of elections has produced a copious amount of work centered on the voter. Little effort has traditionally been directed at understanding elections from the perspective of candidates. Darrell West puts forth a refreshing attempt at examining the 1980 presidential election and nomination process by researching candidate motives and actions. West's fundamental thesis is that candidates and their campaigns not only are an integral part of elections, but they also define voter choice, influence political realignments, assist in shaping the nation's political agenda, and play a crucial role in coalition formation.

West's analysis of the 1980 election is an exploratory study. Building on Richard Fenno's inductive research design, West utilizes six data

bases in which to draw inferences: public and private interviews with the top campaign advisors, traveling with various candidates, a content analysis of campaign rhetoric, an examination of candidates' daily travel schedules, an analysis of press coverage, and a study of audience reactions to candidate presentations. This innovative and extensive research yielded several fundamental conclusions.

First, although the post-1968 nomination process provided unprecedented opportunities for candidates, they also faced risks. Campaign strategies, developed years before the presidential elections, may be tactically incorrect, but quite likely they may be entirely ill-conceived. West argues that Baker's and Connally's campaigns in 1980 were unsuccessful because they pursued strategies that were based on assumptions that proved to be incorrect.

Second, candidates build their electoral coalitions based on three specific forces: their own electoral history, their party history, and their policy interests. West asserts that these three forces develop over years, and that "presidential coalitions [do] not arise full blown or by accident" (p. 36).

Third, candidates use rhetoric to establish agendas, effect party realignment, and form coalitions. Campaign rhetoric can provide a valuable clue to eventual presidential performance. West argues, for example, that Reagan's presidential agenda and his campaign rhetoric in 1980 were remarkably similar.

Fourth, an analysis of audiences on which candidates allocated precious time provides clues about the electoral coalitions they were attempting to form. West concludes that "candidate travels were not just media events; they were a vital technique for building coalitions" (p. 112).

Fifth, candidate symbolism is critical to developing coalitions because of the current era of open nominations. West writes that "it was apparent that they [candidates] devised symbolic appeals that communicated their coalitional strategies" (p. 130).

Sixth, campaigns and the media rely on audience reactions to provide crucial feedback as to whether a candidate's message is popular or well received. West also found that crowds are more receptive to nonpolicy speeches than policy appeals.

Seventh, the study of candidate coalitions can provide assistance in predicting presidential behavior. West asserts, for example, that Reagan's rhetoric, symbolism, and his coalition formation demonstrated that "he meant to undertake a major reassessment of the national political agenda . . ." (p. 157).

The notion that candidates work tirelessly at

forming winning coalitions during the general election is not new. West's analysis provides excellent insights into some of the techniques candidates use to build these coalitions. However, I disagree that coalitions are being formed in the nomination process. In *Consequences of Party Reform* (Oxford University Press, 1983), Nelson Polsby argues that a nominating system dominated by primaries encourages candidates to mobilize factions rather than build coalitions. The main goal of presidential aspirants during the nominating season is not to win with majorities, but to simply survive. Survival in the fast-paced serial nature of multiple candidate primaries means garnering a relatively small percentage of the vote. The winner of the primary often captures less than one-third of the possible votes.

Polsby argues that this type of nominating system encourages factional candidates. Coalition-building is not necessary to capture the nomination. West's argument that candidates pursue coalition strategies throughout the nomination process is in direct contrast to Polsby's thesis. A cursory glance at nomination struggles since 1968 reveals that Polsby is indeed correct.

Even with this theoretical disagreement, the book is well worth examining. This type of inductive research reveals numerous aspects of elections that are routinely missed by only examining voters.

PATRICK J. KENNEY

Wichita State University

Comparative and Other Area Studies

Against the State: Politics and Social Protest in Japan. By David E. Apter and Nagayo Sawa. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 271. \$22.50.)

Three basic themes are developed in this comprehensive and careful study of the protest movement against the building of the airport facility at Narita, Japan. The first and most extensively developed theme centers on the struggle itself. The confrontation at Sanrizuka over the building of a new airport facility is representative of many other protests. Protest in Japan is characterized by experience and ritualization. Not only is there ritualization in techniques used by protestors, but also by the government in response to protest. Eventually the government prevailed at Sanrizuka, but the protest continues in truncated form. The levels of participation in the early months was quite high, attracting a large number of diverse groups and individuals in support of a narrowly defined objective—opposition to the construction of the airport. In time, however, the movement became a catalyst for other protests of wider import. The building of the airport at Narita became the symbol of state power in its most overwhelming, expansive, and oppressive form.

The displacement of the farmers and the challenges to traditional values gradually became subordinated to other causes such as state imperialism, economic penetration of Japan into Southeast Asia, war, U.S. defense policy, pollu-

tion, education, human rights, and religion. As the causes became more diverse, polarization among the protestors occurred resulting in unity of purpose giving way to fragmentation.

The Narita protest, as presented in *Against the State*, provides considerable insight into the more negative side of the Japanese miracle. The social overhead costs of the protest have been great. Modernity is suddenly thrust into the quietude of traditional order and society. "In a world of households, farms, and other small-scale enterprises, the construction of any large facility intrudes. Social space is violated; livelihoods are affected" (p. 79). The disruptions have been significant. Politics as protest protrude into a patron-client political system. Women and children are politicized. In this sense the Narita protest is unique. Seldom is a protest of such magnitude found in the serenity of a traditional stronghold of the ruling Liberal-Democratic party.

A second, yet less developed, theme derives from the first. Questions are raised as to the role of violent protest in democratic societies, specifically in Japan. Citizen protest is endemic to Japan. Extra-institutional protest becomes very common, especially in the closed, political system of Japan. Japanese politics is not necessarily the politics of accommodation and compromise. As Apter and Sawa state, once deeply felt principles are invoked and become rigid, compromise and reconciliation are impossible. As the protest lengthened, the more fixed became the issue posi-

tions of the various participants. The Narita protest transcends Japan's movement from an industrial society to a postindustrial society. The evolution of the protest and the movement into a postindustrial era not only challenged the basic assumptions of democracy, but they also challenged the validity of the protest itself which became somewhat anachronistic in the process.

The final theme is really an implicit criticism of social science theories. These theories "omit what might be called the cognitive dimension of daily life and action in favor of overkill theories in which case materials become merely illustrative" (p. 13). No such problems arise in this study. The people, the protest, and the inevitable outcome take priority throughout. Not since George W. Packard's *Protest in Tokyo* (Princeton University Press, 1966) has there been a study so significant in its contributions to our understanding of protest politics in Japan.

L. JEROLD ADAMS

Central Missouri State University

Elections in Israel-1981. Edited by Asher Arian. (New Brunswick, N.J.: Transaction Books, 1983. Pp. 300. \$19.95, paper.)

In democratic societies, elections appropriately have had a privileged position as a topic of research. Elections embody philosophies of politics. They function as political institutions. They reflect social processes. They support economic interests. They emerge from particular histories and foster distinctive paths of development. Elections are studied both because they delineate the nature of a political community and because they appear to measure its well-being.

Although material that instantly clarifies these deep and fundamental concerns is not easily recoverable from election studies, such concerns cannot be properly addressed without uncovering the relationship of the election process to the democratic citizen and to the democratic political community. It behooves political scientists to examine elections with these issues in mind and with instruments capable of registering more than media coverage and tabulated returns. American political scientists have pioneered in creating sophisticated behavioral modes for explaining elections, and Israeli political scientists have excelled in applying these techniques. If the advantage of such techniques (the disposition to quantify) also constitutes their major deficiency (the avoidance of issues not subject to measurement), this has not been true for the particular case of Israeli election studies.

Asher Arian has coordinated a team of political scientists that has produced monographs on every national election in Israel since 1969, examining the campaigns and interpreting their results. The framework of analysis is American, but the flesh and bones are Israeli. Although not every issue central to Israeli political life is covered in the volumes, no one can understand Israeli politics without them. Taken together, they have become the analysis of record. Not all volumes address identical issues, nor do they all deploy the same techniques. The essays reflect not only growth in political science, but also the ways in which each historical moment can bring different issues into salience.

The Elections in Israel-1981 maintains the same high scholarly standard set in the previous volumes: Many of its essays are simply superb. Arian's introduction recreates the tone of the election and delineates its political and economic ambience. Michal Shamir, Asher Arian and Hannah Herzog, in two separate contributions, place the ethnic dimension in its proper perspective, noting that although ethnic political organizations are marginal in Israeli politics, "ethnicity" itself is an important political resource. With sensitivity and astuteness, Herzog writes that ethnic parties have always emphasized the temporary nature of their organizations, justifying them as the only means of redressing social injustice. She adds that many successful Israelis of non-European origin tend to discuss social and economic problems under the rubric of ethnicity. Avner Yaniv and Majid al-Haj have written an insightful overview of the patterns of Arab voting behavior. Giora Goldberg and Steven A. Hoffmann, expanding the latter's work on the process through which political party leaders are selected, have produced a comparison of the nominating processes employed by Israeli parties before the 1981 election. Notwithstanding the recent 1984 elections, this volume is still relevant and informative. Above all, regardless of scholarly approach or perspective, students of Israeli society will find this book indispensable.

DONNA ROBINSON DIVINE

Smith College

Venezuela: Politics in a Petroleum Republic. By David Eugene Blank. (New York: Praeger Publishers, 1984. Pp. xi + 225. \$27.95.)

Ironically, at a time when the focus of analysis of Latin American politics is shifting from the study of the nature and dynamic of authoritarianism toward the examination of the prospects of re-democratization, many still regard the Venezuelan democracy as either at the verge of collapse or

with its survival very much at stake. Blank's book is, in a way, an answer to this faulty perception. He "expects to see Venezuela's nonspectacular democracy survive the challenges of the 1980s" (p. 6), and does so happily, because "the worst democracy is preferable to the best dictatorship [and] Venezuela's democracy, with all its faults, is far from being the worst" (p. 10). This conclusion is the result of a solid analysis and his many years of incisive study of the Venezuelan political system.

Blank's objectives are more ambitious, however. He attempts to elaborate a systematic explanation of the Venezuelan polity based on the refinement of a scheme he developed in the early 1970s. His framework of interpretation is based on the assumption that three systems (planning, representative or populist democracy, and praetorianism) lie behind the Venezuelan political structure, each having a specific dynamic, occupying a distinct space, and meeting different demands. Although basically sound, there are some problems with Blank's framework. First, the praetorian system appears in clear decline, because political violence in Venezuela today has become a marginal phenomenon. Second, although the "planning" system may assume the form of a technical arena designed to deal with public policy problems in a nonpolitical fashion, it is the arena preferred by business elites, and not because of the "complex, technical reality of public policy making" (p. 7), but rather because business leaders can press their demands more effectively in the absence of the constraints associated with electoral politics. Thus, the existence of a dual policy arena (parliament/planning) reflects not the nature of the decisions (technical as opposed to political), but rather the uneasy but stable coexistence of two political logics: that of the general public, best represented by party and parliamentary politics, and that of the business elites, best served by ad hoc government-business committees. Finally, to label the third system populist is, at least, confusing. Parties in Venezuela act out of electoral considerations and develop extensive clienteles, but this is a characteristic of most representative democracies.

A study of the functioning of these three systems, or "impulses," this book is useful not only because of its attempt at developing a theoretical framework of understanding, but also because Blank is well informed and relies on many valuable but often neglected Venezuelan sources including magazines and newspapers. He updates the analysis of the political process up to the election of President Jaime Lusinchi in December, 1982 and includes two chapters dealing with the management of the oil industry. The chapter

covering the period that followed the 1976 nationalization is a welcome contribution to the study of a process that, despite its importance, has so far received only scant attention.

In brief, Blank has written a useful volume, a needed contribution to the understanding of the political system of Venezuela.

DIEGO ABENTE

Miami University

Parties and Democracy in Britain and America. Edited by Vernon Bogdanor. (New York: Praeger Publishers, 1984. Pp. xviii + 282. \$25.95.)

In this collection of essays by leading British and American political scientists on the respective countries' party systems, Bogdanor wishes to examine the role of political parties in the "world's two leading democracies" (p. ix). In his introduction, he characterizes the two countries as "exemplars" of two contrasting models of democratic government, parliamentary and presidential. Recent years, however, have seen the erosion of many of the conditions that historically have underpinned the stability of the respective systems. Consequently, the book's two main aims are to analyze how "the process of social and electoral change [has] altered the role of party in Britain and the United States, and how the British and American parties [have] reacted to the social forces that press upon them" (p. xi).

On this two-pronged score, however, *Parties and Democracy in Britain and America* has mixed results. At least one-half of the essays concentrate on descriptions of the first of these two aims, while not emphasizing the second, the relationship between social forces and political parties, strongly enough. As Bogdanor himself states, "Political parties are, to a considerable degree, dependent variables. Their power to influence the direction of social change and of electoral loyalties is, under modern conditions, severely limited" (p. xvii). Therefore, given this characterization of political parties, one would expect *Parties and Democracy in Britain and America* to offer a much stronger treatment of the interaction between dependent and independent variables in order to deal with the issue of causation. Unfortunately, Bogdanor has not taken this approach.

A number of essays fall into the first, or descriptive, camp. S.E. Finer's short chapter on the decline of party is primarily an extended outline that only hints at such important themes as the marginalization of the parties and the "departifying of public policy" (p. 2). Leon D. Epstein's chapter on the redistribution of power in British parties concentrates almost exclusively

on the Labour party, and only during 1980 and 1981. The chapters by Bogdanor and David Adamany on party financing offer a wealth of detail on the processes by which parties in the two countries finance themselves, but offer little in the way of prescriptions on how the respective disadvantages can be overcome. Finally, Larry Sabato's chapter on new campaign techniques and the American party system provides an extensive account of this phenomenon. It misses, however, an opportunity to address whether this new "style" of campaigning is at all helpful in getting at the substance of electoral politics.

The remainder of the essays deal more successfully with both of the book's aims. In a chapter titled "Power and the Parties: The United States," Philip M. Williams offers a nuanced and dynamic account of the erosion of the American parties. In a chapter on American antipartisanship, William Schneider locates its causes in the distrust of institutions and an American ideological belief system, both of which became more sharply drawn after the mid 1960s. David Butler and Austin Ranney give an enlightening account of the role of the media in both countries. And, in concluding essays, Bogdanor and Gerald Pomper hint that institutions that ensure stability during tranquil times (first-past-the-post single member constituencies, for example) may prove much less representative during times of turmoil.

Ultimately, *Parties and Democracy in Britain and America* remains uneven because too many of the essays fail to appreciate the magnitude of the recent changes that have affected both countries. In fact, these changes may call into question whether both still remain the same exemplars of democratic government that they once were during more tranquil periods.

CHRISTOPHER S. ALLEN

Harvard University

The State and Nuclear Power: Conflict and Control in the Western World. By J.A. Camilleri. (Seattle: University of Washington Press, 1984. Pp. xiv + 347. \$25.00.)

Avoiding the cases for and against nuclear power, this study analyzes the political, cultural, and international dimensions of civil nuclear energy since the atomic age began. Camilleri focuses on the mode, extent, and efficacy of the liberal capitalist state's part in nuclear decision making. To Camilleri, the entire nuclear experience has been one of increasing regime intervention, the state's emergence as an arena for nuclear disputes, a shift from U.S. hegemony to

imperial rivalry in nuclear markets, and a decline in capitalist policy effectiveness everywhere.

The state's use of "national interest" appeals and police powers is illustrated. The state was typically able to secure worker and union support for pro-nuclear policies in several countries. In effect, the workers supported their own interest, not the interest perceived by Marxian analysts to be the workers' interest. The bulk of the study focuses on Britain, the United States, France, West Germany, Sweden, and Brazil. A chapter is devoted to the adverse impact of the recession of the 1970s on the nuclear option. If the American state had the power attributed to it, the nuclear depression would not have become so severe. Also, if the oil problem had become worse, the nuclear option would probably be healthier today.

To limit his inquiry, Camilleri excluded the nuclear Marxist state. He comments that a comparison would probably reveal "a qualitative difference in the scope and mode of [state] intervention" (p. x). A comparative study would also presumably reveal that nuclear decision making is monolithic in Marxist states and pluralistic in capitalist states.

Notice is taken of the vital role of the ecological movement, especially in the United States, in challenging the nuclear preferences of industrial and governmental elites. Yet the decentralized nature of environmentalism is also stressed as a factor aiding the state's ability to cling to pro-nuclear policies despite setbacks such as the Three Mile Island accident. The state's policies included the Carter reversal of plutonium recycle followed by the Reagan reversion in principle to recycle orthodoxy.

International currents and nonproliferation issues are addressed in one chapter. Camilleri aptly finds that neither Carter nor Reagan was able to impede the decentralizing trend in world nuclear commerce found in the growth of new enrichment and reactor production facilities in Western Europe that have developed overcapacities. Much the same is true of proliferation-proneness. Yet no state has begun a nuclear-weapons program since the Non-Proliferation Treaty came into force in 1970.

Marshaling data from a variety of sources, Camilleri concludes persuasively that the anti-nuclear movement did not "succeed in disrupting the technical apparatus of the state, much less in undermining the normative integration of the social system" (p. 282). Although not mentioned, much of the protest in the United States at least appealed to basic norms. There may be a discontinuity between protesters in America and anti-nuclear groups in France and West Germany.

According to the book, nuclear controversy

"was the symptom of a deeper crisis, which itself reflected a contradiction between a capitalist system integrated by a single division of labour and a capitalist system divided between rival corporations and nation-states" (p. 293). These are declarative findings subject to ideo-analytical disputes. Although there is nothing preordained about the future of the less-than-ideal liberal capitalist state, it gives signs of not only surviving nuclear energy controversies however they turn out, but also of Marxist attempts to pour the history of the liberalist capitalist state into a mold of the kind this book posits.

PAUL F. POWER

University of Cincinnati

Local Politics and Development in the Middle East. Edited by Louis J. Cantori and Iliya Harik. (Boulder, Colo.: Westview Press, 1984. Pp. xii + 258. \$25.75.)

It may be accurate to assert that economic development has been uneven in the Middle East. In fact, over the past decade Arab states that do not produce oil experienced significant socio-economic hardships similar in nature to those of other developing countries. Much of these difficulties occurred because of poor planning at the state level and poor implementation at the local level. As a result, most of the assistance provided by central governments for the construction of industrial projects or the establishment of agricultural cooperatives lacked relevance to indigenous populations whose needs were not necessarily served. Perhaps more importantly, local leaders feared that government-sponsored activities would bypass the traditional decision-making process resulting in the erosion of "their" power base.

Based on extensive field research, the 11 essays assembled here attempt to discuss how local politics influence development in Egypt, Syria, Jordan, Lebanon, North Yemen, Tunisia, and Turkey. To a large extent, they succeed in describing the nature of the center-periphery issue in these countries, mostly by examining specific case studies at the local level. They also reveal a number of interesting contradictions in the relationship between local and central governments.

For example, Louis J. Cantori and Peter Benedict illustrate how limited the influence of local leaders may be in a blue-collar suburb of Cairo, while Iliya Harik and Nirvana Khadr discuss the relative socioeconomic freedoms enjoyed by Egyptian farmers and fishermen. In fact, at the urban level while local leaders affect the initiation and development of local politics, their in-

fluence does not seem to extend beyond the occasional mediation effort. At the rural level, on the other hand, by pursuing independent marketing strategies in their businesses, often contradicting government-dictated policies, it is revealed that local leaders succeed in persuading central authorities to adopt positions perceived to be more beneficial to the periphery. Yet, despite such accomplishments, rural dependence on the center's financial and technical support continues to guide local development strategies. This second point is best illustrated by Peter Gubser and Barbara K. Larson in discussing agricultural cooperative cases in Jordan and Tunisia.

In both instances, the technical support of an international organization and a central government was essential. Acknowledging that the required expertise for such ventures was beyond the means of the villagers, local leaders provided their assistance to facilitate the completion of the projects. But when the international agency withdrew its technical support from the Jordanian project, Amman could not shoulder the added burden by itself, and without the required technical backing, the cooperative failed. Larson, for her part, skillfully reports how village leaders in Tunisia offered their political support to the Destour party in exchange for technical and financial assistance. This arrangement proved quite fruitful for all involved because it guaranteed prosperity for the villagers and permitted the government to consolidate its regional power bases.

Richard Tutwiler describes a similar situation in his study on North Yemen, where village leaders persuaded the government to finance the construction of a road connecting Mawhit to the Sana'-Al-Hudayda highway. Interestingly, although Mawhit officials accepted the government's technical and financial cooperation, they initiated work on the project before arrangements could be finalized. This unorthodox development presented the Sana' government with a *fait accompli*, forcing officials to commit themselves to the project.

A third point of interest is the impact that education has had on the relationship between local politics and development. Raymond A. Hinnebusch, for example, looks at how higher literacy rates permitted the Ba'th party to enhance its political position in Syria. Reportedly, the party's elaborate patronage system permits Damascus to channel development funds to those areas of the country where party leaders exercise considerable leverage. Nevertheless, despite these visible signs of strength, local officials do not seem to exercise much influence at the national level. Increasing levels of literacy, however, raise critical questions for the future should the party suffer political upheavals.

Education also seems to have made a definite impact in Turkey. In the only study using extensive statistical data, Mubeccel Kiray documents how Taskopru officials banked on local know-how to enhance the area's economic situation.

Finally, Suad Joseph's discussion of local political influence in Borj Hammoud, a suburb of Beirut, illustrates how ethnicity in Lebanon further heightens the disparities between the haves and have-nots.

As Nicholas S. Hopkins points out in his introductory chapter, local leadership figures exercise considerable, albeit limited, influence on central governments in the Middle East. Yet, although it has indeed been possible for many states to initiate development projects, the political costs have been very heavy, particularly when self-imposed limitations curtailed anticipated results.

This highly recommendable book suffers from too many typographical errors that distract the reader from the interesting and provocative discussions. It is also regrettable that no maps are provided to locate the numerous villages referred to in the text. Although useful references are included in most studies, Tutwiler's essay lacks its final three footnotes.

JOSEPH A. KECHICHIAN

University of Virginia

The Roots of Begin's Success. Edited by Dan Caspi, Abraham Diskin, and Emanuel Gutmann. (New York: St. Martin's Press, 1984. Pp. iv + 297. \$25.00.)

Perhaps Menachem Begin waited longer than any other modern leader to become head of government. Not only was he the fiercest opponent of the 28 years of Labor party ascendancy in Israel (1948 to 1977), but he was also the most persevering. He joined, and then departed from, a wall-to-wall coalition (1967 to 1970). In the end, Begin became a remarkable testament to political democracy by patiently waiting three decades and nine national elections before finally assuming the prime ministership.

The mechanics of Begin's elevation are as interesting as his long career. He was regarded as an almost illegitimate figure (and sometimes an irrational one) by many of his contemporaries including David Ben-Gurion. But he also developed into a nonestablishment hero to the "second Israel," the non-European Jews who favored Begin's hard line (because they had been forcibly expelled from several Arab states) and his opposition to the Labor establishment (because it was primarily composed of European Jews). As these

non-European Jews became a numerical majority of Israel's electorate, they helped catapult Begin forces (the Likud) and allies into a parliamentary majority.

The ethnic and religious concerns that motivated Likud supporters to continue to support Begin and his party in 1981 were not dissimilar to those that brought Begin to power in 1977. The data in several of the chapters clearly demonstrate that Israeli Jews of Middle Eastern background still appreciated the Likud government's approach to hostile Arab governments, as illustrated by the Israeli raid on the Iraqi nuclear reactor in Baghdad shortly before the 1981 election.

Unfortunately, Begin's political philosophy, which apparently inspired the votes of hundreds of thousands of Israelis, is only briefly explored (for two pages) in the first chapter, "Critical Aspects of the Elections and Their Implications." This is a pity because such an inclusion would help to provide a thematic quality for the various well-written but somewhat isolated chapters; in this respect the book's title is somewhat misleading. Much of the material does indeed provide substantial explanation of Begin's political abilities. What is lacking is an appreciation of Begin's political personality. It is certainly a pertinent consideration to explore in view of the Likud's narrow victory in 1981. Begin, for example, was assumed in the months before the election to be apathetic and resigned to a Labor victory—an apathy that returned in 1983 when in the wake of the proximate deaths of close friends and his wife Begin's depression returned, and he quit politics altogether. When Begin revived in 1981 so did the Likud's electoral chances.

This volume includes a useful variety of 11 original essays by 12 Israeli scholars, three of whom are the book's co-editors. In themselves, the essays are of substantial worth to scholars whose research and/or teaching take them into the dynamics of Israeli politics. Collectively, the essays tend to explore the current status and the changing quality of Israeli politics. Ethnicity (chaps. 2 and 4), the religious variable (ch. 6), and the increasingly important and less predictable Arab vote (ch. 7) are among the crucial considerations in understanding the difficult time major parties have come to have with an increasingly unstable electorate.

In this respect, chapter 5 is of special import. Instead of realignment, "Polarization and Volatility among Voters" may best explain what is going on in Israeli politics. It does seem safe to conclude that political parties in Israel are dealing with a very fluid situation. Even the communists can no longer count on receiving at least half of the Arab vote.

Each chapter is a worthy example of exacting research. Several of the authors also contributed to *The Elections in Israel-1977* (Jerusalem: Academic Press, 1980) edited by Asher Arian. For that reason as well as the availability of updated material the present volume is an excellent supplement.

MARTIN SLANN

Clemson University

The Southern Cone: Realities of the Authoritarian State in South America. By Cesar Caviedes. (Totowa, N.J.: Rowman & Allanheld, Publishers, 1984. Pp. x + 212. \$34.95.)

This is an important interdisciplinary effort to synthesize the nature and prospects of authoritarian states in the Southern Cone, defined here to include southern Brazil as well as Argentina, Chile, and Uruguay. Caviedes is a professor of geography, which leads him to provide an interesting exposition of trends in physical and political geography as well as trends in classes and hierarchical institutions. His pleasant writing style reinforces his obvious competence in Southern Cone affairs.

Nonetheless, so much ground is covered from various disciplinary perspectives that the narrative does tend to devolve into a summary of the literature on different topics rather than providing a genuine interdisciplinary synthesis. Examples of topics discussed that do not go beyond the existing literature are the role of the contemporary church in the Southern Cone states, the role of unions, the tendency towards centralization, and the impact of the national security doctrine and geopolitics.

A related problem is that each of the four main chapters examines the Southern Cone authoritarian states from a different perspective without any clear theoretical unity between them. The Conclusion might have provided a synthesis, but instead is limited to an impressionistic prognosis of military rule and is the weakest part of the book.

Nor does the book's central theme—the durability and pervasiveness of the authoritarian state as reinforced by militarism—ever adequately explain recurring pressure towards democracy in the Southern Cone. Only in the last few pages of the Conclusion are the Brazilian political *abertura* (opening) and the new democratic government in Argentina finally mentioned. Because the book does purport to explain the nature of political realities in the Southern Cone states, this is a serious defect.

The treatment of Argentina is also less than satisfactory. What have become standard explanations of the alleged innate aggressiveness of the Argentine military are repeated here, whether expressed through an unprovoked attack in 1982 on the Falklands/Malvinas islands or through bullying of Chile over much of the last decade on the Beagle Channel issue. Of course, the record of the Argentine military has been severely marred on these as on other matters, but the reader is disturbed that a supposedly impartial analysis of regional militarism practically acquires an anti-Argentine bias. For example, the Argentine cases regarding the Falklands/Malvinas and Beagle Channel issues are not mentioned, although Argentine conduct in each case is roundly criticized.

The treatment of Brazil poses other problems. Caviedes does present a cogent case for including southern Brazil as part of the Southern Cone, but Brazil is generally given short shrift in the analysis. This is particularly troublesome because the implications of Brazil's emergence as the leading state in the region are not carefully explored.

Although these problems detract from the overall impact of the book, Caviedes has gone well beyond previous studies in presenting an interdisciplinary analysis of Southern Cone political trends. It is hoped that others will follow his example.

MICHAEL A. MORRIS

Clemson University

Political Cohesion in a Fragile Mosaic: The Yugoslav Experience. By Lenard Cohen and Paul Warwick. (Boulder, Colo.: Westview Press, 1983. Pp. 186. \$18.00, paper.)

In attacking a perennially explored topic, the viability of multinational Yugoslavia and its continued cohesiveness, Cohen and Warwick have pooled their expertise in Communist political systems and in legislative behavior respectively to develop an eclectic approach complete with sophisticated methodology.

First, they deal with the country's entire 65-year existence as an independent state and utilize the concept of a "single entity" (p. xi) and system to bridge and transcend changes in regime, ideology, and the various strategies to manage cultural diversity worked out by nonsocialist and socialist elites. Second, the authors focus upon upper-level legislative elections and voting behavior, primarily as sources of a quantitative data base, but also as phenomena worth considering in themselves. Last, they attempt to apply the "entropy measurement" developed by Stephen Coleman in *The*

Measurement and Analysis of Political Systems (Wiley-Interscience, 1975) by utilizing data on voter choice and turnout in order to "assess the degree of political incorporation of the various geographical units of the political system and its likelihood of survival" (p. 4). If a state is well-integrated, its "entropy" levels throughout its territory should be uniform, or at least harmonically distributed. The results of their statistical tests and schematic analyses are combined in a final, largely qualitative, chapter that brings the Yugoslav experience up to date. The latest quantitative data used relate to 1969 federal and republican elections.

Cohen and Warwick claim that the "entropy hypothesis" is most useful in analyzing electoral behavior in the kingdom between world wars. Nonetheless, conclusions reached do not appear particularly startling. By the 1920s, "the Yugoslav polity . . . was demonstrating an increasing resemblance to a collection of distinct subsystems" (p. 37); moreover, the more restricted elections held during the following decade "did not have the intended effect of decreasing the centrifugal tendencies in the country" (p. 46). The postwar Titoist regime, however, managed to induce a "process of convergence" enabling "national stimuli such as the purging of the secret police . . . [to] produce a common response across all regions of the country" (p. 87).

More provocative are results obtained when ballot invalidation is used as a measure of dissent: Increased candidate choice during the "liberal era" from 1965 to 1969 did have the effect of checking the percentage of invalidation wherever choice was provided, although this option was increasingly used wherever choice had been reduced or remained nonexistent. The authors note that the elections of 1969 marked "the end, rather than the beginning of pluralistic socialism" (p. 98); elections held under the delegate system in 1974 and thereafter were controlled by the regime and "safe" (p. 15).

One might doubt the implied contention that mass electoral behavior during the 1960s, and further expectations "for a wider expression of preferences that would probably not be compatible with a one party state" (p. 143), were major reasons behind the reassertion of party control over the electoral process. As Cohen and Warwick point out, the repoliticization of the "national question" as evidenced through Serbo-Croatian language disputes, revolt in Kosovo Metohija, enlarged autonomy for the Republican parties resulting in federal governmental stalemates, and Croatian student unrest, supplied ample justification for the leadership's fears that liberalization had gone too far.

In socialist and nbsocialist Yugoslavia, elec-

tions and the corresponding selection of upper-level legislative bodies have rarely been central in one determination of policy outcomes. Real political power has traditionally been exercised, if not at present by professional bureaucratic elites serving on federal executive and administrative bodies and their republican counterparts, then by partisan professionals ensconced in the higher party. An analysis of the operations of these institutions might therefore be more useful in determining whether the country can be governed as an integrated whole. In socialist Yugoslavia, latitude for autonomous "legislative" activity has been greatest at grass-roots levels such as in the communes and self-managing economic enterprises. The authors do not study these arenas. Furthermore, because they are concerned only with statistically measureable aggregates, Cohen and Warwick are unable to explore the particular dynamics of the electoral contests during the 1960s that were sharply competitive and/or resulted in the election of maverick deputies.

Nonetheless, the terse, historical, and objective commentary with which the authors surround their statistical analyses demonstrates the difficulties encountered by Yugoslav elites during the course of nation-building. Particularly useful is Cohen and Warwick's exposition of the CPY's labile nationalities policy and their attempt to match cultural diversity management strategies with particular eras of the Yugoslav example.

Empiricists might be attracted to this book by the authors' attempt to apply sophisticated quantitative techniques to the study of a multicultural and presently socialist system in development. Others, however, might find the book's qualitative parts more cogent and successful than the whole.

KAREN ROSENBLUM-CALE

Mount Holyoke College

The Independence Movement in Quebec 1945-1980. By William D. Coleman. (Toronto: University of Toronto Press, 1984. Pp. xii + 274. \$30.00, cloth; \$12.95, paper.)

This unpretentious book provides a new view on Quebec's Quiet Revolution and the ensuing rise of an independence movement by relating the provincial government's economic policies to its cultural policies. According to Coleman, the Quiet Revolution resulted from a coalition of forces distressed, after World War II, by the gap between traditional French-Canadian institutions and the demands of an increasingly industrialized economy. Before the Quiet Revolution, the provincial government had not attempted large-scale

economic planning, and educational and social-welfare institutions had been controlled by the Catholic Church. Faced with the continuing economic dependence of French Canada vis-à-vis anglophone North America, coupled with the inability of the Church to respond to the social problems accompanying industrialization, francophone businessmen, organized labor, and elements of the intelligentsia united to demand increased governmental activity in economic planning and social services. The Quebec Liberal party, which came to power in 1960, represented the shared interests of these "class fractions" (p. 111) and implemented programs intended "to create viable and competitive capitalist enterprises, controlled by members of the francophone community" (p. 92). Assessing Quebec's economic policies between 1960 and 1980, Coleman argues that the government has been only partially successful in increasing the economic clout of French-speaking Quebecois; nonetheless, the result of its attempts has been to bring Quebec society into the North American mainstream.

This assessment leads to Coleman's most interesting hypothesis: "the more the francophone community becomes integrated into the North American economy at the level of management and of consumption, the more difficulty it will experience in defining for itself and maintaining a distinctive culture" (p. 132). In several comprehensive chapters on indigenous philosophies of culture and their relationship to cultural policy-making, Coleman argues that government programs in these areas have undermined rather than strengthened Quebec's cultural distinctiveness. For example, educational reform destroyed the province's religiously based educational system (which had been unique in North America) and replaced it with one focused on the technical skills needed by citizens of advanced industrial economies. And language legislation, which has made French the primary language of business in Quebec, has facilitated the integration of francophones into a world that has nothing to do with traditional French-Canadian values. In sum, seemingly nationalistic cultural policies have complemented integrative economic policies and hastened the demise of a distinct Quebec culture, rather than creating the institutional structures necessary to protect it.

Coleman's book is useful as an analysis of the recent history of cultural policies in Quebec, but it has two theoretical failings. First, Coleman's discussion of class promises much but delivers little. At the outset he rightly criticizes theories that explain the Quiet Revolution and separatism in terms of the interests of a specific class; Coleman argues that these social movements sprang from coalitions of classes and subclasses. Yet he is as

uncritical as those he attacks when it comes to identifying relevant classes: He assumes that the ideological texts he analyzes represent discrete classes, but never gives evidence to show how he has correlated text and social grouping. In the absence of such arguments, one could use Coleman's data to construct a history of class conflict that would differ from that proposed by him. Second, Coleman is never clear in his use of the notion of cultural distinctiveness. At times he writes as if a distinctive traditional culture once existed but has now been destroyed; yet elsewhere he speaks of the "intellectualized" vision of a French-Canadian culture that clerical elites "felt" to be authentic (pp. 46, 157). In sum, Coleman points out the difficulties of legislating authenticity, but he uncritically uses a notion of authenticity as a baseline for the study of social change.

RICHARD HANDLER

Lake Forest College

Democratic Theory and Practice. Edited by Graeme Duncan. (New York: Cambridge University Press, 1983. Pp. 262. \$39.50, cloth; \$10.95, paper.)

This book contains 16 essays on the theory and practice of democracy in contemporary political life. Most of the contributors would agree that democracy has fallen on hard times. The frontiers of democratic politics are contracting, the rate of political participation is waning, and most citizens are neither acutely informed nor highly interested in political affairs. It is Duncan's hope that the essays in this volume will offer a basis "for a more hopeful picture of democratic futures than much of modern life seems to warrant" (p. 12). Despite the high quality of several of the contributions, the hope remains a modest one.

The book has several themes that bind the essays together. First, essays on Mill and Rousseau, Madison and Tocqueville, Marcuse, and Marx, make it quite clear—if it ever was in doubt—that what is referred to as "classical democratic theory" is subject to various and varied expressions.

Second, the contributors urge readers to reconceptualize democracy in light of changing political practices. The operative assumption seems to be that our moral and political concepts are basic constituents of our moral and political practices, and as our practices change, our concepts must be revised. Thus Dennis Thompson's essay on bureaucracy and democracy offers some guidelines to enable democratic theorists to explain what none has yet adequately explained: how democratic politics can be reconciled with the im-

peratives of modern bureaucracy. And Paul Nurse-Bray maintains that neither liberal nor Marxist perspectives are sufficient for an understanding of African democracy. African one-party democracy is a peculiar product of African social and political experience and, as such, it is a valid contribution to "a specifically African vision regarding political life and the processes of government" (p. 111).

Third, the contributors seek to broaden our conception of politics and to enlarge our vision of democratic possibilities. Duncan's essay on human nature and radical democracy illustrates this theme. Duncan suggests (p. 197) that "people have significant and unrealized potentialities, and could be more than they are with more favorable conditions (more or better material resources, education, information, and the like)." Carole Pateman's commentary on feminism and democracy is an effort to place such issues as the relations between the sexes, the problems of the family, and the status of women in the economy at the center of debates about modern democratic practices.

So, as we might anticipate from thinkers broadly sympathetic to participatory models of democracy, there is a general dissatisfaction expressed with liberal democratic concepts and categories. But the criticism is by no means limited to liberal democracy. Indeed, one of the strengths of this book is its all too rare scholarly self-criticism. Paul Corcoran, for example, reminds readers that democracy has not, at least until recently, been considered a praiseworthy form of government. Corcoran thinks that the perspective of 25 hundred years of Western political thought cannot be casually dismissed. He thus proceeds to question directly one of the volume's primary objectives (e.g., to provide a basis for democratic futures) by asking, "What if the modern democratic tradition in political theory is fundamentally *wrong*, both in its presuppositions about social and political values and in its labors to purify democratic theory and apply it in social practice?" (p. 15).

In a similarly self-critical spirit, Pateman singles out participatory democrats (including herself, as represented by an earlier work) for special criticism. She finds it somewhat ironical that proponents of a democratic model that takes into account social inequalities would have so systematically overlooked the domination characteristic of sexual relationships. She adds, "the present volume at least acknowledges that feminism might have something significant to say to democratic theorists or citizens, albeit in a token paper by a token woman writer" (p. 204).

This volume successfully identifies major issues demanding the attention of contemporary demo-

cratic theorists. And if one is not left hopeful about the democratic future, neither is one given grounds for total despair. For as Henry Kariel observes in the volume's closing paper, we "can still use what wit we have to engage in enterprises like the one engaging me at this very moment . . . to keep *enacting* the theory of democracy during times that deny this possibility" (p. 262).

GREGORY LEYH

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Constitutional Democracy: Essays in Comparative Politics. Edited by Fred Eidlin. (Boulder, Colo.: Westview Press, 1983. Pp. viii + 516. \$25.00, paper.)

This extremely broad-ranging collection of essays is a *Festschrift* in honor of Henry W. Ehrmann. Five of the 23 essays deal with the role of interest groups in constitutional democracies, the area which marks Ehrmann's major contribution to the discipline. He also maintained a strong interest in constitutional law, which is represented in four essays. The remaining 14 contributions deal with topics as diverse as rational choice and culture (chap. 9), politics and faith (chap. 6), voting behavior (chaps. 14 and 15), and the problem of transition from authoritarian regimes (chap. 23). This variety results from the editor's selection of friends, colleagues, and students of Ehrmann as contributors. Coming from the United States, France, Germany, and Canada, they represent the countries in which he has lived and worked.

Whatever one may think of this approach, its result is a work that exemplifies the broad range of political research, excluding, however, examples of more methodologically oriented research like formal modeling or positive theory. Francois Goguel's essay on the evolution of the institution of the French presidency (chap. 2) combines analysis of constitutional provisions, historical events, and personal observations to provide a rich picture of an institution and the leaders who shaped it to achieve their ends. Roger D. Masters (chap. 5) draws on etiological research to assess the validity of Rousseau's psychology. Fred Eidlin's contribution attempts to resolve the controversy about the scientific status of area studies. These are just a few examples of the different variants of political science covered in this volume.

The most exciting essays are those focusing on interest groups. Wolfgang Hirsch-Weber's comparative study of labor unions in Britain, the United States, and West Germany (chap. 16) examines relationships between membership, structure, and strike behavior. One wishes that an ex-

ample of the quantitative research approach focusing on these questions (i.e., Douglas Hibbs) could have been included to clarify the interesting hypotheses spelled out by Hirsch-Weber. Kay Lawson (chap. 18) elaborates the articulation of corporatist themes and policies under different presidents during the Fifth Republic. William Safran's study of interest groups in France, West Germany, and the United States (chap. 17) analyzes the relationship between interest groups and the state. These essays (and a few others) cohere in a way that a large part of the book does not: They share a focus on interest groups, their relations with each other, and with the state. They analyze those relationships in a particular context, and they contribute to an ongoing debate about the usefulness of concepts like corporatism or neocorporatism for the study of interest groups in postindustrial societies.

Although the norms regarding the format of a *Festschrift* seem to be fluid, one might expect that its contributors would focus on the opus of the honored scholar. Instead, Eidlin has chosen an approach that covers a good deal of the intellectual territory claimed by political scientists. Only four contributors even quote from Ehrmann's works, and even fewer reflect on its continuing relevance. Ehrmann's contribution to our discipline could have been acknowledged in a more meaningful way than by the ill-defined selection process represented in this volume.

JUTTA A. HELM

Western Illinois University

Local Organizations: Intermediaries in Rural Development. By Milton J. Esman and Norman T. Uphoff. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 391. \$35.00.)

In the comparative and development administration literature, Esman and Uphoff's analysis of the role of community-based organizations in the development process clearly stands out as one of the most significant studies published to date. Drawing on more than a decade of field work, the authors survey systematically the universe of local organizations in Asia, Africa, and Latin America. They also submit to rigorous analysis the factors that explain why such third-sector organizations as cooperatives, clubs, interest groups, and citizen associations constitute a crucial ingredient in explaining those instances where success characterizes rural development programs. As a consequence, their book brings together theory and practice far more successfully than do most other studies in this field, since the quest first began for works in public administration, more

attuned to third-world realities, which would transcend the limits of single-country case studies. At the same time, it meets the standards of quality work in general political science and draws upon the best traditions of empirical research in the social sciences.

Beginning with the findings of their 1974 study, Esman and Uphoff establish at the outset the premise that where local organizations involving the rural poor exist, they fill a vital function as intermediaries between state and society. Through their review of the development literature, they clarify why participatory local organizations constitute a crucial part of rural development programs. They also state straightforwardly their own position as advocates of a "structural-reformist" approach to the problems of underdevelopment. From there, Esman and Uphoff move on to define comprehensively the types and tasks of local organizations. They then proceed to a quantitative assessment of those indicators that best explain economic, social, and political gains in rural communities. To accomplish this they select 150 cases from the available literature; this data set, once converted from isolated qualitative assessments into uniform quantitative measures, is then submitted to correlational and multiple regression analysis.

Esman and Uphoff's most significant finding concerns the importance of internal structural variables as the primary source of successful program performance and the relative insignificance of exogenous or environmental variables linked to more accepted theories of development. These data are evaluated in terms of their capacity to account for the difference between success and failure in sustained development activities at the community level. With this finding in mind, and after detailed discussion of its implications for development strategies, the authors next consider questions of organizational strategy—those actions that most hinder or assist rural residents in their ability to establish, maintain, and promote effectiveness in local organizations serving the poor. This portion of the study embraces chapters on the vulnerabilities with which local, rural organizations must contend; the sources of improved organizational performance; the strategies that have produced the greatest results; and those external contributions that have had the greatest impact in helping to improve the performance of community-based organizations.

In the process of moving from general development theory to measurement of those variables most conducive to explaining developmental results to concrete questions of alternative strategies, the reader is exposed to the complexities of the development process at the local level with a concreteness that is generally missing in the

whole of the comparative politics literature on developing countries. At all levels of their analysis, Esman and Uphoff provide excellent insight into the intricacies of the development process through the eyes of local organizations and from the vantage point of the rural poor. In so doing, they present a convincing argument for using grass-roots, participatory approaches to promote social change. *Local Organizations: Intermediaries in Rural Development* is, in short, a superb study in design, in comprehensiveness, in relevance to core developmental issues, and in linking sophisticated analysis of a complex set of variables to concrete discussion of alternative strategies designed to produce results at the grass-roots level.

LAWRENCE S. GRAHAM

University of Texas at Austin

Bureaucracy and Democracy: A Political Dilemma. By Eva Etzioni-Halevy. (Boston: Routledge & Kegan Paul, 1983. Pp. xi + 266. \$29.95.)

The uneasy relationship between bureaucracy and democracy has long been a central theme of Western administrative theory. A large, expansive literature on administrative responsibility and accountability has its roots in this most basic of governmental concerns. Thus, one approaches a new book on this subject with great anticipation, hoping for an injection of clarity into a continuing and frustrating debate over the proper role of administration in modern democracies. This anticipation is well, but partially, met by Etzioni-Halevy. That this subject is both complex and paradoxical, and therefore makes the author's job well-nigh impossible, is clear. Many have been, and will be, daunted by a task that resembles an academic Gordian knot.

This interesting volume attempts to defend a common thesis: "bureaucracy is necessary for democracy, yet is also a constant source of tension, friction and conflict in it" (p. 2). This is indeed the bureaucracy-democracy paradox known to most who teach or research in this area. However, the book's utility derives from the detailed analysis that it gives to a problem that is normally subjected to article or chapter-length treatment. Rather than treating the topic as merely the context for the discussion of a wider range of administrative theory concerns, Etzioni-Halevy has elevated the study of the bureaucracy-democracy paradox to a more central and vital place in the literature.

The first half of the book provides a review of a

number of theoretical perspectives, such as those held by Weber, Marx, Mosca, and the pluralists. The most interesting and least conventional of these reviews are those dealing with the technocratic and corporatist views of bureaucracy in the democratic state. The drawing out of this view from a body of corporatist literature, which tends to neglect the special role of the bureaucracy, is particularly useful. Part 1 terminates with a chapter titled "Bureaucratic Power—Democratic Dilemma," which provides both a fitting conclusion to the arguments of the first six chapters and the theses to be examined in the second half of the book: that bureaucracy generates a dilemma for democracy, that democracy generates a dilemma for bureaucracy, and that these dilemmas create strains and power struggles on the political scene (p. 87).

Part 2, "Empirical Perspectives," uses essentially a qualitative analysis to substantiate the basic theses. This effort, even when combined with modest quantitative analysis, is only a mixed success. Although Etzioni-Halevy capably elaborates such familiar themes as the proliferation and growth of bureaucracy and the countervailing influence of the New Right, the net effect of chapters 8 and 9 is ambiguous. For example, the notion that the New Right's campaign against bureaucratic growth has been ineffective (p. 143) is not clearly substantiated by the author's data. This is largely because the effect of the New Right can only be seen clearly in data from 1980 onward. Where such contemporary data are shown by the author (e.g., pp. 137 and 139), they seem to support the argument that bureaucracy is contracting, if only slightly. In other words, Etzioni-Halevy's conclusion on this point may be simply and—for those who support government's social-service role—unfortunately premature. Current developments in the United States, Britain, and British Columbia seem to contradict her position.

Similarly, Etzioni-Halevy's criticism of Marxist theory as ignoring the rift between the bureaucracy and elected politicians (e.g., both share the same values as part of the ruling bourgeoisie) is correct but somewhat overdrawn. The effectiveness of her argument is blunted by her failure to challenge the standard Marxist assertion that disagreements, like those illustrated in chapter 13, are minor spats among "family."

Finally, the conclusion is unnecessarily limp. This is partly due to Etzioni-Halevy's unwillingness to specify possible actions or solutions. Surely there are, for example, institutional arrangements that might assist in keeping the bureaucracy-democracy conflict within reasonable limits. Or failing that, there may be other helpful methods such as the embodiment of a

British-style neutrality among civil servants. Without such suggestions, readers—especially advanced students—are left in the air. Nevertheless, the book is a solid theoretical contribution, even if it borders on the ethereal. One can only hope that the author's considerable talent will move in a slightly more pragmatic direction in the future.

KENNETH M. GIBBONS

University of Winnipeg

Soviet Jewry in the Decisive Decade, 1971-1980.

Edited by Robert O. Freedman. (Durham, N.C.: Duke University Press, 1984. Pp. xvi + 167. \$34.75.)

One of the most significant occurrences in Soviet politics and foreign policy since World War II was the exodus of a quarter-million Jews from the USSR from 1971 to 1980. The emigration of Jews became a point of contention between the United States and the Soviet governments. It also represented a serious brain drain for the Soviet economy. The departure of one-tenth of the Jewish population of the Soviet Union during the decade was also unprecedented in Soviet nationality policy, raising concerns among other nationalities about their right to emigrate and prompting a backlash against Jews remaining in the USSR. Finally, the exodus also provided Western observers with the opportunity to scrutinize various facets of Soviet life through the accounts of these former Soviet citizens.

Yet, despite its significance, the emigration of Jews from the USSR has not been adequately examined by Western scholars. This study, the result of a conference held in 1981 at Baltimore Hebrew College, provides the most detailed analysis to date of the phenomenon of Jewish emigration from the Soviet Union and the process of resettlement in Israel and the United States.

In the past there has been a tendency to view the emigration of Soviet Jews as a relatively uniform phenomenon that only varied over the decade in terms of the numbers allowed to emigrate. However, in Jerome Gilson's excellent overview, we see at least two distinct waves of emigration, one from 1971 through 1973, and the other from 1974 to 1980. The first group of emigres tended to come from Central Asia and the Caucasus, were motivated by Zionist sentiments and chose to resettle in Israel. In contrast, emigres leaving the Soviet Union after 1973 tended to come from the Ukraine, the Baltic republics, and urban centers of central Russia, were motivated by the deteriorating quality of life in the USSR compounded by a mounting anti-Semitic campaign and chose to settle in the United States. The latter

group appears to have a higher level of education, but may not necessarily have an easier process of acculturation to Western society.

Jerry Goodman explores trends in the treatment of *refuseniks*—those Jews who are denied exit visas by Soviet authorities. Here too we see dramatic differences among republics and even among different cities in the same republic. Goodman attributes these disparities to the varying dispositions of local authorities, who appear to enjoy some degree of discretion in awarding exit visas.

According to William Korey, one of the chief causes of Jewish emigration from the USSR during the last half of the 1970s was a rising tide of anti-Semitism. He notes that oral entrance examinations to the major universities appeared to exclude Jews. Officially sanctioned publications, which depicted Zionists as traitors and repeated racial stereotypes, flourished. The increased anti-Semitism among average Russians may have derived in part from resentment that the Jews were being allowed to emigrate while other nationalities did not enjoy that option.

Robert Freedman provides a comprehensive analysis of the place of the Jewish issue in Soviet-American relations. He demonstrates that since the Stalin era, Soviet leaders have been sensitive to Western pressure for easing Jewish emigration. Freedman's conclusions are reinforced by Gilson's finding of a positive 0.62 correlation coefficient between the volume of U.S.-Soviet trade and the annual number of Jewish emigrants during the period from 1969 to 1980.

The second half of the volume concerns the problems of absorption of Jews. Theodore Freidgut's chapter on acculturation to Israeli society shows marked contrasts to those offered by Zvi Gitelman and Ilkya Levkov in their studies of acculturation of Soviet Jews in the United States.

This is an important and balanced study of a complex social and political phenomenon.

GORDON B. SMITH

University of South Carolina

Political Repression in Nineteenth Century

Europe. By Robert J. Goldstein. (Totowa, N.J.: Barnes & Noble Books, 1983. Pp. xv + 400. \$27.95.)

Echoing the general expectations of the age, the early nineteenth-century Greek liberal Adamantios Korais described his time as "the century of the liberation of nations." Goldstein's book comes to alert us to the fact that the nineteenth century was as much a period of repression as it

was an age of expanding freedom and civil liberties. The author's main thesis is that political repression, especially in the form of severe suffrage discrimination, was one of the major forces shaping the political development of nineteenth-century Europe. Through a wide-ranging survey of historical trends and modes of political behavior, the narrative attempts to show the extent to which political repression determined the character of political and social change as well as the policy orientations of European governments.

The first part of the book discusses a variety of techniques of repression. Besides suffrage discrimination for lower and upper legislative chambers and local elections, they include more direct measures such as oppression of the press, restrictions on freedom of assembly and association, academic freedom, and labor unionism. As Goldstein reminds us, European governments did not hesitate to resort to more drastic means as well like the use of violence against dissidents and the employment of "dirty tricks" by secret police and *agents provocateurs*.

The second part of the book presents a political history of repression in nineteenth-century Europe. Goldstein organizes his narrative around a convincing periodization in three phases (1815 to 1850, 1850 to 1870, and 1870 to 1914) according to the character of policies followed by European governments in facing the pressures of political change.

One of the most attractive aspects of the book is the extension of its purview beyond the four or five major European countries to consider developments in the historical evolution of smaller nations. This enriches significantly the empirical basis of the argument and allows Goldstein to trace overall trends and commonalities without losing sight of fundamental regional specificities and differences. Exclusive reliance on secondary sources in English, however, leads to a certain degree of schematization. Furthermore, the analytical weakness of the book leaves the reader with an inescapable sense of being bombarded by "news from everywhere." This weakness is characterized by the bibliography's absence of theoretical sources and analytical studies of political change. Thus, some important insights, such as the suggestion that twentieth-century authoritarianism and totalitarianism can be connected with nineteenth-century repressive antecedents in certain regions of Europe, are not exploited as they might have been. Instead of pursuing these theoretical possibilities, the book remains within the confines of an anecdotal approach to political life and historical change. Where this can lead is illustrated by the surprisingly rosy description of the prison experience of some famous dissidents, which is obviously in-

compatible with the thrust of the general argument. Thus, the convincingness of perhaps the most intriguing, and potentially one of the most important, chapters of the book is considerably diminished: Instead of offering a sense of repression as social and human experience, it simply recounts some picturesque anecdotes. A parallel observation can be made about the neglect of ideological and cultural reactions to political repression, which formed such an important part of European civilization in the nineteenth century. Consequently, the promise of a historically based political science, which is hinted at by the subject matter and structure of the book, remains unfulfilled in this particular contribution.

PASCHALIS M. KITROMILIDES

University of Athens

Groups and Politics in the People's Republic of China. Edited by David S.G. Goodman. (Armonk, N.Y.: M.E. Sharp, 1984. Pp. vi + 217. \$25.00.)

This is an effort to apply the interest group model to the study of Chinese politics. The volume is divided into chapters that employ social and occupational groupings as the units of analysis. There are seven chapters on various groups—economists (Barbara Krug), provincial first secretaries (David S.G. Goodman), the military (Gerald Segal), teachers (Gordon White), peasants (John P. Burns), workers (Tony Saich), and intellectuals (James Cotton). Goodman, Peter Ferdinand, and Michael Waller discuss the group perspective and define interest groups in the Chinese context. Jurgen Domes attempts to improve the understanding of factions by providing an elaborate typology of "intra-elite conflict."

The "group" chapters provide few new insights or fresh empirical material, but on the whole they are solid and useful descriptions of these segments of Chinese society. The book as a whole is not consistently analytical, nor does it offer much that is new theoretically. The clearest point to emerge from the book is the insistence that notions of pluralism and lobbying be divorced from the idea of group politics when speaking of a communist setting. The authors try to address two major problems of group theory as applied to communist systems: how to define a group, and whether the presumably shared group interests are really pursued through collective group action. To get around the problem of defining groups that genuinely share interests, the authors make finer gradations in the defined group. They simply use smaller segments as their unit of analysis: schools

of thought among economists, branches of the military, peasants who live in different communes and production teams or who are members of different lineages, and teachers at different levels of the educational system. These smaller groups may be more uniform in their presumed interests, but we still do not know if people within these groups act collectively to pursue a common interest. These authors do what Griffiths cautioned against long ago: They reify the group.

Interestingly, with the exception of Domes who examines elite factions, none of the authors seems confident that their group(s) (broadly defined) act politically as groups. What the authors describe is what Franklyn Griffiths called "tendencies of articulation"—not group politics, but "countless parallel individual articulations." Segal is the one author to address the analytical question directly, and he concludes that "the group politics approach confuses more than it clarifies" (p. 98). Yet almost all the authors portray their groups as articulating interests insofar as they have support from some leadership faction. That factional politics at the top allows groups the opportunity to pursue their interests is the main theme that emerges from the volume and is at the center of its perspective on group politics. This is not the conceptual advance that a number of the authors claim, but a reiteration of the dominant group model employed in the China field since the beginning of the Cultural Revolution.

What is most unsatisfying about the book is its limited conception of politics. It focuses almost exclusively on articulation and influence on policy-making. Most of the essays simply describe the ways in which the various "groups" are *allowed* to articulate their interests through approved channels, along with some speculation about the success that groups have in the pursuit of their interests, which in some instances are only imputed. It is unfortunate, especially in light of the authors' own sensitivities to the limits of interest articulation, that most of the essays end rather than begin with the finding that interest articulation is limited by elite-level factional support. The chapters could have benefitted from the large comparative literature on the importance of participation at the stage of policy implementation in countries with poorly developed pluralist institutions. That interests may be pursued through individual strategies, personal ties, and corruption receives little consideration, except in Burns's essay. Perhaps this is an instance in which the group model restricts our vision of politics.

In the final analysis, this book does not grapple with the difficult criticisms that have been raised about the group model, beginning with *Interest Groups in Soviet Politics* (Princeton University Press, 1971) by H. Gordon Skilling and Franklyn

Griffiths. The book thus lags a decade behind the comparative politics literature. But more surprisingly, the volume has benefitted surprisingly little from the mainstream of China research on the topic. Goodman claims that the collection fills a large gap and thus will advance China studies. This statement fails to acknowledge the past efforts of China scholars who have used the group perspective in studies of participation and interest articulation, including the past work of Burns and White. Although it is true there has been no volume on China comparable to Skilling and Griffiths', it is far from the case that "most approaches to the study of politics in the PRC . . . rest on the vestigial totalitarian vision which has changed little since before the Cultural Revolution" (p. 5).

JEAN C. OI

Lehigh University

Unions and Economic Crisis: Britain, West Germany and Sweden. By Peter Gourevitch et al. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. viii + 394. \$30.00.)

Unions and Economic Crisis is the second of two volumes sponsored by the Harvard Center for European Studies on the response of European trade unions to the economic crisis of the 1970s. The first volume, which was published in 1982, examined how trade unions in France and Italy interpreted the economic crisis and what programs and strategies they adopted in response to it. The present volume asks the same questions as the first, only for trade unions from the northern, social democratic tier of Western Europe. Stephen Bornstein and Peter Gourevitch analyze the response of British trade unions to economic decline, Andrei S. Markovitz and Christopher S. Allen review the German case, and Andrew Martin examines the reactions of Swedish trade unions. A concluding chapter by George Ross and Gourevitch summarizes these case studies and probes the future of European trade unions in a period of challenge and uncertainty.

The case studies provide a detailed description of trade union activity over the entire postwar period, and not simply for the 1970s when economic growth ended. They trace the rise and decline of the postwar settlement between unions and social democratic parties in which the unions offered parties political support in exchange for "aggressive demand management on behalf of full employment, social welfare programs, and union voice in party and governmental decision making" (p. 366).

In their chapter on Britain, Bornstein and

Gourevitch claim that the behavior of the Trades Union Council (TUC) after the war was shaped by voluntarist traditions and Depression memories. They argue that the TUC wanted the Labour party to adopt Keynesianism, and thus create favorable conditions for the unions to exert their market power in collective bargaining. But the weak state of the British economy could not support both a Keynesian program of full employment and free collective bargaining. Based on their memories of the 1930s, the unions were caught between their fear of what the Conservatives might do if returned to office and a Labour party that demanded wage restraint. The unions responded to their predicament with strikes, a program of structural economic reform, and greater activism within the Labour party. But as the economic crisis deepened, the TUC was divided over which of these approaches, an industrial or political response, was more appropriate.

Markovitz and Allen argue that German trade unions were staunch Keynesians precisely because they experienced little Keynesianism under either CDU-CSU or SDP governments. Thus, German trade unions advocated Keynesian solutions to the crisis further into the 1970s than other union movements. It was not until 1977 the DGB recognized the roots of growing unemployment as structural and technological and not just cyclical. At this point, the unions adopted what the authors refer to as a Keynes-plus strategy. The SDP was to stimulate the economy, but this was to be supplemented by union efforts through collective bargaining to curb technological unemployment.

According to Martin, the Swedish labor movement has been able to devise solutions to economic problems that are consistent with its ideology and organizational needs. The Swedish labor movement was the first to adopt Keynesianism and the first to recognize its inflationary consequences. Their solution was a solidaristic wage policy. But this policy squeezed profits that could result in a lack of investment capital. This problem finally emerged in the 1970s, when "irreversible changes . . . in the international pattern of comparative advantage" (p. 303) made new investment in new sectors and products necessary. In response, the labor movement proposed "wage earner funds" as a progressive solution to the shortfall in equity capital. The problem in Sweden now is one of strategy: how the labor movement can sell its solution in the midst of a vigorous employer campaign against it.

The analysis in these case studies is subtle and detailed. Martin's essay, especially, goes beyond the existing literature in English to add considerably to our knowledge of Swedish trade

unionism. But *Unions and Economic Crisis* is not without flaws. The approach of the authors to their material is overly institutional. The activity of peak labor organizations is divorced from larger social forces. As a result, the reasons why unions chose the programs and strategies they did is unclear. Second, these essays are historical and descriptive with little theoretical content. The conclusion, which was to compare the results of the two volumes, is especially disappointing in this regard. Still, as history and description, the accounts in this volume are not likely to be surpassed.

ALAN DRAPER

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Totalitarismes. Edited by Guy Hermet, Pierre Hassner, and Jacques Rupnik. (Paris: Economica, 1984. Pp. 254. 125 FF, paper.)

We are witnessing the second coming of totalitarian theory. Whereas in its heyday the concept of totalitarianism was developed primarily in the United States, it is now alive and well in France. Ignored for 30 years (Hannah Arendt's classic has only lately been translated in its entirety; Raymond Aron's voice was a lonely one), totalitarianism is now the rage on the Left Bank.

This is the context in which a number of French academics, as well as two outsiders (Juan Linz and Richard Lowenthal), have assumed the responsibility of putting the French discussion concerning totalitarianism on a scholarly level. The year of 1984 seemed an appropriate moment to do so, and the volume under review with its paradoxically plural title is the result of a colloquium held early in the year. The 11 essays deal with images of totalitarianism among Western theorists (Pierre Hassner) as well as among Soviet and East European dissidents (Jacques Rupnik); historical, philosophical, and cultural explanations of totalitarianism (Blandine Barret-Kriegel, Pierre Manent, Bertrand Badie, Pierre Birnbaum); possible applications of totalitarian theory to the most diverse cases ranging from a comparative essay on the classic examples of Italy, Germany, and the Soviet Union (Guy Hermet), through separate examinations of the Soviet Union (Aleksander Smolar), China (Jean-Luc Domenach), Africa (Jean-Francois Bayart), and the Arab countries (Jean Leca). Linz and Lowenthal contribute epilogues.

Obviously, such a collection contains various points of view. Generally speaking, however, the contributors agree in their definitions: The essence of totalitarianism lies in the absorption of

civil society; usually, society is absorbed by the state, although several contributors (e.g., Birnbaum and Hermet) remind us, citing the example of Nazism, that totalitarianism can be fundamentally anti-statist. The contributors also insist that other characteristics traditionally associated with totalitarianism—terror, a single party, charismatic leadership, an official ideology—are neither sufficient nor necessary concomitants of totalitarianism. Finally, the contributors agree in their overall attitude to totalitarian theory. They attribute a heuristic value to the concept at least comparable to that of other social science approaches. In short, they approach the concept of totalitarianism with openminded, although not uncritical, sympathy.

The definition of totalitarianism adopted here and the openminded attitude of the contributors lead to some unexpected and probably unintended conclusions. As Linz notes, many specialized studies seek to show that individual regimes are not "as totalitarian as we thought" (p. 243). In fact, this is what happens here. Domenach concludes his chapter with the carefully qualified hypothesis that China may be abandoning totalitarianism; he thus calls into question one of the accepted verities distinguishing totalitarian regimes from authoritarian ones (only the latter can evolve toward pluralism or democracy). After carefully sifting the evidence, both Bayart and Leca conclude separately that in spite of "bits and pieces" (*bribes*, Bayart p. 209) of totalitarianism, this term does not really apply to Africa or the Arab world. Hermet denies the designation of "totalitarian" for Nazi Germany and fascist Italy, preferring to call even the former "an almost complete modern absolutist regime" (p. 150). Smolar does staunchly insist that there is a totalitarian regime, and it is the Soviet Union. However, the evidence he marshalls could just as easily lead one to the opposite conclusions, and even Smolar dilutes his argument by distinguishing between an (extinct) totalitarian movement and a (surviving) totalitarian system.

All the essays offer sound scholarship and intellectual originality, but the most thought provoking are those by Hassner and Manent. Hassner measures the ebb and flow of totalitarian theory, reserving his strongest criticism for post-totalitarian sovietology. He suggests that "the error of the theory of totalitarianism would lie in the passage from Arendt to Friedrich and Brzezinski, in the translation into a series of empirical criteria of something which can only be grasped through a totalizing perspective inseparable from political philosophy" (p. 36), and he concludes that the concept of totalitarianism reveals the limits of political science. Manent pursues this theme further by focusing on the modern separation of society and

state and on the relation between legislator and subject, seen here as the problem of representation, in modern political philosophy. He suggests that "the absorption of civil society by the state is but the appearance of a transformation which in its essence is the absorption of the state by civil society" (p. 99).

One wonders whether this rich and stimulating volume will have the impact it deserves in France. It is unlikely that the *nouveaux philosophes* showmen of totalitarian theory will be deflected from their course by it, although the book may provoke a debate with serious theoreticians of totalitarianism such as Claude Lefort. Whatever its impact on its home ground, the book is well worth reading on this side of the Atlantic as well.

ANDRÉ LIEBICH

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Parliamentary Democracy and Socialist Politics.

By Barry Hindess. (Boston: Routledge & Kegan, 1983. \$9.95, paper.)

In *Parliamentary Democracy and Socialist Politics*, Barry Hindess states his deep concern over strategic thinking on the Left in Britain. He feels that if the Labour party continues with its "inadequate response to current political conditions" (p. 7), it will lose its chance to direct British politics in the near future.

Hindess identifies several basic problems in socialist thinking that have led to the declining strength of the Labour party in Britain. He traces some of these difficulties back to fundamental conceptual weaknesses in Marxism itself. Marxism's inability to conceptualize economic agents other than human, and its conception of society as a structured whole, limit its usefulness in an age of multinational corporations and competing interest groups. Much theoretical discussion is spent on Marxist debates concerning the class character of parliamentary democracy and the possibility of a democratic road to socialism. These debates are reinvestigated in order to show Marxism's inadequacy as a theory of politics. Hindess regrets that this inadequacy is adversely affecting Labour politics today. For example, the Labour party has not tried to build broader coalitions between competing groups on the Left because of the Marxist conception that the working class is a large part of the electorate and will automatically vote for a socialist party.

The next section of the book probes the democratic mechanisms through which socialism must work. Tony Benn and others on the Left in the Labour party have argued for some time that the role of Parliament needs to be strengthened in

order to achieve a truer socialist state. Hindess shows why a more democratic parliament might actually hinder the effectiveness of a socialist state. He states, "there are inescapable limits to the control of state apparatuses through parliamentary mechanisms alone" (p. 81). Furthermore, one cannot avoid conflicts between the various organs under democratic control without reducing the level of democratic control. The whole question of democracy has confused Labour's strategies, and many socialists do not realize that the extension of democracy in Parliament might raise many more problems for socialism than it would solve. Other disputes about democracy in the Labour party have considered the relations between parliamentary democracy and party democracy. The remaining chapters consider the implications of these theories on current socialist politics in Britain.

Hindess presents his theses logically and carefully. Although the book tends to be a little too theoretical, many of his points are very convincing. Those who are concerned about the future of the Labour party will applaud his attempt to awaken socialists in Britain to some of the possible causes of Labour's declining power and to the need for restructuring their party with broader-based coalitions.

On the other hand, Hindess might have spent less time discussing his former book and more time on recent events and on developing his promise, "positive policy positions" (p. 154), in order to give more force to his arguments. One is left with a feeling that much more could have been said. Furthermore, Hindess's often long and cumbersome sentences require rereading and hinder the flow of his ideas. Old-line socialists, as he recognizes, will identify him as a traitor and revisionist for opting to gain votes at the expense of remaining true to the basic tenants of Marxism.

In the end, however, perhaps it is enough to carefully identify the fallacies in present socialist strategies. If others can recognize why Labour is losing votes, they will be in a much stronger position to reverse that trend. *Parliamentary Democracy and Socialist Politics* makes a contribution to the study of modern socialism in democratic settings and to the understanding of socialism's declining appeal in Britain today.

ELEANOR E. ZEFF

Iowa State University

The Harmonization of European Public Policy: Regional Responses to Transnational Challenges. Edited by Leon Hurwitz. (Westport, Conn.: Greenwood Press, 1983. Pp. xviii + 264. \$37.50.)

Although Hurwitz assures the reader in the Preface that the original essays in this book "reflect a balance among theoretical overviews on the nature of the policy harmonization process, empirical data analyses, and descriptive-narrative case studies" (p. xi), the last category best describes the nature of the volume's 11 contributions. Indeed, the strength of this book is the information it contains about various efforts by Europeans and some non-European countries, for example, Canada, that have joined the Europeans to achieve a harmonization or coordination of public policy concerning a wide variety of issues. A number of scholars and practitioners have been brought together to describe or report on the successes and failures of harmonization efforts in foreign policy (Werner J. Feld), space research (Roy Gibson), terrorism (Rodger M. Govea), scientific research (John Goormaghtigh), and the regulation of multinational enterprises (Joseph Rallo). Other chapters describe the recruitment of higher civil servants into the European Community (David M. Wood), the European Monetary System (Harmen Lehment), the extent and nature of bloc voting in the European Parliament (Hurwitz), and the tentative development toward a European Bill of Rights (Jonathan M. Miller). Perhaps to demonstrate the completeness of the enterprise, there is even a final essay, "The Evolution of the European Pharmaceutical Profession" (J.-Albert Verreydt).

There can be no question about the qualifications of the authors, all of whom have written informative but at times rather technical and dry chapters. For serious students of intergovernmental organizations in general and of European organizations in particular, the book will be a welcome addition to personal or institutional library holdings. For both the specialist and non-specialist, the book can serve also as a useful handbook for a review of the background and status as of the early 1980s of a number of well-known, and not so well-known, European efforts to achieve policy harmonization. The contributions that do not deal with policy harmonization but with the European Parliament or civil service recruitment in the European Community contain the best examples of the empirical data promised in the Preface. The chapter on the European Parliament and the evidence that shows that in general ideology is more important than national origin in voting on the relatively few controversial issues that come before the Parliament is perhaps

of broader interest than most of the other selections in the book.

The book contains an index, a long and necessary list of abbreviations and acronyms, and a bibliographical essay. An unfortunate error exists in chapter 5, however, where several pages are out of order (the correct order is pages 108, 110, 109, and 111).

ARTHUR B. GUNLICKS

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Undermining Capitalism: State Ownership and the Dialectic of Control in the British Coal Industry. By Joel Krieger. (Princeton, N.J.: Princeton University Press, 1983. Pp. xii + 321. \$27.50.)

Joel Krieger's study of the British coal industry is an important and significant contribution to understanding the realities of political power in industrialized societies. He begins with the argument of many Marxist and Weberian analysts that capitalism and bureaucracy resemble each other and will prevail over more traditional, parochial patterns (even if Marxists assume that further change will result in the overthrow of bureaucratic capitalism). In the United Kingdom, congruence between capitalism and bureaucracy resulted from the Labour party's nationalization efforts that followed the Fabian tradition and the London Transport model created by Herbert Morrison. Although social democrats argued that state ownership was a precondition of social and economic equality (p. 3), some contemporary Marxist analysts have begun to question the validity of this assumption and its implication that a state bureaucracy would differ from capitalism in serving working-class interests (pp. 26-36).

Krieger examines the degree to which workers' perception of their interests as shown in their normal work patterns differ from, and have the capacity to hinder, the efforts of central authorities to rationalize industrial relations. The National Coal Board's (NCB) effort to impose a uniform daywage system throughout the industry in the form of a National Power Loading Agreement (NPLA) in 1966 is the focus of Krieger's study. British mining, with its traditional pattern of residential isolation and particular work relationships in each mine (and shift) depending on the geological conditions at the coal face, is especially useful in demonstrating the limits of central authority.

Nationalization of the coal industry was a goal long sought by British miners and British social-

ists. Success in 1947 seemed to mark the start of a new era. But what Krieger describes as a perfect Weberian bureaucracy in the NCB (pp. 19-20, 29, 33, and 207) was soon confronted with the reality of uneven regional development that proved the wisdom of Robert Burns's sentiment that "The best laid schemes o'mice and men gang aft a-gley."

Following John U. Nef's *The Rise of the British Coal Industry* (Routledge, 1932), students of British mining have long been aware of the importance of regional diversity in industrial practices. Even the miners' union, the National Union of Mineworkers, and its predecessor, the Miners Federation of Great Britain, had recognized these differences. Although not citing Nef's work, Krieger examines variations in work patterns in Durham and in Nottinghamshire and their impact on the NPLA. The contrast between the team-oriented "cavilling/mara" system in Durham (pp. 80-90) and the "butty system" of subcontracting in Nottingham (pp. 90-94) illustrate the impact of local variations on the centralized bureaucracy of the NCB.

The heart of Krieger's analysis is found in detailed anthropological studies of five Durham collieries (chap. 4) and five Nottingham collieries (chap. 5) including examination of actual work patterns before and after the imposition of the NPLA and its impact on productivity in each of the 10 mines. Krieger clearly demonstrates the limitations on the NCB to change work practices within the mines (Table 4.4, p. 171; Table 5.7, p. 257).

The implications of Krieger's analysis are clear. Rationalized central bureaucracies face significant limitations in an industry where traditional patterns of work are slow to change and the formal rules governing industrial relations may be quite different from the actual rules governing behavior at the coal face. And the NCB (or other central authority) will face serious resistance to its efforts to rationalize the industry, as seen in the coal strikes of 1972, 1974, and 1984.

There are two minor criticisms of the book. First, despite Krieger's conceptual framework, the title would have been more descriptive had it been "Undermining Bureaucracy," as he admits when discussing the ineffectiveness of the NPLA in a section titled "A Failed Weberian Bureaucracy" (pp. 18-20). And second, the degree to which we can generalize from Krieger's findings is limited by the special nature of coal mining and the problems of supervision of the coal face. But mechanization and power loading may end that limitation in the long run.

WILLIAM D. MULLER

State University of New York, Fredonia

The Critique of Ultra-Leftism in China. By William A. Joseph. (Stanford, Calif.: Stanford University Press, 1984. Pp. 312. \$35.00.)

This book, a product of a doctoral thesis submitted to Stanford University, is perhaps too schooled and technical for a political science generalist. It focuses on a narrow subject—an analytical description of the Chinese ultra-Leftists from 1958 to 1981, a period commencing with the Great Leap Forward and ending with the trial of the Gang of Four. Simply put, the ultra-Leftists are the “impatient revolutionaries” (p. 10). Contrary to common conception, the author attributes the origin of ultra-Leftism partly to the petty-bourgeois ideology (p. 51). One wonders whether, in reality and in *realpolitik*, the ultra-Leftists simply exploited the political situation to gain and retain power without, as Joseph would like his readers to believe, invoking such exalted notions as dogmatism (pp. 33-39), subjectivism (pp. 39-47), and sectarianism (pp. 48-51). If parallels could be drawn with past dynastical political struggles in Chinese history preceding the existence of the doctrines of Marx, Engels, Lenin, et al., it may not be at all necessary to interpret political events from 1958 to 1981 on such an ultrasophisticated theoretical plane.

The most important contribution that Joseph makes to our understanding of the political history of the relevant period is his imaginative analysis of the ambivalent role played by Mao Zedong, whose thoughts constituted the cornerstone of the ultra-Left line, but who both actively encouraged and vehemently criticized the ultra-Leftist activities at different stages (pp. 223-228).

Two meaningful and prognostic messages emerge from the book. First, the ultra-Leftists failed because they “seriously misjudged the political methods and policy priorities appropriate to the task of building a socialist society” (p. 228). Second, the post-Mao Four Modernizations advocates’ negative assessment of the past is insufficient to ensure enduring stability: “China’s future will be determined by how successful its leaders are in promoting healthy change in both the political and the economic life of the nation” (p. 244).

Readers who are not familiar with events of intrigues, political personages, and power struggles in contemporary China would perhaps be puzzled by quaint terms and expressions scattered throughout the text, which demands intensive and careful reading and not casual perusal in order to decipher and appreciate Joseph’s intricate analysis and complex methodology.

Indeed, I do not share many of Joseph’s views. Their major departure is that the events that took place during the period of more than 20 years can

be conveniently interpreted as much as a series of conflicts between politically ambitious factions and personalities as conflicts in their underlying ideologies. Perhaps an equally tenable hypothesis, devoid of any ideological connotations, can be advanced to explain such political struggles because similar ones also took place in other eras of Chinese political history. Nevertheless, my espousal of a more parsimonious and atheoretical view should not be taken as a deprecation of Joseph’s intellectual efforts in analyzing political development using a totally different frame of reference.

To Joseph’s credit, much reliance has been placed on diverse primary Chinese sources; both the notes and the bibliography of his book are thorough and exhaustive. To a political scientist interested in China, this book is a delight to read and keep.

FRANKIE FOOK-LUN LEUNG

Hong Kong

The Legislative Connection: The Politics of Representation in Kenya, Korea and Turkey. By Chong Lim Kim et al. (Durham, N.C.: Duke University Press, 1984. Pp. xvii + 237. \$39.75.)

The Legislative Connection reports the results of the most ambitious research project undertaken in the area of comparative legislative studies since John Wahlke et al.’s *The Legislative System* (John Wiley & Sons, 1962). The authors, working with a host of collaborators, have collected parallel interview data from samples of legislators, their constituents, and local notables in Turkey, Kenya, and Korea. This huge data set is used to assess the nature of representation in these three nations, the role that their legislatures play in the policymaking process, and the determinants of public support for these legislatures.

Most previous studies of third-world legislatures have been characterized by their single-nation focus and by data bases that have been either journalistic and anecdotal or composed of “easy data” such as election returns, background characteristics of MPs, or constitutional provisions relevant to the legislature. By contrast, *The Legislative Connection* is comparative and reports rich new data on legislators’ perceptions of their roles and constituent perceptions of the legislature and its members. The three countries analyzed, while selected primarily for the convenience of the researchers, nonetheless offer sufficient regional

and political diversity to provide the grounds for tentative generalizations if not definitive findings. And, although the thrust of the work is clearly toward these comparative findings, the authors also carefully identify and consider relevant contextual variables in their explanations of national findings rather than extracting the legislature from its political setting as is so often done in legislative studies. Methodologically, the authors have in general made prudent and wise decisions as they have dealt with the enormous problems involved in implementing a cross-national research design in three third-world countries. Although some variables are operationalized in a rather slapdash manner the data are, for the most part, carefully presented and appropriately analyzed. On the theoretical front, the concept of a "legislative culture" is explicated and will prove helpful to legislative scholars, and the categorization of legislators into "internals" and "externals" is also quite useful. The brilliant causal analysis of legislative support in the last one-third of the book is also worthy of special note.

Many students of developing nations, however, will be disappointed by this study. Although the relevance of the legislature to creating links between the center and the periphery is explored, the book never really gets at the role of the legislature in dealing with problems of economic and social underdevelopment. This is in part attributable to the authors' specific adoption of the "mainstream" or SSRC approach to political development, as opposed to what they call the "neo-Marxist" or dependency model, a decision that guarantees that the questions of political economy so intrinsic to the politics of the Third World will be slighted.

The connection between this study and the literature on legislatures is also peculiar. The research design is clearly informed by the past work done in the field. However, in presenting their findings, the authors follow the pattern of selectively citing this literature in the beginning of each section, presenting their data, summarizing what they have found, but making no attempt at all to connect their findings with what we already know about legislatures.

The concluding chapter of the volume, instead of attempting such a synthesis, contents itself for the most part with a summary of what has been discovered about these three legislatures. At this point, we know more than we care to about Kenya, Turkey, and Korea and what we need to know is what these three legislatures have to tell us about legislatures in general and how these findings fit or don't fit with the work that has been done by other scholars at other research locales. Although some brief allusions are made to some of this work, the failure to consider this literature

in any systematic way mars what is otherwise a major empirical contribution to the discipline.

MICHAEL L. MEZEY

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Religion and Politics in the Modern World.

Edited by Peter H. Merkl and Ninian Smart.
(New York: New York University Press, 1983.
Pp. xii + 276. \$30.00.)

The upheaval in contemporary world politics evinces a renewed interest in religion; similarly, the current anarchy in religious thought and institutions often betrays a not-so-subtle interest in politics. Hence, for social scientists, among others, new studies of religion and politics are always welcome. Yet despite its title, this set of essays focuses primarily on nationalism and its religion-like manifestations.

Most of the essays were presented in 1979 at a symposium on "religion, myth, and politics" (p. xi); some have been updated through 1982. After a forward by Merkl and Smart and an introduction by Merkl, the collection's 13 essays are grouped into three parts followed by a conclusion from Smart.

Part 1 consists of three essays that attempt to set the groundwork for exploring the dynamics of nationalism and the inherent meaning and role of religion. In the first essay, Smart's especially insightful categories of the myth of nationalism and religion set a potentially fruitful analytical framework for further case study evaluation (Part 2). Unfortunately, however, his framework is infrequently applied. Jaroslav Krejci's discussion on the parallel dimensions of nation-state development and religion proves equally as promising and unfulfilled. Even more intriguing is Raimundo Panikkar's hope to shatter the dualism of religion and politics as well as the monism of religion/politics assumed in current research. He offers for consideration the promising prospects of political theology and its variants, such as liberation theology; as with the others, though, fulfillment of his hope is eluded.

Nevertheless, Part 2 provides seven excellent case studies of a particular combination of nationalism, politics, and religion in a designated country. Yet given the diversity of the authors' approaches, four subgroupings of essays suggest themselves. First, with regard to the phenomenologically religious aspects of nationalism, Merkl ably retells the conditions for and rise of Hitler's messianic drive to power. Second, focusing on the conscious incorporation of a religious component into nationalistic ideology, three authors provide illuminating examples: Gary Lease on the Chris-

tian element of National Socialism, Wilbur M. Fridell on state Shinto in Japan, and Barbara Metcalf on Islam and Pakistan.

Third, considering the involvement of religious institutions in politics, Michael J. Carey provides a fascinating account of the Catholic Church's role in Irish struggles. Michael D. Kennedy and Maurice D. Simon also depict well the involvement of the Catholic Church in Polish politics, especially with regard to Solidarity. And finally, focusing on the political theoretical dimension of religious thought, David J. Beale provides a detailed description of the messianism of one thread of Jewish thought. Still, notably lacking in this kind of collection are case studies dealing with Nicaragua and Iran.

Incongruous yet in many ways more interesting, Part 3 consists of three essays that return to the original theme of myths, religious and nationalistic, and that introduce the problem of alienation. Leonard Greenspoon provides a disturbing account of Jewish perceptions of God and war and the myth of their interrelatedness from ancient scripture. Continuing on the theme of myths of war and the warrior, Edward Tabor Linenthal documents the painful devolution of a myth: the role of the American soldier in national life, from patriotic hero of the Revolutionary War to mass murderer of Vietnam. Finally, Kees W. Bolle competently argues that any "novel" claim to a materialist theory as a "complete" solution to social conflict (his example is Marxism) is neither novel nor complete, the myths of materialism being timeless, pervasive, and shortsighted.

Despite the unevenness of the book's organization, this collection should appeal to a variety of readers. Merkl and Smart have assembled a set of essays that are extremely informative, well written, and intelligently argued.

JOHN R. POTTENGER

California State University-Long Beach

Leadership Selection and Patron-Client Relations in the USSR and Yugoslavia. Edited by T.H. Rigby and Bohdan Harasymiw. (Winchester, Mass.: George Allen & Unwin, 1983. Pp. ix + 246. \$29.50.)

Leadership Selection and Patron-Client Relations in the USSR and Yugoslavia is a collection of six essays presented as papers in 1980 at the Second World Congress for Soviet and East European Studies. Despite the seemingly narrow focus of the topic, the anthology suffers from an unusual degree of diversity and disagreement within the work.

In the first essay, Joel C. Moses argues that

although Soviet cadres have fairly consistent regional associations, they see themselves primarily as specialists, and as this self-perception grows, they will become more assertive, eventually causing friction within the policy apparatus. In the next chapter, John Miller flatly contradicts Moses's findings of regional association patterns. Lenard Cohen, in his discussion of elite recruitment patterns in Yugoslavia, then argues that differing skills and backgrounds may well be translated into future conflict. Gyula Jozsa calls the patron-client relationship between the USSR and Yugoslavia a political *Seilschaft* and a budding interest group in Soviet politics. Daniel T. Orlovsky discusses the historic pattern of clientilism in Russia which, he feels, is "a more prominent feature in Russian history and political culture than elsewhere," but he fails to discuss the relationship of that culture to current Soviet politics. Shugo Minagawa's comparison of Soviet and Japanese patron-client patterns then flatly contradicts Orlovsky; contrary to the other authors, Minagawa suggests that patronage, far from causing disruption, may well serve to oil the system. So many are the contradictory conclusions in this volume, and so little attempt is made by the authors to compare findings or to discuss their significance, that it is not surprising that Harasymiw does not try. In his conclusion, he acknowledges that the book's contributors "blaze no new trails, nor do they stray much from the recognized path" (p. 235) and that their theoretical work was "often fragmentary and implicit" (p. 241). He therefore offers his own model for future research in this area.

Yet this volume has a greater significance than, as Harasymiw notes, merely conveying the importance and complexity of the cadre selection process. It raises the issue of how much our knowledge of the Soviet regime can be advanced by focusing attention entirely on the minutiae of Soviet politics when detailed information in this area is both scarce and questionable. This volume also makes manifest the problem of defining a research project so narrowly that its results are virtually meaningless because the questions asked of it are too narrow, and because the issue has, for the sake of methodological purity, become divorced from nearly every important social, political, personal, or cultural consideration. It seems strange, for example, that in a compendium on such a central aspect of Soviet politics, the role of ideology is scarcely mentioned. When all the information is gathered, there remains the problem of its interpretation and integration into the broader field of Soviet studies.

This is not to say that the authors have not, for the most part, done an excellent job of documenting and presenting their findings. There is a great

deal of useful information in this book that confirms prior suspicions about the rigidity and tensions within Soviet cadres policy. The study would have been even more useful, however, if it had also discussed the significance of these findings.

NICOLAI N. PETRO

University of Virginia

Europe's Industries: Public and Private Strategies for Change. Edited by Geoffrey Shepherd, Francois Duchene, and Christopher Saunders. (Ithaca, N.Y.: Cornell University Press, 1983. Pp. ix + 276. \$34.95.)

Recent vicissitudes and long-term shifts in the industrial organization of the international economic system have underscored an old truth: Domestic economic prosperity and national political standing are intimately tied up with the capacity of industry and government to adjust. What is the nature of the international economic challenge facing particular industrial sectors? How do government adjustment strategies differ in method and effectiveness? How do sectoral characteristics of industry and national political setting influence the success or failure of private adaptive strategies? These questions guide the analysis in this edited volume, which focuses on recent European experience in industrial change.

The study is organized around the examination of particular industrial sectors such as textiles, autos, and machine tools. From there it moves to national patterns and characteristics of industrial change. The approach is decidedly inductive and microanalytic. Out of this exercise, patterns are found and lessons are drawn that are modest yet interesting. The authors argue that the most important generalizations are those between national institutional style rather than the sectoral characteristics of industries themselves: "it seems more feasible to find general strategic similarities across sectors within the same country than across countries within the same sector" (p. 15). At the national level the editors find a variety of "strategic" patterns in government and industry adjustment behavior. Although governments across Europe have increasingly resorted to selective intervention to manage industrial change, the scope left for autonomous corporate maneuvering and the play of competitive forces varies greatly. In searching for useful generalizations to characterize the sources of these differences, the authors talk rather vaguely in terms of divergent "government-industry-institutional" relationships. In accounting for successful or effective intervention and nonintervention, the analysis becomes even more contingent and hesitant. Nonetheless, the

editors conclude that national economic goals are served most consistently where governments rely on the strategic use of "environmental" incentives (e.g., broad protection of industries or tax programs) rather than "structural" interventions that support specific firms or specific corporate strategies.

The book's virtues are found in its gathering and juxtaposition of empirical materials on European industrial change. In an area where many analysts are doing related but scattered research this is an important contribution indeed. For those searching for a powerful analytic apparatus or a fully elaborated theory of industrial change, this book will fall short.

The project serves a useful analytic bridge between the very specialized and narrow studies of single industries or nations on the one hand, and the broad macroscopic arguments of comparative political economists on the other. The editors promise a second volume, *Managing Industrial Change in Europe*, that will take up the international implications of particular national approaches to industrial change. As an important building block for that study and as an empirical casebook for other theoretical enterprises, this collaborative volume is quite valuable.

G. JOHN IKENBERRY

Princeton University

Policy Making in the German Democratic Republic. Edited by Klaus von Beyme and Hartmut Zimmerman. (New York: St. Martin's Press, 1984. Pp. 220. \$25.00.)

Recent developments in Central Europe have heightened interest in the politics of the German Democratic Republic (GDR), raising questions as to the state of our knowledge and the most promising strategies for further investigation of the subject.

A major center of research on the GDR has been West Germany. Researchers there have the motivation, a wealth of materials, and familiarity with the cultural and historical background. However, they have had to overcome political bias and polemics, an excessive focus on possible reunification, and neglect of a comparative framework.

Policy Making in the German Democratic Republic thus arrives at an appropriate juncture. It is designed to display to an English-reading audience a sample of West German scholarship on the GDR. All the authors are from the Federal Republic, all use such empirical data as are available, all are very familiar with the relevant

literature, all are influenced by the "imminent" approach of the late Peter Ludz—they judge GDR policy against the GDR's history and ideological-political program.

This volume, furthermore, is intended to show how GDR and communist studies generally should be directed away from "inputism," that stress on elite decision making and techniques of social control said to be characteristic of both totalitarianism and interest group theories. Instead, research should focus on policy outputs, not least because that would facilitate more objective comparisons.

The book is framed by a useful bibliography of (mostly) German-language works of the 1970s and an introduction by von Beyme. Hartmut Zimmerman provides an encyclopedic analysis of party, state, and mass organization structures as these relate to individual participation in the political system; Zimmerman is insightful and stimulating in discussing GDR thinking on the future development of the political system. (The length of this chapter may account for the limited scope given some contributors.) Gero Neugebauer is excellent but cursory on the social role of the military establishment; Gert Joachim Glaessner's discussion of the relationship between education policy and broader social goals is elegant and thoughtful.

Helga Michalsky's chapter would benefit from an expanded discussion of social policy's ideological and economic setting. Irma Hanke points out the extent to which the GDR has preserved earlier bourgeois cultural values. Her account of policy changes is helpful, as is Juergen Strassburger's review of economic policy in the 1960s and 1970s. Gerhard Wettig displays his formidable knowledge of recent GDR-FRG relations; one would have wished more stress on the GDR perspective. Hanns-Dieter Jacobsen reviews the diplomatic and economic aspects of foreign trade. Klaus von Beyme considers the GDR in a comparative output perspective. His interesting chapter, in which one might have expected a summation, demonstrates the strengths and weaknesses of his approach. Von Beyme never makes quite clear what his 39 "indicators of system performance," some of which seem more reliable than others, are to tell us about GDR politics. Surprisingly, there is no discussion of youth or sports, and nothing on church policy.

Measured by its editors' intentions, and despite the impressive quality of many chapters, the work is somewhat disappointing. A focus on policy output, if it is to shed light on the nature of the regime, should make clear the larger significance of a given policy for the system. Moreover, "policymaking" as such, that is, how policies are made or implemented, is not discussed. Some chapters are unavoidably dated. Whether and

when these essays were translated into English is not indicated.

Nevertheless, *Policy Making in the German Democratic Republic* is a valuable survey of the policy results of Honecker's first decade in power. It will fulfill one of the editors' goals by providing a valuable benchmark for comparisons with other industrial societies.

HENRY KRISCH

University of Connecticut

Nomenklatura: The Soviet Ruling Class, an Insider's Report. By Michael Voslensky. Translated by Eric Mosbacher. (Garden City, N.Y.: Doubleday, 1984. Pp. xvii + 455. \$19.95.)

Students of the Soviet political system, from Merle Fainsod and Frederick C. Barghoorn to Milovan Djilas and Wolfgang Leonhard, have emphasized the importance of the *nomenklatura* in the highly centralized and extremely politicized system of elite recruitment in the USSR. Yet one looks virtually in vain for scholarly literature on this important facet of the Soviet political system—especially in English. As a matter of fact, Voslensky's work is the first book-length study of the subject in any language.

Defined in a recently published Soviet dictionary of foreign terms as "the circle of officials whose appointment or confirmation falls within the competence of some higher organ" (*Slovar' inostrannykh slov*. Moscow: "Russkii iazyk," 1984, 11th ed., p. 338), the *nomenklatura* has been variously described by Western writers as a new class, a political or central bureaucracy, a caste, a partocracy, and a new and invisible aristocracy. Voslensky analyzes the *nomenklatura* as a concealed, parasitic, and corrupt ruling class. To the political scientist, his study is of interest *inter alia* because it sheds light on the question of how an increasingly complex but essentially monocratic political system copes with the problem of elite selection.

Administered by Party and government officials at the various levels of the Soviet political system, the *nomenklatura* system is supposed to ensure high and uniform standards in the selection, placement, transfer, and promotion of key officials, both inside and outside the Party. At least in theory, the institution of the *nomenklatura* serves as an instrument for the operation of a merit system and the application of rational criteria in the evaluation of qualifications and performance. Voslensky seeks to demonstrate that

the *nomenklatura* has in fact become a privileged class, a self-perpetuating elite that exploits the rest of the Soviet population for its own benefit. He brings to this task the experience and insights of a man who, although he did not hold a senior Party position, claims to have had close contacts with the Central Committee apparatus from 1950 to 1972.

Voslensky deals at some length with the history and development of the *nomenklatura* and undertakes fairly detailed calculations of its size, arriving at an overall figure of 750,000. Families included, he estimates the ruling class to encompass 3 million, that is, less than 1.5% of the total population (pp. 95-96). The author is at his best when detailing the special prerogatives of the *nomenklatura*, describing the life style of its members, and discussing the rules of the system which even the Politburo cannot ignore with impunity (p. 71).

Although Voslensky's study contains a good deal of information about the role of the Party apparatus, the functioning of the Politburo and the Central Committee Secretariat, as well as the decision-making process, only one of the nine chapters of his book deals with the operation of the *nomenklatura* system. On the subject of the different types of *nomenklatura*—basic (*osnovnaia*), registration and control (*uchetno-kontrol'naia*), and reserve (*rezerv na vydvizhenie*) *nomenklatura*, as well as on the scope of the system in Soviet society—his book is not particularly illuminating. As a result, the overlap in *nomenklatura* jurisdiction and the duplication in the exercise of *nomenklatura* powers—important weaknesses in the system—do not receive the attention they deserve. Close scrutiny of the Soviet press in recent years, especially the pages of *Partiinaia Zhizn'*, would have been helpful in this regard. Likewise, the all-important question of what positions are included in the lower ranges of the system, that is, the question of just how far down the *nomenklatura* reaches, is left unanswered. All in all, one cannot help but be disappointed that Voslensky does not provide more details with respect to many important aspects of the *nomenklatura* system, especially since he claims to be so intimately acquainted with the subject. His perspective as an insider, moreover, could have been enhanced by the utilization of the findings of Bohdan Harasymiw on the organization, mechanisms, and social determinants of the *nomenklatura* (*Canadian Journal of Political Science*, December, 1969, 2(4), 493-512; *Osteuropa*, July and August, 1977, 27(7 & 8), 585-597, 665-681) and the work of Boris Lewytzkyj (*Osteuropa*, June, 1961, 11(6), 408-412), whose position in regard to the importance of the *nomenklatura* the author, unfortunately,

misrepresents by quoting out of context (p. 70).

Nevertheless, Voslensky's book is a most valuable contribution to our understanding of the Soviet political system because of his insight into the world of Soviet politics in general and the *nomenklatura* in particular. The reader should be warned that the English edition of Voslensky's book is not a complete or verbatim translation of the German version on which it is based (itself a translation from the Russian). The English translation does not include most of chapter 1 and all of chapter 2 of the German version. On the other hand, the English translation contains a new chapter (chapter 8, translated by Roger DeGaris), which discusses political developments since the death of Brezhnev, in particular the succession of Andropov. No explanation is offered as to why the English translation was not prepared from the original Russian.

ROLF H. W. THEEN

Purdue University

Crosses on the Ballot: Patterns of British Voter Alignment since 1885. By Kenneth D. Wald. (Princeton, N.J.: Princeton University Press, 1983. Pp. xvi + 263. \$25.00.)

Early in the twentieth century, two major changes occurred in the British party system. First, the Liberal party ceased to be the major party of the Left and gave way to Labour, thereby producing the modern British party system. Second, the old Victorian alignment based on religion was replaced by a class alignment, with the major issues of political debate becoming the redistribution of wealth and economic inequality.

Two theories have been put forward to explain these changes. The older, traditionalist view asserts that the First World War was pivotal: Before 1914 the party alignment was religious, after 1918 it was class-based, and the displacement of the Liberals by Labour was an inevitable concomitant of this process. The newer, revisionist view argues that the transformation of the party alignment from religion to class took place before 1914, and rather than being inevitable, the collapse of the Liberals was a consequence of elite mismanagement: They, not Labour, could well have been the electoral champions of the working class.

By applying the techniques of modern social science to this historical debate, Kenneth Wald has, for the first time, produced comprehensive empirical evidence to test these two theories. By applying ecological analysis to British electoral

and census data between 1885 and 1910 and from 1918 onwards, he comes to one unambiguous conclusion. The elections before the First World War were based on a religious, not a class, alignment, whereas in the general election of 1918 and thereafter, "class assumed primacy and religion joined region as a variable with intermittent electoral power" (p. 250). The rise of Labour, Wald argues, was essentially a consequence of this shift, and not the result of elite mismanagement on the part of the Liberals.

Wald further argues that the displacement of the Liberals by Labour was facilitated by political socialization. The shift from denominational schools towards nonsectarian, state-supported elementary schools served to undermine the Victorian religious alignment. Because most working-class men were disenfranchised at this time, school rather than home provided the central component of political socialization. Unfortunately, this hypothesis cannot be conclusively tested with the data at hand, although the evidence that is presented on its behalf suggests that it might, indeed, explain the rapid demise of the Liberals after the First World War.

Crosses on the Ballot, which as a dissertation won the Gabriel Almond Award and the Samuel Beer Prize, is cogently and elegantly argued, methodologically rigorous, and marshals the literature on the subject with authority. Its important findings shed light on a debate that has continued in British politics for several decades. It also provides significant methodological and conceptual advances for those wishing to apply modern quantitative analysis to other important historical questions.

IAN MCALLISTER

The Australian National University (Canberra)

Islam, Nationalism, and Radicalism in Egypt and the Sudan. Edited by G.R. Warburg and U.M. Kupferschmidt. (New York: Praeger Publishers, 1983. Pp. xi + 401. \$39.95.)

Unless the reader is an area specialist familiar with both the Arabic language and the minutiae of Egyptian and Sudanese history, this valuable piece of scholarship, a collection of essays first presented in December, 1981 at the University of Haifa, is of marginal value.

The editors have allowed each contributor to transliterate Arabic terms without bothering to translate them. Even in the introductory essay

Warburg uses several Arabic terms, such as *dar al-Islam*, *salafi*, *shubban al-muslimin*, *sufi*, and *shura*, each carefully transliterated but not translated. Such inattention to the linguistic limitations of most readers could have been easily avoided, if only to add a glossary of some 25-30 Arabic terms. Ironically, one of the first terms that is translated is *jihad*, translated by Warburg as "holy war" despite the fact that the contribution by Hannah R. Rahman goes to great lengths to demonstrate that the term *jihad* has been subject to a variety of meanings, depending upon the theological and political climate at the time of its use.

Many of the essays are essentially exegetical treatments of the works of some Egyptian or Sudanese authors with little sensitivity to modern methodological techniques. This is not to be construed as a criticism *per se*, but to stress that such approaches to themes such as nationalism, Islam, and radicalism are subjected to rather traditional methods of analysis. Even the concluding essay by Elie Kedourie takes note of the fact that the study of such terms as applied to either Egypt or Sudan can lead to a variety of interpretations. "Such are the pitfalls of intellectual history that try to deal with the relations between concepts like [sic] Islam and nationalism" (pp. 379-380), concludes Kedourie.

The editors could have aided the reader's travail here by deleting the word "radicalism" from the title, focusing their introductory essays on the terminological dilemma faced by the contributors, and alerting the reader to the range of understandings that terms such as *nationalism* and *Islam* have in the minds of either Egyptians or Sudanese at different times and with reference to different classes.

The editors have made the assumption that the reader is thoroughly familiar with modern Egyptian and Sudanese history. The Mahdist movement in the Sudan may be familiar to all, but the events at Dinshaway in 1906 in the lower Delta are not. Because knowledge of this event is critical to understanding Subhi Labib's essay on the Copts of Egypt, it would have helped if either Labib or the editors could have given a brief description of the Dinshaway event in a footnote. Similar comments could be made with reference to several other essays as well.

The book is most rewarding for the area specialist. It brings out the fact that during the past century political authorities have exploited Islam for their own ends in both Egypt and Sudan. The impact this has had on the Coptic and Jewish minorities of Egypt is the topic of insightful studies by Gudrun Kramer and Labib. And although most of the essays deal with Egypt, John Voll's essay on Islamic fundamentalism in Sudan

constitutes a valuable counterpoint to those on Egypt.

William Brinner's essay is the most significant of the entire collection. His analysis of contemporary "anti-Orientalism" in Egypt as manifested in the works of 'Aisha 'Abd al-Rahman is a chilling reminder to us all of the danger accompanying a merger of religious fervor with a violent hatred

of a peoples (in this case the Jews). The subordination of religion to the political order is dangerous; the subordination of politics to religion is manifest evil.

RICHARD H. PFAFF

University of Colorado

International Relations

International Violence. Edited by Tunde Adeniran and Yonah Alexander. (New York: Praeger Publishers, 1983. Pp. xiii + 265. \$24.95.)

This important volume explores some of the fundamental causes for international violence, offers an analysis of patterns of conflict in five regional settings, and presents a number of different perspectives on violence.

The four chapters on the sources of international violence (Part 1) are not of equal originality. The chapter by R. Olukayode Jegede on the psychological foundations of violence is merely a review of well-known theories of aggression, a review that does not even attempt to assess the new scholarship in the field. The chapter by David G. Hubbard on the psychodynamics of terrorism is somewhat more original: By calling attention to the neurochemical changes occurring in persons involved in a violent crisis, Hubbard points out the possibilities for new directions in the study of conflictual behavior.

The chapter by Malvern Lumsden on the sources of violence in the international system constitutes an attempt to offer a general, theoretical framework for analyzing the subject. Lumsden's approach is neo-Malthusian: He believes that the key factor determining international violence is the relationship between man and his environment. Although the chapter includes many interesting theoretical ideas, it fails to provide new insights regarding today's international system. Social inability to satisfy the biological and psychological needs of a growing population on a limited resource base is certainly an important factor in generating international violence, but it does not account for numerous aspects of international violence in today's international system. Wilton S. Dillon's contribution also offers some fascinating insights, especially from the field of anthropology, but the application of these insights to a better understanding of international affairs requires further analysis.

Among the essays on regional violence, Howard Stoffer's chapter on Central Europe

deals extensively with potential nuclear violence, the only chapter to do so in the entire volume. Ernst Halperin offers an interesting analysis of violence in Latin America, focusing on Nicaragua. In his view, there are two kinds of international terrorists: those such as Bader-Meinhof, the Red Brigades and the S.L.A. who act pathologically, using politics as an excuse for acting out infantile rage, and those who act "methodically and rationally" as part of a political struggle. He sees the Sandanistas as the second kind of group. William H. Overholt's essay on international violence in Asia sees the USSR as the power most interested in upsetting the continental status quo. At the same time, he concludes that there is a dramatic decline in interstate violence in Asia because of the decline of nationalism and the adoption of an inward-looking attitude (focusing on economic development) in most Asian states. Like two other essays in the volume, Barry Rubin's chapter on the Middle East lacks supportive footnotes, so its claims are thus hard to substantiate. However Rubin is correct in noting that surrogate warfare and terrorism are central forms of violence in the region. Rubin believes that although these modes of violence are effective as harassment and intimidation, they can hardly lead to the building of empires. T. A. Imobighe's essay on violence across African borders notes the limitations on the Organization of African Unity in trying to localize interstate conflicts, limitations that he sees as a function of Africa's dependence on external sources of military supply.

Parts 3 and 4 offer a variety of analytical perspectives. Robert A. Scalapino's chapter is the most general and all-encompassing. He notes the intimate relationships between national and international politics and sees them as the key for understanding violence in the modern world: The existence of "targets of opportunity" in domestically unstable states or regions is, to him, the greatest single cause for international violence today. Scalapino also offers a number of alternative scenarios for intervention in foreign countries. He

notes that international terrorism, and even guerilla movements, are less effective than traditionally believed, and he offers some reasons for why this is so.

The book concludes with two essays dealing with economic perspectives on international violence and two on legal aspects of the phenomenon. These contributions demonstrate again the unusual breadth of the volume that will be useful for scholars, students, and the general public.

ILAN PELEG

Lafayette College

The Influence of the Carnegie, Ford, and Rockefeller Foundations on American Foreign Policy: The Ideology of Philanthropy. By Edward H. Berman. (Albany: State University of New York Press, 1983. Pp. viii + 227. \$34.50, cloth; \$10.95, paper.)

This book is an academically radical, historical-revisionist, conceptually Marxist appraisal of the funding of U.S. foreign policy analyses and related activities by the three major foundations named in its title. As such, not surprisingly, it finds their efforts to be an intentional and effective propagation of the gradualist-reformist, but unalterably capitalist, "liberal" middle-class values that these foundations are presumed to embody. The already converted will have no problems with this indictment; the unconverted will have at least three.

Edward Berman raises the first problem explicitly himself on page 2: Doesn't "everyone know" that American capitalist foundations want to propagate ideas that are essentially compatible with capitalist economic and American national interests? Yes, Berman does know that. What bothers him, however, is that this is not what these foundations *claim* that they do and that they are getting away with their claims, as it were. What they claim, in the particular context explored here, is that they encourage and support "independent" scholarship, especially in the United States, and then bring the benefits of appropriate parts of it to third-world countries, allegedly in the interests of those countries. But all American foreign aid, including foundation aid, is really designed to further the national interests of the United States, Berman insists. Among others, Secretary of State John Foster Dulles is quoted to this end (p. 46) as once making the unsurprising contention to a congressional committee that the purpose of foreign aid "is to look out for the interests of the United States."

A second problem is the "OK, but" problem. It arises over and over again. One example will have to suffice. In regard to what we may call the brainwashing allegedly endured by foundation fellowship winners from the Third World to study at "carefully selected, elite American universities," Berman admits (p. 96): "This is not to suggest that they [the fellows] are never exposed to intellectual perspectives considered as unorthodox; clearly they are. However, care is taken to assure that the few unorthodox approaches examined in, for example, their courses in Third-World development are more than counterbalanced by exposure to the voices and perspectives of developmental orthodoxy." In other words, the foundations really sometimes do right by their own proclaimed standards, and even by Berman's, but—there is always a "but" or a "however"—there are other aspects of the situation by which these achievements are inescapably overwhelmed. Such even-handedness, otherwise admirable, tends to weaken the very case Berman is making against these foundations and even suggests a possibility that what "everyone knows" about them may not in fact be true.

Finally and most seriously, there is the pervasive problem of empirical weakness of argument. Again one example will have to suffice. In respect once more to third-world fellows, now specifically African, enrolled at Columbia University Teachers College, Berman observes (p. 92) that "it seems likely that most, aware that persistent dissent from the intellectual orthodoxy as defined by Teachers College faculty would jeopardize their academic standing and subsequent careers, internalized many of the [capitalist] lessons taught during their graduate days." What is the evidence for such a statement? Only that the truth of it "can be seen," the following sentence contends, "if we accept the argument that schools process knowledge as well as people and that an institution like Teachers College had an interest in stressing only that knowledge consistent with its role as an integral institution in the dominant culture." For those of us who are not already convinced, such explanations will not be enough.

KEITH S. PETERSEN

Rhodes College

The Command and Control of Nuclear Forces. By Paul Bracken. (New Haven, Conn.: Yale University Press, 1983. Pp. xii + 252. \$19.95.)

In the strictest sense, Bracken's book is an excellent technical and political analysis of command, control, communication, and intelligence (C³I). In the broadest sense, it is an exploration of

two fundamental questions: Who controls the most catastrophic means of war (ours and the Soviets') and what kind of control can be exercised, especially during a crisis or a nuclear exchange? The implications for national security doctrine, arms control, and foreign policy decision making in general are enormous and more than a little disturbing.

First, the questions.

By constitution general authority over military forces, and by law control over nuclear weapons, is held by political leadership, specifically the president. Bracken explains how and why a good deal of control has been devolved to the military over time, especially in wartime conditions when, for example, communications and intelligence may become significantly disrupted or the National Command Authority destroyed or otherwise rendered inoperable.

Nearly all doctrines governing the use of nuclear weapons, both strategic and theater forces, assume centralized control. Again, the probable facts of life rudely intrude, resulting in the greater likelihood of decentralized control. Part of the difficulty is technical. For example, if a major nuclear exchange were to take place, it would be extremely difficult to centrally assess damage and therefore difficult to know how to respond or what to anticipate. Part of the difficulty is conceptual, especially in the case of theater nuclear war in Europe. As Bracken notes, "A failure to define unambiguous centralized direction for a defense system means only that direction is supplied from decentralized, or bureaucratic, sources" (p. 135).

Now the implications.

First, important questions are raised about how well grand nuclear strategy can fit with our system of democratic constitutional government. More generally, there are disturbing questions raised about the role of political authority, democratic or nondemocratic, in situations of the most severe crisis. Military commanders can control weapons, but they do not ordinarily have authority or even the capability to determine the political conditions in order to terminate their use.

Second, Bracken's study does not provide much confidence in certain doctrines governing the use of nuclear weapons. For example, the doctrine of escalation dominance assumes that at some point in the escalation ladder, it should be possible (for rational political leaders) to terminate war. But if political leaders have already lost centralized control, it is difficult to even imagine how war could be terminated at an early moment.

Third, there are important implications for the role of arms control, particularly in the area of antisatellite weapons and confidence-building

measures. Stability, especially crisis stability, is a function not only of the deployment of the right mix of certain kinds of offensive weapons but also of the maintenance of effective systems of intelligence and control. To anticipate an even more complicated situation, we must also consider the grave problems of C/I in the context of horizontal nuclear proliferation. Because the fundamental purpose of arms control is achieving and reinforcing stability, Bracken's book is useful in identifying a number of issues that should be on the agenda of arms control (fortunately, some already are).

What makes Bracken's study useful and convincing is that detailed technical explanations are provided in historical context so that the reader can follow the development of policies. Clearly, political perspectives cannot be disassociated from technical considerations.

The Command and Control of Nuclear Forces should be required reading for analysts of nuclear doctrines, philosophers of arms control, and those concerned with the nature of foreign policy decision making, and, above all, policymakers.

BARCLAY WARD

The University of the South

Le Grand Frère: L'Union Soviétique et l'Europe Soviétisée. By Hélène Carrère d'Encausse. (Paris: Flammarion, 1983. Pp. 382. Price unavailable.)

Not since the publication of the most recent edition of Zbigniew Brzezinski's *The Soviet Bloc* in the late 1960s (Harvard University Press, 1967) has an attempt been made to provide a comprehensive scholarly assessment of the nature of the Soviet-East European relationship. Although numerous studies have appeared that focus on specific aspects of that relationship—e.g., military, economic, or political relations—none has provided a broad analytic overview. In *Le Grand Frère*, Hélène Carrère d'Encausse has synthesized a vast array of research in primary and secondary sources into an integrated analysis of the evolution of the relationships between the Soviet Union and its East European dependencies during the course of the past four decades.

Although the study presents a comprehensive overview of the history of that relationship, it is not primarily a historical study. Carrère d'Encausse, who in recent years has probably become the best known in the United States of French specialists on the Soviet Union, begins her analysis with a detailed discussion of the "Stalinization" of Eastern Europe. In her view, this process, although oriented toward guaranteeing Soviet

security interests in the region, aimed at total social and political change. Mechanisms of control developed originally in the Soviet Union were transplanted to Eastern Europe over the course of the late 1940s and early 1950s.

In the remainder of the study Carrère d'Encausse examines the factors which have both challenged and supported the continuation of Soviet domination in Eastern Europe over the course of almost four decades since World War II. In the first category she gives special attention to the series of political shocks—from the Soviet-Yugoslav split of 1948 to the Solidarity movement in Poland in the early 1980s—that have demonstrated the continuing attraction of national traditions and the periodic open resistance to Soviet hegemony. Among the factors that have reinforced Soviet domination, Carrère d'Encausse discusses the numerous areas of political, economic, and military integration that contribute to the maintenance of the Soviet role in Eastern Europe. Despite the anachronism of imperial control at the end of the twentieth century and the ongoing resistance to Soviet domination evident throughout the region, the Soviets remain strongly committed to holding on to the one example of revolutionary "success."

Although *Le Grand Frère* is written primarily for a general audience, the specialist will benefit as well from the balanced and comprehensive treatment of Soviet-East European relations contained in the book. When it becomes available in English, this study will provide the type of text, long unavailable, essential for undergraduate courses—that is, an integrated and readable book that combines a balanced historical overview with trenchant analysis.

ROGER E. KANET

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The Federal Republic of Germany and the United States: Changing Political, Social, and Economic Relations. Edited by James A. Cooney et al. (Boulder, Colo.: Westview Press, 1984. Pp. xii + 253. \$22.00, paper.)

It seems altogether fitting that the Woodrow Wilson Center chose 1983, marking the 300th anniversary of the first German settlement in Pennsylvania, to call together some 40 social theorists and political practitioners from both sides of the Atlantic to reflect on past, present, and future FRG-U.S. relations. This collection of essays by 13 prominent participants offers a politically provocative overview of those deliberations. Their aims were fourfold: to assess the importance of national value structures underlying relations be-

tween the two states, to examine a growing divergence in their respective economic policies and security preferences as a function of value change, to suggest policy alternatives for sustaining and strengthening their special relationship, and to place FRG-U.S. ties and tensions in the larger context of European-American relations.

James Cooney's introductory observation that both countries are experiencing a loss of self-confidence and lack of self-identity in a rapidly changing socioeconomic environment finds substantiation throughout the work. Indeed, this vulnerability accounts for many of the strains besetting the Alliance. Gordon Craig traces the evolution of German and American post-Enlightenment values as they pertain to foreign policy approaches. American values historically evince a penchant for idealism, missionary zeal, and periodic withdrawal; German values have relied upon discipline, professionalism, and accommodation. Richard Loewenthal holds cultural and generational changes responsible for the FRG's permanent integration into the Western-democratic camp, his ambivalence toward the born-again peace movement notwithstanding. Hans Peter Schwarz addresses the FRG's commitment to democracy and the Western Alliance as reflected in public opinion data. Finding strong majority support for both, one is puzzled by his vitriolic assaults on the "so-called peace movement" (p. 90) and "lunatic fringe" neutralists (p. 78), whose significance he seeks to downplay. Juergen Moltmann notes a changing role for religion in politics stimulated by the debate over the morality of nuclear defense doctrines; he sees ecumenical solidarity providing a new basis for East-West *rapprochement*. Sidney Jones and Reimut Jochimsen attribute FRG-U.S. tensions to the global economic malaise for which protectionism offers no panacea. Both point to the "naive complacency" (p. 129) of the United States in pursuing one-sided monetarist policies that reverberate throughout the European export economies.

The discussion becomes more emotionally charged with Ernest May's reflections on the past, present, and future role of U.S. military forces in Germany. The options range from a deterrent alliance, to an armed alliance, to a symbolic alliance, to no alliance. May's disquietude over Reagan's "global unilateralism" and the inappropriateness of current NATO structures is shared by Josef Joffe in a review of West German security policies. Joffe paints the FRG as a "net importer of security" (p. 181) caught in a nuclear double bind that seeks maximum coupling with the United States and minimum provocation vis-à-vis the Soviet Union. For Kurt Biedenkopf, the future of FRG-U.S. relations will depend upon

structural reforms based on the new premise that "Europe is capable of looking after itself" (p. 207). In searching for ways to sustain the Alliance in an era of strategic reassessment, James Schlesinger admits that there are universal doubts about American willingness to provide Europe with unqualified military support despite his claim that the United States has taken European security problems much more seriously than the Europeans themselves for the last 25 years.

Fritz Stern's closing essay highlights the extent to which the recent TNF deployments have given new life to the German Question "in all of its intractable complexity" (p. 324). Having "forgotten" the Federal Republic's *provisional* status enshrined in the Basic Law—for reasons of superpower expediency—the world conveniently managed to forget the national identity problem as well. After four decades of military complacency and exemplary economic performance, Germany remains a focus of *Angst* for friends and foes alike, while its own fears mount about the prospects of becoming *Deutschland unter alles*.

The real lesson of this book is not that Germany needs to accept its status as a "repressed nation" with open arms in order to allay old fears. Rather, the moral is that the United States itself must abandon the psychology of "paramountcy" and adopt a truly "flexible response" to the sea change in European security needs and economic interests. This is the first step to a very important dialogue.

JOYCE MARIE MUSHABEN

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The American Style of Foreign Policy: Cultural Politics and Foreign Affairs. By Robert Dallek. (New York: Alfred A. Knopf, 1983. PP. xx + 313. \$16.95.)

Robert Dallek, like many critics of American foreign policy, seeks to understand the consistent irrationality of American foreign policy so that potentially catastrophic shortcomings can be overcome. For Dallek, the key to America's failings in foreign policy is the "hidden side of American foreign policy. . . . those subjective influences that makers and backers of foreign policy barely glimpse themselves" (pp. xii-xiii). Specifically, he describes "foreign policy as a product of emotional displacement, of the impulse to make overseas affairs a vehicle for expressing unresolved inner tensions" (p. xiii). Thus, the "American style of foreign policy" is to seek in foreign policy a symbolic resolution of the conflict between the romantic urge to return to a nineteenth-century individualism and the drive

toward organization and conformity of urban, industrial society, and of the problem of maintaining a stable democratic regime in the face of that conflict.

Dallek claims that from McKinley to Nixon and Kissinger the domestic climate consistently shaped foreign policy more strongly than objective, external conditions. The Spanish-American War was a quixotic flight from the oppressive lifestyle of emerging industrial society; Wilson's "missionary" diplomacy was part of a general progressive reformism; the universalistic policy of the 1940s, including the promotion of the United Nations, was an expression of American feelings of cohesiveness; and America's cold war policy was largely an effort to promote domestic conformity and unity and to resist older and (in twentieth-century America) dysfunctional forces of individualism. In each case, Dallek finds that "such displacement" was "an unconstructive way to deal with international relations" (p. 282).

His contribution is most welcome. Irrationality in foreign policy is a serious problem, and the essence of his argument is convincing. Clearly, the various levels of public attitudes and emotions condition foreign policy, and we should explore how these forces affect foreign-policy makers. Unfortunately, the scope of this work and the subjective nature of the forces dealt with here prevent a rigorous examination of the mechanism by which mass emotional forces influence policy. Dallek's account, therefore, sometimes borders on the mystical: leaders responding to forces of which they are only vaguely aware and which they do not understand.

Some of the conclusions ring true, but others seem strained. Wilson's diplomacy was certainly an extension of progressive domestic goals. That the Truman Doctrine was the president's assurance that he would not allow domestic strife to endanger freedom at home is somewhat less convincing. The notion that Johnson refused to withdraw from Vietnam because he feared "that a pullback from the Cold War might open the way to a destabilizing shift from large-scale organization and uniformity to romantic individualism or renewed inner direction" (p. 243) is less satisfying still. Because he must fit eight decades of American foreign policy into this scheme without a discussion of how hidden emotional forces contribute to policymaking, Dallek is left to rely on little but his assertions: Policy X was irrational, therefore it must have been a response to domestic concerns.

Also, it is not entirely clear what standard is used to judge the rationality of a given policy. Again, we are at the mercy of his assertions: American policy in 1948 was irrational because the Czech coup and the Berlin Blockade were

defensive reactions to American challenges; Johnson was unreasonable to fear that American failure in Vietnam would embolden the Soviet Union. Perhaps, but one could reasonably take exception to these claims.

However, because Dallek intends this book to encourage discussion about the hidden side of foreign policy, these difficulties should be considered an opportunity to follow his lead in examining and describing the forces underpinning American foreign policy.

BRIAN E. KLUNK

Union College

Our Own Worst Enemy: The Unmaking of American Foreign Policy. By I. M. Destler, Leslie H. Gelb, and Anthony Lake. (New York: Simon & Schuster, 1984. Pp. 320. \$17.95.)

The authors address an important topic that so far has not been high on the agenda of U.S. publications, although it has been very high on the lists of complaints of both foreign governments and students of American foreign policy: the inconsistency of American foreign policy since the sixties. The authors' political starting point is the hard to disprove view that an incoherent foreign policy approach harms U.S. interests and, therefore, should be seen as our "worst enemy." The authors' analytical starting point is the "systemic breakdown" (p. 30) of the consensus of U.S. foreign policy as the central legacy of the Vietnam War. Destler, Gelb, and Lake examine the emergence of dissent as the main driving force of the "wild swings" (p. 262) of U.S. foreign policy. They also analyze the factors that in their view have contributed to the polarization and ideologization of American foreign policy: presidential politics, changes in the foreign policy elites, the press and the more assertive Congress, increased bureaucratic fights between White House "courtiers" and various departments' (particularly State's) "barons," and a stronger involvement of presidents in the day-to-day politics of foreign policies.

The authors argue convincingly that U.S. presidents have contributed to the breakdown of consensus, centrism, and majorityship by raising expectations of U.S. and presidential omnipotence that are no longer compatible with international and domestic realities. But the importance of this factor (which seems to be overstated) is not made clear, nor is it clear how important the role is of the public with its contradictory demands, and to what extent it determines the political leeway of presidents. A systematic analysis of the relationship between the president and the public would

have been very helpful in assessing presidential leeways more accurately and consistently, at least with respect to Jimmy Carter (see, for example, the contradictory remarks on pages 78, 86, and 120-121).

The study provides a good overview of the "new professionals" who have replaced the old "establishment"; an essential change in the political culture—the emergence of a number of elites as well as the more ideologized and partisan "new professionals"—is very well analyzed. It is surprising, however, that the broad fundamental changes in the domestic area such as establishment of interest groups and single-issue groups are hardly addressed (pp. 121-122). Although the authors make good points about the media, their remarks on the highly decentralized Congress are in part highly general and selective—as a whole, the legislative cannot be labeled as irresponsible, as congressional behavior in the arms control area shows. Without sufficiently analyzing the causes of "bureaucratic wars," the authors deal extensively with the increasing battles between "courtiers" and "barons" as well as among the various bureaucratic units since the Truman administration.

Their complex diagnosis determines their interesting therapy. It is not confined to relatively short-term bureaucratic and organizational fixes (e.g., centralization of Congress), but puts its main emphasis on a profound reexamination of U.S. "fundamental attitudes and ways of thinking about foreign-policy making" (p. 279). This means to find a "new political culture," a "new and very broad American center" (p. 286) that deals adequately with the main problem of U.S. foreign policy: the adjustment of a strong, but no longer dominant superpower to international realities. Regardless of whether one strives for a new consensus or a "centrist political force," a coherent U.S. foreign policy, as the authors have convincingly outlined, cannot be reached overnight and inexpensively. Despite its shortcomings, this book is an inspiring contribution to a topic that needs further systematic analysis.

BERND W. KUBBIG

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Forgotten Partnership: U.S.-Canada Relations Today. By Charles F. Doran. (Baltimore: Johns Hopkins University Press, 1984. Pp. viii + 273. \$32.50.)

While most observers agree that maintenance of the relationship between Canada and the United

States is of utmost importance, few give it very much thought. A goal of *Forgotten Partnership* is to focus our attention on that relationship while highlighting a variety of peaks and valleys in it. Doran believes that there is a very real disparity of Canadian and American attention to their bilateral relationship. It is given more attention and analysis on the Canadian side of the border than it is on the United States side. *Forgotten Partnership* rectifies that disparity by a successful discussion of its sensitive and complex nature.

Doran is uncomfortable with the assumption that the Canada-United States relationship can be characterized as one of simple interdependence. Instead, he suggests that analysis of the relationship should be in terms of five of its characteristics: intervulnerability, asymmetry of power, offsetting bargaining strengths, ambiguity of foreign policy interests, and a propensity to conduct foreign policy with prudence. Further, analysis of Canadian-American relations must be based on recognition of the factors that shape and determine the issues. Doran calls these factors "dimensions of analysis" and identifies three: the political-strategic dimension, the trade-commercial dimension, and the psychological-cultural dimension.

On the political-strategic dimension, the United States has a global view of politics. It has worldwide responsibilities and interests, and its decision makers tend to see international issues as security ones. Canada is less interested or involved in global political or security matters. Its major foreign policy problems and opportunities are regional: They involve the United States. On the trade-commercial dimension, further differences emerge. This dimension has a greater role in the foreign policy of Canada than it does in the United States. This is at least partly due to the smaller size of the Canadian economy and the higher percentage of its GNP represented by foreign trade. Finally, Doran believes the psychological-cultural dimension to be the most important of the three in explaining both disputes and their outcomes between the two governments. At least partly this is because it is basic to any analysis of the issues, and because the psychological-cultural dimension underlies the political-strategic and trade-commercial dimensions.

Two chapters contain case studies. Doran illustrates characteristics of the relationship between the countries and the dimensions upon which it rests in discussions of the Law of the Sea, the environment, and fishery issues in chapter 6 and Canadian energy policy in chapter 7. These case studies are intended to highlight the nature of the relationship and the status of the Canadian-American partnership.

Doran concludes that the most important

aspect of the relationship is the psychological dimension upon which it rests. Each country knows its own goals and interests and what it wants from the relationship. The conflicts that are generated result from the fact that these goals and interests do, indeed, conflict in many instances. A continuing and healthy partnership is one based upon a recognition of this basic fact of international political relationships coupled with an attempt to find common ground for resolving those conflicts.

Forgotten Partnership is an important and timely book. Doran does an excellent job detailing the present Canadian-American relationship and in suggesting its future. Although the very specificity of the subject matter and the price may make *Forgotten Partnership* inappropriate for most personal libraries, it deserves a place in every university library.

RICHARD HENRY FOSTER

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Arms Deal: The Selling of the F-16. By Ingemar Dörfer. (New York: Praeger Publishers, 1983. Pp. xvii + 287. \$29.95.)

The F-16 is by now the most successful combat aircraft of its generation; more than 3,600 have been ordered for 12 air forces. It has won encomium from budgetary officials and pilots and has proven its worth in the skies of the Middle East. But as Ingemar Dörfer shows, left to its technical merits it might never have come into existence, and would almost certainly not have won "the deal of the century."

Dörfer's book is ostensibly about the 1975 deal in which General Dynamics sold the F-16 to Belgium, Denmark, the Netherlands, and Norway. On a broader scale, he sees the F-16 deal at the nexus of a web of domestic problems, diplomacy, military requirements, and industrial needs facing buyers and sellers alike. To explain how this situation was resolved, the book circles around the F-16 deal, telling and retelling the story from the perspective of each of the nations involved. The result is the clearest account in print of a major arms deal.

The first half of the book concentrates on the Pentagon's decision to buy the innovative airplane. The lightweight fighter was originally conceived by renegade Air Force officers and the Northrop Corporation as a small and cheap alternative to the costly machines the procurement bureaucracy prefers. Civilian defense officials supported it as a solution to their budgetary headaches. To improve the case for the lightweight fighter, these officials—aided by Henry Kissinger

—arranged a *quid pro quo* whereby the Air Force would buy it if the four European nations did likewise. With this arranged, all eyes turned on the competition between General Dynamics and Northrop. Although General Dynamics won, Dörfer shows how slight adjustments in the rules would have Northrop the winner.

The European side of the story revolves around the creation of the four-nation consortium to buy and coproduce a standardized fighter. All four were small NATO countries looking to modernize their aging arsenals. Aside from this, they had little in common. Norway leaned toward the United States, Belgium toward Paris, the Netherlands waived, and Denmark was unsure it needed new fighters at all. Through Norway's leadership, the consortium was formed to assure the political and economic benefits that could only accrue through joint procurement. This led to another competition paralleling the one in the United States, pitting the two American corporations against Dassault of France and Sweden's Saab. The Americans had the inside track; their prototypes benefitted from newer technology and Washington's superior guarantees. The wonder of it all is not that a U.S. airplane won, but that the consortium held together. Dörfer argues that the reason lay in high politics. The outcome reaffirmed traditional patterns of influence in NATO; a French victory would have shaken the alliance to its core, a Swedish sale might have evoked outright hysteria.

Officials on both sides of the Atlantic used each other to achieve their own goals. American advocates overcame the Air Force's resistance to the diminutive fighter by dangling the carrot of European sales. European supporters needed the U.S. commitment to overcome their own domestic opponents. Direct intervention at the highest level was necessary every step of the way. The tale challenges prevailing notions that weapons procurement is determined by technological imperatives or the machinations of the military-industrial complex.

Dörfer makes good use of hundreds of interviews and reams of unpublished documents. He has the inside story on seven governments, four major corporations, and two U.S. armed services. He empathizes with the plight of all without becoming a spokesman for any one. Nevertheless, some actors have his admiration. There is Thomas Jones, the cosmopolitan chairman of Northrop who helped create the lightweight fighter concept and the European consortium but lost the deal. There is Norwegian Defense Minister Fostervoll who made the idea of the consortium into a reality. And Dörfer shows great respect for Belgium's then-Defense Minister Vanden Boeynants, who shrewdly fought against the American airplanes

but still won much for his country. If there is a villain, it is the French officials whose ill-considered blundering cost them more than the deal.

Although the book's title associates it with arms transfers, it has more to say about U.S. military procurement and NATO diplomacy than about the global arms trade. The way NATO members do business with each other is unique. The technical merit of the aircraft under consideration was only one of several major factors in the deal. Prime ministers, cabinet officials, and corporate chairmen were the stars; arms sales bureaucrats were almost invisible. Bribery—as Dörfer repeatedly points out—was present but irrelevant if not counterproductive. The situation is, of course, much different in the Third World.

AARON KARP

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The OAU after Twenty Years. Edited by Yassin El-Ayouty and I. William Zartman. (New York: Praeger Publishers, 1984. Pp. 406. \$29.95.)

The Organization of African Unity is celebrating its twentieth anniversary in the face of severe threats and challenges. Disagreements among member states over the status of the Western Sahara and over the civil war in Chad have forced the organization to the verge of collapse at the very time that its services are most sorely needed. The contributors to *The OAU after Twenty Years*—which follows El-Ayouty's 10-year anniversary collection—are acutely aware of these threats to the OAU, but at the same time are convinced that the organization has played an important role in African international relations in the past and potentially could do so in the future.

The OAU was designed with four purposes: the direction and encouragement of African unity, the direction and encouragement of continental development, the assistance of full decolonization and the end of minority rule in Africa, and peace-keeping. Each of these is covered in different forms in *The OAU after Twenty Years*.

The OAU was the offspring of decolonization in Africa, and as such was shaped by the conflict between the desire for continental unity—led by Kwame Nkrumah of Ghana—and the intense nationalism of the new states. Although it was originally envisaged as the instrument of Pan-African unity, the nationalism of member states eventually ended such hopes. This has meant, as Michael Wolfers notes in his essay on the institutional evolution of the OAU, that the organization has never actively encouraged continental

unity, but rather has reinforced the sovereignty of member states. The best evidence of this is the fact that most of the concrete and effective actions of the organization have taken place through the annual summit of the heads of state of the member nations.

The OAU has been somewhat more successful in dealing with decolonization and minority rule. William J. Foltz and Jennifer Widner discuss this in the context of southern Africa and conclude that, although OAU aid was of some help to liberation groups such as Frelimo and ZAPU/ZANU, the ability of the organization to solve the last and largest problem—South Africa—is quite limited.

As John Ravenhill notes in his essay on the OAU's role in Africa economic cooperation, development issues have become more important in the organization's second decade. As is true on several issues, the inability to obtain formal commitments and to create permanent institutions to oversee development projects has limited the role that the OAU has played in African development, but Ravenhill remains at least moderately optimistic that these shortcomings can be transcended.

On the problem of peacekeeping among African states, Henry Wiseman notes that again the lack of formal administrative, organizational, logistical, and financial institutions for peacekeeping operations have greatly hampered the OAU. This has meant that in many cases the best that the organization could do was offer good offices to belligerents. Wiseman argues that international conflict is likely to be a part of the African political environment for some time, and thus if the OAU is to attain its stated goal of finding African solutions for African problems, it is essential that more formal structures for peacekeeping operations be developed.

Following sections on the institutional growth and operational trends of the OAU, the book closes with a series of case studies and with an essay by El-Ayouty on the future of the organization. In this piece the general consensus found among the various essays is repeated: The OAU is a potentially useful organization, but if it is to survive and prosper in what will likely be a traumatic decade for Africa, it must grow and develop.

Overall the book suffers from a lack of communication between contributors that results in a great deal of repetition among the various essays. But at the same time, the importance of the subject and the impressive expertise of the contributors means that *The OAU after Twenty Years* is likely to become the standard work on the Organization of African Unity during this very trying time in its history.

STEVEN METZ

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Managing U.S.-Soviet Rivalry: Problems of Crisis Prevention. Edited by Alexander L. George. (Boulder, Colo.: Westview Press, 1983. Pp. xii + 415. \$30.00, cloth; \$11.75, paper.)

Just as it is difficult to identify weapons the Soviets have successfully hidden, so also is it difficult to identify crises prevented. This latter problem is not necessarily large if one views crisis prevention regimes as obtainable and as a result of the rationalization of diplomatic practices. Alexander George has developed a solid reputation for his case studies of contemporary great power confrontation. His classifications of diplomatic activity and calculation are constructed after an attention to detail that exploits the public record of crises to the fullest. And the attractiveness of his approach for both diplomatic historians and foreign policy analysts guarantees a comfortable niche in the realist portion of the discipline of international relations.

George's latest work is a collection of 15 chapters by 10 authors around the theme that it is possible to draw lessons from superpower confrontations of the 1970s. Although George suggests that it is also possible to formulate theories from this approach, the contributions do not move far in this direction. Useful typologies, not elegant theories, are the primary product of this work. George writes six of the chapters in this volume, and the remaining chapters demonstrate continuity in both organization and style. George Breslauer has two very useful essays that complement George's chapters well by focusing on Soviet perspectives and goals. Breslauer characterizes Soviet behavior of the détente period as that of a "collaborative competitor." His argument is a crucial one because crisis prevention regimes as an objective of U.S. foreign policy could only emerge in a context that has many bilateral elements. As George points out in his conclusion, crisis prevention regimes can only occur if the rivalry of the United States and the USSR is "composed of a variety of competitive 'games' that have different structures and somewhat different logics" (p. 381). Exploring carefully the symmetries of these games establishes the driving factor in successful crisis prevention. And as I. William Zartman suggests in his chapter, parties in a conflict are often far better off with agreements than without them.

The contributions to this volume are primarily case studies of third world-U.S.-Soviet confrontations of the 1970s. An early chapter by Paul Lauren on crisis prevention in nineteenth-century diplomacy, although well done, does not fit with the pace and pattern of the remaining contributions. Most chapters examine, either alone or in combination, the cases of the Middle East, Angola, and Ethiopia. We are led through

descriptions of crisis management that frequently do not flatter Henry Kissinger or President Carter. But the miscalculations of the Soviets are not ignored, either. The early Reagan administration is dealt with only briefly by George in his concluding chapter. Although he writes hopefully about the Reagan administration, the reader naturally tries to apply the lessons and suggestions of the volume to the Reagan record. The scope of this review does not permit that application, but the resources are present in *Managing U.S.-Soviet Rivalry* to evaluate critically the Lebanon and Nicaragua crises in particular.

Much of this volume proceeds from the notion of the importance of the Basic Principles Agreement of 1972. The difficulties of following through on the agreement provide a constant backdrop. Crisis prevention is, then, a creative diplomatic process that must contend with bureaucratic and domestic political factors on the one hand and your opponent's incoherence and misperceptions on the other. Decay of crisis prevention regimes may not be inevitable, however, and their persistence and reconstructions can be optimistic signs for the planet.

This book should be required reading in the National Security Council and in the USSR Ministry of Foreign Affairs. George's work never pretends to have solutions to the problems of managing the U.S.-Soviet rivalry, nor does he ever claim that a strategy can be constructed around crisis prevention. The modesty of purpose is both attractive and sensible. As long as the superpowers drift between the poles of unrestrained hostility and condominium, George's work will be useful.

PATRICK DRINAN

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Die Ära Nixon-Kissinger, 1969-1974: Konservative Reform der Weltpolitik. By Christian Hacke. (Stuttgart: Klett-Cotta, 1973. Pp. 319. DM 64,00.)

In 1969, the United States occupied the center of an emerging pentapolar world system driven by the interplay of competing national interests and regulated by the balance of power. The United States had experienced by this time a palpable decline in its military-strategic superiority and an erosion of its economic dominance. The over-extension of American power initiated by the Kennedy administration was no longer sustainable, and Richard Nixon understood that isolationism was no longer a viable option for American foreign policy.

It is Christian Hacke's thesis that Richard Nixon and Henry Kissinger, in response to these

circumstances, inaugurated a conservative reform of world politics with the "de-ideologizing of American foreign policy [and] a new emphasis on power and the national interest rather than on global communism" (p. 43). His analysis is organized around the Nixon administration's efforts to achieve balance among the great powers (China, the Soviet Union, and the United States) and to coordinate the foreign policies of the allies (Western Europe, Japan, and the United States). Nonetheless, the discussion inevitably returns to an analysis of the impact of the Nixon-Kissinger foreign policy strategy on European-American relations.

Détente is treated as a three-cornered affair. The interests and objectives of the great powers are set out, compared, and skillfully combined to give a dynamic portrait of great power relations and the course of détente.

The Chinese sought to isolate the Soviet Union by allying themselves with the United States. The United States was driven by its desire to exploit the Sino-Soviet rift to American advantage and Henry Kissinger's conviction that until the People's Republic—viewed as a revolutionary state—became a status quo power, it would remain a source of instability in Asia. Chinese cooperation was also desired to help end the war in Vietnam, but the Soviet Union was believed to hold the key to "peace with honor."

American policy in Asia, however, was always conducted with one eye fixed on the Soviet Union, because normalization of relations with the Soviet Union was the centerpiece of American policy. Hacke believes that Soviet-American détente was built on the foreign policy weaknesses of both nations and a confluence of interests. The Soviets desired access to Western capital, technology, and markets; arms control to relieve bottlenecks in the economy; and a relaxation of tensions with the United States in order to frustrate Chinese efforts to isolate them. In addition to ending the war in Vietnam, the United States sought a general relaxation of tensions in Europe, a limitation of the arms race in response to domestic political pressures, and the reining of Soviet activity outside its acknowledged spheres of influence. Hacke weaves these weaknesses, interests, and objectives into a convincing tapestry of Soviet-American relations.

But Hacke inevitably speaks with a European voice: European discomfiture with the "new" Pacific orientation of American policy is evident, and European suspicions and fears of Soviet-American bilateralism are underlined. Yet the Nixon-Kissinger foreign policy receives high marks because it served American interests, conformed to the dictates of the realpolitik, and established a framework for peace.

The Nixon-Kissinger foreign policy did not

enjoy similar success in its efforts to cope with the tensions arising within the Atlantic Alliance. American security policy increasingly subordinated European interests to the demands of Soviet-American bilateralism, and American foreign economic policy assumed a more strident, less generous position on trade and monetary issues. The cumulative effect was a leadership crisis in the Atlantic Alliance. Hacke successfully integrates the interplay of economic and political forces shaping Alliance politics during the Nixon administration, beginning with the New Economic Policy in 1971 and concluding with the oil crisis in 1973 and 1974. His most provocative and original thesis is that the Federal Republic's *Ostpolitik* set the initial pace of East-West détente and that it "initiated the erosion of the American claim to leadership in the Atlantic Alliance."

His discussion of Alliance politics concludes with policy prescription rather than praise. Future conflicts, he believes, are to be avoided by developing policies in tandem so that "connected but different problems can be handled simultaneously, but separately" (p. 207). But Hacke does not question whether the Atlantic Alliance, in its present form, persists from mere force of habit or from a realistic assessment of either European or American interests. He approaches the precipice, but does not take the final step.

Die Ära Nixon-Kissinger is dedicated to Hans Morgenthau, to whom the author is indebted for his theoretical—and many of his substantive—insights. Hacke concludes his well-written and judicious interpretation of American foreign policy with a spirited defense of the "realist school" of international politics. He argues that the national interest, power, the balance of power, and diplomacy form the basis of a "timeless model." His defense will not convince any but the already committed, but his theoretical and methodological atavism is welcome relief from some of the more exotic, scientific treatments of the subject.

JAMES CLYDE SPERLING

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A Palestinian State: The Implications for Israel.

By Mark A. Heller. (Cambridge, Mass.: Harvard University Press, 1983. Pp. x + 190. \$16.00, cloth; \$6.95, paper.)

Passionate hyperbole promoting one perspective or another on the Israel-Palestine conflict is not unusual, but such writing typically does little more than reinforce the myths of self-selected audiences. Put more kindly, much of the literature—whether popular or scholarly—has a pre-

dictable quality about it. Although there are predictable aspects to *A Palestinian State*, the book is distinguished by its ability to arrive at conclusions that confound our expectations. Heller, a senior research associate at Tel Aviv University's Center for Strategic Studies, has written a dispassionate, well-reasoned, and solidly researched policy analysis of Israeli options in meeting the claims of Palestinian nationalism in the occupied West Bank and Gaza Strip.

Heller makes no claims of impartiality. He forthrightly notes his commitment to Israel's security and well-being (broadly defined to include social, political, and moral considerations), and his calculus is colored throughout by his sensitive evocation of Israel's apprehensions. Indeed, Heller's solid credentials as an Israeli scholar give *A Palestinian State* a special authenticity and persuasiveness and help to make the book a formidable contribution to the literature (not to mention the policy arena).

In addition to a Palestinian state under the leadership of the Palestine Liberation Organization (PLO), Heller identifies five alternative outcomes (model-types) that Israel might pursue. These include nonsettlement, territorial compromise (usually identified as one of the variants of the Allon Plan that would leave certain militarily significant parts of the occupied territories under Israeli jurisdiction), nonterritorial settlement (such as the Begin Government's promotion of Palestinian autonomy short of sovereignty), a settlement bypassing the PLO (as represented by Israel's attempts to cultivate alternative leaders on the West Bank), and a settlement bypassing any Palestinian interlocutor (typified by the Labor party's penchant for striking a deal with Jordan's King Husain).

In Heller's view, none of the five alternatives share the qualities of feasibility and suitability. For example, nonsettlement promises persistently increasing national security costs while also threatening the durability of the peace treaty with Egypt. Territorial compromise, like solutions bypassing the PLO, brings with it the "extreme improbability" of finding any interlocutor with whom to conclude a settlement. Nonterritorial settlement would merely internalize Arab-Jewish conflict, perhaps spawning unattractive and reprehensible domestic policies. Thus, Heller comes to the relatively novel conclusion that the "least of all evils" is a settlement with the PLO leading to an independent Palestinian state.

Heller reaches the latter conclusion after considering the military, political, economic, and demographic characteristics of the prospective state. Although some of Heller's assumptions are questionable—for example, that only 750,000 Palestinians from the diaspora would elect to

move to the nascent state—his presentation is, in the main, persuasive. The weakest segment of his analysis is his treatment of economic viability, but this is less a shortcoming than a symptom of Heller's concentration on first-order political and strategic considerations.

Although Palestine would be a sovereign state, its sovereignty would be truncated. The peace agreement that would bring Palestine to life would include provisions for binding neutrality, territorial adjustments in Israel's favor, restrictions on the exploitation of water resources, and severe limitations on standing military forces. Jerusalem, the proposed capital of the new state, would remain in Israeli hands, but under a sort of blurred sovereignty arrangement. Simply put, the municipality would be somewhat autonomous of the central government so as to allow a degree of communal independence. As Heller notes, the point is to obfuscate sovereignty "to the point where all parties can credibly claim that they have secured their essential objectives" (p. 120).

Obviously, the major problem is not to outline a settlement, but to achieve one, and Heller's unintended contribution may have been to emphasize how unlikely it is that a reasonable compromise can be attained between the adherents of Palestinian and Israeli nationalism. Neither side seems anywhere close to the solution presented in this book. Israel's governments persistently chase the specter of solutions bypassing the PLO, and the PLO is rife with internal conflict over the very issue of negotiation versus military struggle. The enlightened solution may well be close to Heller's prescription, but non-solution remains the likely prospect.

AUGUSTUS RICHARD NORTON

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American Leadership in World Affairs: Vietnam and the Breakdown of Consensus. By Ole R. Holsti and James N. Rosenau. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. xvi + 301. \$28.50, cloth; \$9.95, paper.)

Considerable discussion about the impact of the Vietnam War on the evolution of American society has taken place in the study of American foreign policy. *American Leadership in World Affairs* by Ole Holsti and James Rosenau represents the first comprehensive, empirically based study of contemporary American foreign policy beliefs.

Holsti and Rosenau examine the evolution of foreign policy beliefs held by American leaders based on two nation-wide surveys conducted in

1976 and 1980. Three major research questions are addressed:

1. What has been the impact of the Vietnam War on America's leaders?
2. If the impact has been dissensus, what are the main lines of cleavage?
3. Have the post-Vietnam beliefs been maintained or have they undergone change over time?

Holsti and Rosenau find that the Vietnam War was a watershed event that shattered the post-World War II consensus in American foreign policy. The result of the war was diversity and disagreement concerning the causes, consequences, and lessons of Vietnam, as well as for issues unrelated to Vietnam.

Although diversity was the norm, Holsti and Rosenau find that three major cleavages or schools of thought developed in the foreign policy beliefs of America's leaders: cold war internationalism, post-cold war internationalism, and semi-isolationism. Cold war internationalism was the foundation of the post-World War II consensus, premised on expansionistic Soviet communism that America needed to contain, principally through the use of force. Post-cold war internationalists perceived a pluralistic international system and downplayed the role of force in its quest to promote a stable and just world order. Semi-isolationists emphasized the difficulty and danger of remaining internationally involved and the need to address key domestic problems. Holsti and Rosenau also find that "Vietnam policy position, ideology, and occupation are most strongly associated with the three foreign policy belief systems, whereas divisions corresponding to gender, generation, and military service are much weaker" (p. 173).

Holsti and Rosenau find that continuity in foreign policy beliefs persist when the 1976 and 1980 leadership surveys are compared, and they conclude that dissensus will continue into the future. As the authors suggest, "perhaps achievement of a consensus is at present beyond the reach of even the most skilled and inspired leadership for the cleavages are by no means limited to foreign policy issues" (p. 244).

As in any volume, there are a few things that could have been refined. There appear to be some ambiguities in the presentation of the findings. For example, the categories of "ambivalent supporters" and "ambivalent critics" are not differentiated clearly. The scales used in presenting the data are also confusing; no explanation is provided for the different types of scales utilized, and the discussion of how the responses were calculated for placement on the scale is insufficient.

A more important point to consider concerns the authors' conclusion that a cold war internationalist consensus existed before Vietnam and that three schools of thought have developed because of Vietnam. In fact, the authors suggest that there is an increasing division within the cold war internationalist group between multilateralists and unilateralists. I would suggest that this division, and therefore actually two competitive schools of thought, have always existed. The multilateralists became dominant following World War II, but have been consistently challenged by the unilateralists during McCarthyism in the fifties, the Goldwater candidacy in the sixties, and the rise of neoconservatism in the seventies and eighties.

In my opinion, a similar situation exists with the post-cold war internationalists. Placing Stanley Hoffmann and Seyom Brown in the same school of thought with Richard Falk and Walter LaFeber omits many important differences. Hoffmann and Brown are concerned with "reforming" U.S. foreign policy away from cold war policies in an effort to promote a stable and just world order. For Falk and LaFeber, reform is insufficient—a "transformation" is necessary in American foreign policy in order to create a truly peaceful and just world order. Unfortunately, Holsti and Rosenau's questionnaire does not allow analysis for these differences within cold war internationalism and post-cold war internationalism.

American Leadership in World Affairs is clearly a significant work and a valuable contribution to the study of American foreign policy. One of its additional benefits is that it brings together most of Holsti and Rosenau's work on this subject. Overall, in order to explain and predict the evolution of American foreign policy, one must understand the change and continuity that has developed in the foreign policy beliefs of American society.

JEREL A. ROSATI

University of South Carolina

Can Modern War Be Just? By James Turner Johnson. (New Haven, Conn.: Yale University Press, 1984. Pp. xi + 215. \$17.95.)

In several previous books Johnson—a Rutgers University professor with appointments in the departments of political science and religion—has written of the historical development of the just war tradition. After summarizing the elements of the tradition, this book explores its present applicability.

Begun by theologians such as Augustine and

Aquinas, elaborated by jurists like Grotius and Vattel, incorporating the practical considerations of soldiers, and partially codified and universalized in twentieth-century international law, the just war tradition suggests some rules supposed to govern resort to war by political leaders (*ius ad bellum* principles) and the conduct of war by soldiers (*ius in bello* principles). The most honorable political leaders and soldiers have embraced these rules and have tried to comply with them.

Johnson asks whether they can continue to do so as we near the end of the twentieth century, the century of total wars, Dresden and Nagasaki, with numerous countries armed with nuclear and biological weaponry that make people an endangered species. "Yes," is his thoughtful reply—if our leaders and soldiers keep the principles in mind as they do their jobs, if the moral values of the tradition are allowed to help drive technological and targeting as well as political and tactical decisions.

Johnson examines five types of recent and possible wars. (1) War between the superpowers involving the use of strategic nuclear weapons aimed at cities would clearly violate the just war tradition; so the superpowers ought to adopt counterforce rather than countervalue targeting doctrines, they ought to press ahead with efforts to develop more accurate and thus discriminating strategic weapons (like cruise missiles), they ought to continue the quest for defenses against nuclear-tipped strategic missiles, and they ought to seek ways to move strategic warfare away from non-combatants (even into space). (2) Conflict in Europe involving the use of tactical nuclear weapons would be very difficult to reconcile with just war principles because of population densities and the dangers of escalation to the level of strategic exchanges; but if the Americans, Russians, and Europeans would replace current tactical nuclear weapons with neutron warheads ("definitely superior morally," p. 47) and increase their conventional warfare capabilities, then maybe this type of conflict could be fought within the rules. (3) Conventional warfare within the limitations can still occur, as the 1982 Falklands war illustrates. (4) Insurgency or revolutionary conflicts are almost inevitably "dirty" (p. 55) because the distinction between combatants and noncombatants is blurred, especially by the insurgents; only when the insurgents are able to fight conventionally can this sort of warfare be fought by both sides in compliance with *ius in bello* norms. (5) Terrorism as a form of political violence is irredeemably repugnant for Johnson because it so typically aims deliberately at injuring noncombatants in order to make its political points.

Johnson recognizes some of the problems with these conclusions and suggestions, and he tries to

grapple with them, not always successfully. But in this book at least, he does not address some other problems with the just war tradition and its current applicability. Johnson's work does not help us much in deciding what to think when the principles of the just war tradition collide with impressive principles of other well-established traditions (revolutionary or pacifist or liberal or humanist or democratic or ecological).

Perhaps this is because Johnson is committed to the dubious proposition that the just war tradition is "the only way actually open for persons in our culture to think about morality and war" (p. 29). Many analysts disagree as they assert that the just war tradition is too permissive or statist, that it is unfair to make yesterday's immune civilian into today's fair game combatant merely by conscripting him, that even warfare conducted by the rules has always been a morally shabby enterprise (perhaps akin to slavery). These critics suggest, too, that today's technology and the operation of human nature and Murphy's Law make modern warfare essentially impossible to control morally, that rather than straining to find ways to clean up warfare so it can be continued as a final arbiter of dubious respectability Johnson and other supporters of the just war tradition might better spend their energies developing or supporting alternatives to obsolescent if not yet obsolete warfare (e.g., commercial inducements and deals, diplomacy, bribery, assassination, civilian resistance, and system transformation).

JAMES A. STEGENGA

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Central America: Anatomy of Conflict. Edited by Robert S. Leiken. (New York: Pergamon Press, 1984. Pp. ix + 351. \$19.95.)

This book is the result of meetings of former government officials, diplomats, scholars, and policy analysts assembled by the Carnegie Endowment for International Peace to contribute to the national debate on Central America. In the Preface, Leiken apologizes for the speed with which the essays were produced and for the fact that the whole reflects the "collective dismay" of Central American specialists and does not constitute a "collective work."

In fact, no apologies are necessary. There are indeed a great many questions to be considered, but the book's purpose is fully realized, and it is as useful a study of its subject as any that has appeared in print. The 15 essays represent a diversity of views and employ an eclectic array of sources including archives, personal interviews,

and popular journals. Their quality is consistently high.

The book is organized into four parts. In the first, "Legacies and Contexts," Latin political culture, the history of U.S. intervention in Central America, and the role of the militaries in the region are reviewed. Most of the essential points are covered and occasionally, such as when Christopher Dickey is discussing the tradition of obedience without compliance, one wishes there were more. The second part of the book, "Socialism and Sovietism," does an admirable job of establishing the complex origins of Sandinista foreign policy and the crucial differences between the Sandinistas and the Salvadoran Left. One was stunted by the vacuousness of its political culture, the other was nurtured in a much richer medium resulting in differences in "political experience, ideological diversity, and attitudes towards the superpowers" (p. 114). Although some few reservations can be expressed (Do two selections want to rely on the same Sandinista defector for some important information, given the level of intrigue and fratricidal hatreds known to exist and which the essays themselves corroborate? Does the essay on the Soviets and Central America sometimes make too much of fairly innocuous public statements in Moscow?), these are minor distractions.

"Economic and Military Realities," the third section of the book, contains a particularly creative attempt by Theodore Moran to determine the "base-line cost estimates" of four alternative policy scenarios for the United States in El Salvador. With appreciation of the tenuousness of such an exercise and with deference to the many variables, Moran concludes that the cost of continuing current U.S. policies for the next five years would be the same as the cost of seeking a negotiated solution (\$4 billion) and much less than the cost of attempting either a military solution without U.S. combat troops (\$7 billion) or an invasion and occupation of Nicaragua (\$16 billion). For all of the acknowledged difficulties of such an undertaking, this essay contains precisely the kinds of considerations that should figure prominently in the public debate. This section also offers a sober but optimistic assessment of the potential of an economic program for postwar Central America costing far less (as percentage of U.S. GNP) than the original Marshall Plan and having a much greater impact.

The final part of this book offers five essays on the U.S. policy process. Although little new ground is broken, the analyses are skillful, and selections by Howard Wiarda ("Conceptual Failures in U.S.-Central American Relations") and by I. M. Destler (on the "elusive consensus" on a foreign policy) are especially commendable.

The contributors to this book have crossed the

minefield that is Central American scholarship with admirable skill to produce a study of great vitality and sagacity. Developments since the essays were written generally reinforce conclusions so that one can expect that this study will serve a diverse readership for some time.

ALBERT P. VANNUCCI

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The Oil Decade: Conflict and Cooperation in the West. By Robert J. Lieber. (New York: Praeger Publishers, 1983. Pp. ix + 159. \$23.95.)

At a time when the world oil market is experiencing a prolonged glut and when the Organization of Petroleum Exporting Countries (OPEC) is encountering considerable difficulty either in maintaining its production programming agreement of 1983 or in abiding by its official price of \$29 per barrel, reading Robert Lieber's book is a refreshing experience because it warns the Western industrial countries against becoming overly complacent concerning energy affairs.

As stated in its subtitle, this study is chiefly concerned with the issues of conflict and cooperation among members of the Organization for Economic Cooperation and Development (OECD) and with their struggle to cope with the consequences of their failure to cooperate with each other. In the first four chapters, Lieber lucidly lays out the background of the energy crises of the 1970s and delineates the dynamics of responses of the Western industrial consuming nations to these crises. The first oil shock, that is, the Arab oil embargo imposed by the Organization of the Arab Petroleum Exporting Countries (OAPEC) immediately after the Arab-Israeli War of 1973, caused disarray among the OECD countries, a majority of which had been substantially dependent on Middle East oil supplies since the 1960s. Even the establishment of the International Energy Agency (IEA) was preceded by considerable squabbling between European members such as France and Britain on the one hand, and the United States on the other. Lieber, quite correctly, lists supply-related anxieties, economic imperatives, export competition, differences between allied energy producers and consumers, and divergent beliefs about problems as sources of conflict among the OECD members on the energy issue.

According to Lieber, during the five-year interlude between the first and second oil shocks (i.e., between 1974 and 1978), the Western oil consumers failed to take significant steps to reduce their energy consumption. In fact, the impasse

over energy policy that remained in the United States, even though energy consumption there increased by 28.5%, caused considerable resentment among the rest of the OECD members. Although the latter countries adopted small but significant measures encouraging modest energy conservation, these measures were of limited outcome and were implemented as a result of national rather than collective policies. The economies of the OECD countries also experienced another major shock as a result of energy shortages stemming from political revolution in Iran in late 1978 and early 1979. When faced with energy shortages emanating from the Iran-Iraq War toward the end of 1980, however, the Western industrial states were not only well prepared, but they also readily cooperated in mitigating the deleterious effects of these shortages on their economies.

Lieber's expertise on energy affairs is quite apparent in the ease with which he makes insightful observations. For example, he notes that of all the causes of conflict that remain as national obstacles in the way of the development of coherent and collective action for the OECD countries, divergent economic interests are particularly acute. In order to underscore the divisive nature of these interests, Lieber states that coordination in oil demand restraint and in the management of export competition may be even more difficult than agreement on diplomatic measures or military security cooperation. The book is peppered with similar comments.

As one of the major recommendations to the OECD countries, Lieber suggests that they adopt a mix of incremental policy choices which, in the long run, are aimed at tackling the long-term problem of reducing Western dependence on Middle East oil. His other suggestions include diversification of sources of imports, enhancement of production and fuel-switching, and attainment of energy efficiency through the use of market mechanisms as well as conscious policy measures. To lessen vulnerability, according to Lieber, the Western consumers must increase stockpiling of oil and other fuels, identify and develop production surge capacity, and continue to improve emergency planning at both domestic and international levels. Moreover, Lieber states that the OECD countries must also strive for "international economic coordination to alleviate both long-term and crisis problems" (p. 140).

In general, the study is well reasoned and well documented. Lieber is considerably at ease in dealing with the European countries. The case study of France is one of the two best chapters of this book, the second being the chapter on scenarios of energy crisis. The glaring flaw of this study is the scant attention paid to the issue of

Japanese energy policy. The chapter on the U.S. energy policy is the weakest because Lieber pays too much attention to limitations of space and fails to treat the intricacies of the energy policy process with the detail and the care that they deserve. Overall, Lieber's book is a noteworthy contribution to the extant literature on energy, and it should be read by all serious students of energy affairs.

M. E. AHRARI

East Carolina University

Détente and the Nixon Doctrine: American Foreign Policy and the Pursuit of Stability, 1969-1976. By Robert S. Litwak. (New York: Cambridge University Press, 1984. Pp. vii + 232. \$24.95.)

Robert Litwak's *Détente and the Nixon Doctrine* is a subtly argued and largely convincing interpretation of American foreign policy. Two assumptions are central to his argument: first, that a "nation-state's image of the world . . . is crucial in the delineation and articulation of its interests and values" (p. 6); and second, that the fundamental problem facing American policymakers in the postwar period has been the "integrating [of] force and policy in the nuclear age" (p. 7). From this starting point, Litwak argues that before 1969, U.S. foreign policy was driven by American universalism framed by a bipolar, Manichean image of the international system. The ends of American policy—the containment of global communism—were unquestioned, and attention was "invariably focused on the requisite means of containment" (p. 22). The cold war consensus on both the ends and instruments of American foreign policy collapsed in 1968 in the aftermath of the Tet Offensive and in response to qualitative changes taking place in the international system.

Litwak's thesis is that the Nixon administration implemented a foreign policy strategy of superpower détente and politico-military retrenchment (the Nixon Doctrine) to meet the conflicting policy imperatives of a "continued (indeed revitalized) military bipolarity with the new conditions of global pluralism" (p. 2). This foreign policy strategy sought to avoid the extremes of undifferentiated globalism and disengagement that had characterized American policy in the past.

The key departure of the Nixon administration was the Nixon Doctrine, which represented "a wholly new *philosophy* to govern American security policy towards the Third World during the post-Vietnam era" (p. 120). The devolution of American military responsibilities (although not

commitments) along the Afro-Asian periphery was to be guaranteed by superpower détente that would create the necessary conditions enabling it to devolve its responsibilities to regional surrogates. Détente and devolution were conceived to be interdependent and mutually reinforcing. Thus, détente would allow the United States to extract itself from Vietnam, and the Nixon Doctrine (Vietnamization) would preclude a shift in the balance of power along the periphery that could prejudice its core relationship with the Soviet Union.

The interdependence of the core and the periphery, and the role attributed to regional surrogates in the Nixon-Kissinger foreign policy strategy, is the central focus of Litwak's study. He examines and provides a credible analysis of American policy in Vietnam before the Paris Accords, during the Indo-Pakistani War, and of administration efforts to court regional powers (e.g., Iran, Brazil, and Indonesia).

Litwak characterizes the strategy of détente and devolution as a transitory policy—designed to meet the conflicting policy imperatives of nascent multipolarity and military bipolarity—that signified a break with the postwar policy initiated by the Truman Doctrine. After the near confrontation between the superpowers during the Arab-Israeli War in October, 1973, Nixon and Kissinger jettisoned the pentapolar vision of the international system elucidated in July, 1971, and U.S. policy is judged to have reverted to the containment of Soviet expansionism via the differentiation of means much like previous administrations. Domestic developments, however, hamstrung the Ford administration's foreign policy. The Congress, reasserting usurped foreign policy prerogatives, and the presidency, sapped of its institutional authority by Watergate, disagreed on two fundamental issues: the utility of force as an instrument of policy and the centrality of the East-West conflict in the Third World. The Congress discounted both heavily, controlled the purse strings, and consequently U.S. policy as well. In this way, Litwak handily explains the events leading to the fall of Saigon and American passivity during the Angolan Civil War.

Litwak concludes that the United States should sustain a global role, but that role must be based upon a "coherent, non-universalistic conception of US national interests" if it is to command a domestic consensus essential for a successful foreign policy (p. 198). The role the United States should play in the international system is open to debate, but it is certain that any foreign policy strategy that does not wed American interests to American ideals will fail to forge a domestic consensus on either the ends or instruments of American policy. Perhaps European scholars (Litwak

received his Ph.D. at the London School of Economics) should abandon their hope that Americans will practice anything other than an American statecraft and concentrate instead on understanding it rather than trying to Europeanize it.

Finally, the author's excessive reliance upon italicization and foreign terms is distracting, often unnecessary, and in at least one instance incorrect (*Machtpolitik* rather than *Macht Politik*, pp. 62, 108). Nonetheless, *Détente and the Nixon Doctrine* is an important contribution to the historiography of American foreign policy during the Nixon and Ford administrations.

JAMES CLYDE SPERLING

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The War Everyone Lost—and Won: America's Intervention in Vietnam's Twin Struggles. By Timothy J. Lomperis. (Baton Rouge: Louisiana State University Press, 1984. Pp. x + 192. \$22.50.)

Basing his views on familiar English-language historical studies, journalistic accounts, and government documents, Lomperis draws on his experience as an intelligence analyst in Saigon and on his more recent academic experience in the Ph.D. program at Duke University to advance an interesting thesis—that the communists won their struggle for national liberation while America won its struggle to defeat the communist effort to establish the international legitimacy of its Peoples War strategy.

In chapter 1, Lomperis relies on respected historical studies to trace the struggles of Vietnamese patriots over many centuries to gain traditional legitimacy as rulers by expelling foreigners and promoting land reform. Persuasively presenting relevant information on important political-military events, he describes how the Vietnamese communists in this century were able to counter their ideological opponents, claim the traditional Vietnamese role of freedom fighters, and employ both traditional and modern methods to expel the French and assume ruling power.

In chapter 2, Lomperis interprets the confusing developments of the 1960 to 1975 American period clearly and parsimoniously. Describing America's intervention as conceived in ignorance and aimed at preventing the communists from completing their historic struggle for independence, he tells a compelling story of American military-political operations and failures. He represents the communists as achieving victory by

successfully controlling the pace of the war, maintaining the political-military initiative, rejecting the unwelcome advice of China and Russia, avoiding intolerable levels of physical destruction, supporting southern communists, and discouraging—by launching stunning and timely military attacks—the willingness of American citizens to pursue the struggle. He depicts Americans as failing either to create a capable South Vietnamese counterforce or to develop a useful system for monitoring the war but as succeeding in halting the communist offensives of 1968 and 1972 and at expanding pacification programs from 1969 to 1972.

In chapter 3, the author reviews the development of communist ideology in Asia and Vietnam; describes the changes that led to its increased reliance upon the peasantry, nationalism, and land reform; and portrays the Vietnamese politburo as a practical, innovative, and cohesive team of multitalented leaders who organized over several decades around their own Vietnamese version of Marxism-Leninism. In chapter 4, Lomperis analyzes the Peoples War strategy first conceived by Chinese theorists and describes it as composed of a flowing variety of interacting, overlapping, and ill-defined tactics and stages; as involving a confusing mix of military, political, and diplomatic resources changing in relation to enemy postures and operations; and as aimed at achieving political liberation by outlasting a militarily stronger but politically weaker enemy. He represents the pragmatic Vietnamese leaders as borrowing their version of Peoples War strategy from the Chinese and debating and changing it to meet their different needs, that is, to fight a different war of Vietnamese liberation against different enemies, at different places and times, and under different conditions. According to Lomperis, Vietnamese revolutionaries cannot legitimately claim to have gained liberation by means of a Peoples War strategy because they failed to implement what he believes to be the key features of such a strategy as laid down and promised by orthodox Chinese and Vietnamese theorists. Upon examining the strategy that the Vietnamese implemented on the battlefield, he argues that they suffered such heavy losses to their guerilla forces at the hands of Americans in 1968 that they subsequently had to abandon their Peoples War strategy. The communists thereby failed to demonstrate the legitimacy of their strategy for emulation by other revolutionaries.

Although I find Lomperis's arguments on behalf of the above thesis unconvincing, they are seriously presented and may well provoke interesting reactions among knowledgeable students of both Vietnamese history and the war and may stimulate additional research based on new

primary data from both Vietnamese and American government sources.

JOHN JAMES MACDOUGALL

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Arms Control: The Multilateral Alternative.

Edited by Edward C. Luck. (New York: New York University Press, 1983. Pp. xii + 258. \$32.50, cloth; \$15.00, paper.)

In a series of five meetings from February through July, 1982, a distinguished 36-member study group of the United Nations Association—USA, chaired by Ambassador James F. Leonard, discussed drafts of the 10 expert papers that comprise this volume. UNA-USA commissioned the papers to initiate a dialogue between government officials and selected specialists concerning the future role of the United States in multilateral approaches to arms control. The experts were themselves a distinguished group of academics and practitioners, accustomed to analyzing and coping with the real world of arms control. Their collective product could be called a textbook in practical arms controlling, that is, policy-relevant analysis. Despite their positive, suggestive, and hopeful expectations regarding long-run outcomes of multilateral arms control strategies, the reader cannot escape the thread of short-run pessimism that stretches throughout the book.

Written while the United States was preparing for the second United Nations Special Session on Disarmament in 1982, the expert papers emphasize policy alternatives that would have been particularly relevant for that occasion. In doing so, the authors draw upon all the postwar arms control experience that involved three or more nations negotiating in global, regional, or functional multilateral forums. The book describes the historical and political evolution of multilateralism; the major international forces such as U.S. allies, the Soviet Union, and the nonaligned states with which U.S. policies must deal; and several specific policy areas—nuclear nonproliferation, conventional arms control, and confidence-building measures—particularly needful of multilateral approaches.

A basic observation made by several contributors is that the United States has lacked a clear and consistent strategy for advancing its own and international security through multilateral bodies. Most of the American effort has been focused on tactical and procedural issues, reflecting the official skepticism about the effectiveness of multilateral bodies.

The rise and decline of multilateralism in the

arms control field since 1963 is recounted in the first part of the book. Revival of these multilateral efforts is, according to several of the authors, primarily contingent upon improvement in Soviet-American political relations. Whether or not this requirement is met, it is in the U.S. interest to adopt an innovative, activist posture in multilateral bodies. "A more ambitious U.S. approach would both advance U.S. interests and improve the performance of the multilateral mechanisms themselves" (p. 5) according to these same authors. All agree that multilateral arms control issues are best studied and debated in United Nations' forums.

Another group of essays describes and analyzes the global political environment within which multilateralism is pursued. The authors agree that political rather than technical or military conditions are most influential in determining progress in arms limitation agreements. The book is particularly instructive in the map it draws of some of these complex and often intractable political conditions as they criss-cross among East-West, North-South, regional, and intra-alliance relationships.

For the United States, this complexity arises from relationships with allies in Western Europe and Japan, the Soviet Union and its allies, and the nonaligned states. U.S. alliance problems tend to vary with subject matter and negotiating forums, particularly when an alliance partner (all of them are democracies) must respond to the exigencies of domestic politics. Problems with the Soviet Union, despite that nation's maturing diplomatic methods and participation in several multilateral agreements, are likely to remain so long as the Soviets use the multilateral process for other purposes such as ideological leadership of the communist camp and recruitment of allies among the developing countries. Relations with the nonaligned states will vary from subgroup to subgroup, some of which are led, despite the angry third-world rhetoric, by policymakers with sophisticated security and political objectives such as the maintenance of internal order, the diminution of superpower influence, and the pursuit of regional power.

The essays that examine three specific policy areas add to the pessimistic tone of the discussion. One author feels that the international consensus against further nuclear proliferation may be breaking down. Another believes that the problem of conventional arms limitation is much too low on the U.S. public agenda. A third deems many of the benefits of confidence-building measures to be overstated and many of the liabilities overlooked. Noting the failure of the second Special Session on Disarmament, another contributor suggests shifting from a search for global

consensus to a greater focus on regional arms control problems.

Most of the policies recommended throughout the book are modest and feasible, for example, in the absence of arms transfer statistics published by the United Nations, the United States should make an annual report of its own data on this subject for submission to the Secretary General and distribution among all the members. The recommended policies are realistic responses to the glacial pace of institutional progress in the arms control field. A president dedicated to the arms control process could make most of them happen in very short order. From another perspective, the book as a whole could become an antidote for disarmament visionaries who, in exasperation, will probably—and unfortunately—consider it “minimalist.”

RALPH M. GOLDMAN

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Conventional Deterrence. By John J. Mearsheimer. (Ithaca, N.Y.: Cornell University Press, 1983. Pp. 296. \$29.50.)

John J. Mearsheimer's *Conventional Deterrence* is a complex work on an often forgotten subject. Although nuclear deterrence has been researched at length, few scholars have made the effort to investigate deterrence by conventional military means such as battlefield artillery, tanks, and infantry. Although this book in no way espouses the beliefs of the nuclear freeze movement, its findings could support a new emphasis on a strong conventional military. This well-documented book is a result of solid research and will be considered the definitive new work on the subject.

The book describes deterrence failures that resulted in conventional battlefield wars in which the parties involved were two large armies in direct confrontation in a large area. Guerilla, naval, and air warfare are excluded. Mearsheimer uses the comparative case study method to examine not only the deterrence failures, but also its successes in the following examples (pp. 19-20):

- British-French decision making at Munich (1938)
- British-French decision making in 1939-1940
- The German decision to attack Poland (1939)
- The German decision to attack France (1940)
- The German decision to attack the Soviet Union (1941)
- The Soviet decision to strike against Japan's Kwantung Army in Manchuria (1945)
- The Israeli decision to attack in the Sinai (1956)
- The Israeli decision to strike in the Sinai (1967)
- The Egyptian decision to attack Israel (1973)

The North Korean invasion of South Korea (1950)

The Indian decision to attack Pakistan (1971)

The Vietnamese invasion of Cambodia (1979)

Mearsheimer's conventional deterrence theory is clearly linked to military strategy. In the planning stage, decision makers try to study the proposed strategy of how the armed forces will be used successfully in the battlefield. Decision makers seeking conventional deterrence will be confronted with three alternative strategies: attrition, blitzkrieg, and limited arms. Blitzkrieg leads the list in the area of deterrence failures because it is a relatively inexpensive strategy and one that could result in a rapid victory. Attrition, which is long and protracted conflict, tends to be an expensive strategy that leads to success in deterrence. Limited arms strategy has the drawback of turning a conflict into a war of attrition and tends not to be a good strategy of deterrence.

Providing supporting evidence for the theory with case studies, Mearsheimer shows that deterrence worked when the Allies chose not to attack Germany in 1939 and 1940 because they believed that Germany would launch a war of attrition as it had in World War I; that Germany was not deterred from attacking the Allies in 1939 and 1940 when it used the blitzkrieg strategy; that deterrence failed when blitzkrieg and attrition strategies were used for deterrence resulting in the Arab-Israeli wars of October 1956, June 1967, and October 1973; and that to date, the Soviets have been successfully deterred by a blitzkrieg strategy and possibly a subsequent attrition strategy.

The development of very accurate weapons known as precision-guided munitions has changed the nature of battlefield deterrence and diminished the likelihood of the use of the blitzkrieg strategy. Shoulder-launched antitank missiles have changed the conditions of the battlefield. Limited aims strategy could be improved by use of such munitions.

Conventional Deterrence argues that deterrence will not be successful if one side is able to use the blitzkrieg strategy. Through the use of very highly technical details about battlefield strategy, Mearsheimer proves that an attacking nation can use any of the three strategies that will each have a different effect on deterrence.

MICHAEL A. PREDA

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The Dynamics of Nuclear Proliferation. By Stephen M. Meyer. (Chicago: University of Chicago Press, 1984. Pp. vii + 229. \$20.00.)

Why do governments acquire nuclear weapons? In this stimulating and insightful book Stephen M. Meyer challenges conventional wisdom to answer this question. In so doing he takes on the technological imperative school of thought that asserts that "once a nation acquires the capability to manufacture nuclear weaponry (has latent capacity) it will inevitably do so. . ." (p. 9). Arguing that such sentiment "unfortunately . . . led large segments of the academic and policy communities to perceive nuclear weapons proliferation as almost entirely a technological problem—much like air pollution or toxic waste disposal" (p. 4), Meyer contends that motivational constraints and inducements have been more compelling since the mid 1950s.

Marshalling his argument, Meyer applies quantitative and qualitative methodologies to aggregate data and country case studies to make a telling point: Since the dawn of the nuclear age, 36 nations have acquired a latent nuclear weapons capability. Leaving aside the arguable validity of the measures Meyer uses to construct his list, it is uncontested that for some time states such as West Germany, Italy, Canada, Japan, and Sweden have had infrastructures capable of manufacturing nuclear weapons. But unlike the United States, the Soviet Union, and Britain, which developed the technology upon acquisition of the wherewithal, most of these nations resisted the nuclear weapons option. This leads Meyer to conclude that "one cannot distinguish between countries that decide to 'go nuclear' and those that do not on the basis of nuclear technology" (p. 88). Indeed, the past decades suggest a decline in the rate of proliferation. The statistical data further point to the probability that if a nation does not acquire nuclear weapons within 10 years of its acquisition of a latent capability, it is unlikely to do so at all.

Given the temptation of nuclear arms for purposes of security, prestige, politics, and economics, why do nations resist? Although treaties, fear of preemptive strike by a major nuclear power, and peaceful reputation have moderate dissuasive effects, Meyer finds alliance to a nuclear ally to be most compelling. Will this and other restraints be sufficient to halt proliferation? Meyer is not sanguine. But it is at this point—trying to look into the future—that the work runs into its greatest difficulty, and understandably so given the notorious inadequacy of past nuclear proliferation forecasts. Notwithstanding his own wariness of behavioral laws, Meyer cannot resist the role of soothsayer that characterizes most foreign policy

analysts. He suggests that "a proliferation decision is to be expected" (p. 155) from Argentina in light of its humiliation in the Falklands War. Yet as of early 1985, most analysts would agree that if anything, the civilian Alfonsín government is less disposed toward nuclear weapons than its military predecessors.

Still, other "hard core" (p. 160) proliferants are foreseeable, namely Pakistan, Iraq, and Libya. How is the international community to diminish their propensity for nuclear weapons? Ironically given his own dubious view of the technological imperative school, but realistically all the same, Meyer calls for technical constraints to buy time to change motivational profiles. At the same time Meyer acknowledges that, given the dissemination of nuclear knowledge, such barriers are bound to break down; eventually many nations will be able to build their own atomic plants. By implication, Meyer suggests that nuclear proliferation may be inevitable given the difficulty in modifying motivational profiles. However, such inevitability in the Middle East, to cite one dangerous region, would have to contend with a strategy of proliferation control that Meyer does not dwell on, the ingenuity of Israeli military planners who demonstrated in their 1981 attack on Iraq's research reactor that motivational modification, alliances, treaties, safeguards, and export controls are not the only proliferation remedies.

BENNETT RAMBERG

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Controlling Latin American Conflicts: Ten Approaches. Edited by Michael A. Morris and Victor Millán. (Boulder, Colo.: Westview Press, 1983. Pp. xiii + 272. \$22.50, paper.)

Scholarly preoccupation with armed conflict in Latin America remains high despite the comparative paucity of external wars in this region and the greater aggregate death and destruction from internecine combat, guerilla insurgency, terrorism and state repression, and armed forces' insurrection. Despite their acknowledgement of a "legalistic" tradition of conflict containment in Latin America (p. 1), Morris and Millán contend that a recent upsurge in violence, epitomized by the 1982 Malvinas war, coupled with a growing arms race and a decline in legitimacy of the OAS, provoke concern over prospects for major conflagration.

Nine contributors provide a timely and original volume that divides conflicts into national, regional, functional, sectoral, and external categories of origination. The editors' introduction

establishes a conceptual framework for the subsequent case studies.

Predicating that Latin American conflicts range from broadly systemic and ideological to particularistic (e.g., resources, boundary, or refugee disputes), Morris and Millán assert that "multicausal, overlapping conflicts" (p. 5), such as those generated by revolution in Central America, are highly impervious to legalistic solution and have the greatest likelihood of becoming internationalized. The essays by Juan Carlos Puig on juridical trends, Carlos Moneta on economic community, Michael Morris and Martin Slann on arms acquisition, and Max Manwaring on the implications of military expansion most faithfully exploit this framework.

Puig argues that insufficient attention is paid to international law regimes in shaping state conduct. Latin American history demonstrates that resistance to conflict arbitration can be overcome if there is consensus between government and citizenry over the legitimate application of legal principle and if international jurists are perceived as juridically competent. Puig implies that Argentina's resistance to arbitration in the Beagle Channel and Malvinas disputes would exemplify governmental manipulation of international conflict to achieve domestic consensus (p. 25).

Moneta, a consultant to the Latin American Economic Organization (SELA), assesses the potential of this little-known institution as a "centripital force" (p. 113) toward functional integration. Although SELA's success has thus far been modest, its vicissitudes confirm the apprehensions of international integration theorists. Although Mexico and Venezuela—the region's fastest growing economies—have become SELA's greatest boosters (p. 101), its continued evolution as a distinctly Latin American regional advocate hinges upon further development of member states' economic capacities.

Morris and Slann's analysis of weapons acquisition policy and the arms control regime in Latin America leads them to conclude that arms suppliers have a stake in stability to assure profits. Moreover, the decline of the United States as an arms supplier reduces its competence to influence arms control. Meanwhile, the region's potential nuclear powers are reluctant to break the weapons threshold due to cost considerations and fear of tipping an established balance of power (p. 125).

Manwaring's study of the growth of Latin American military capabilities constitutes the collection's most methodologically sophisticated contribution. Through a pathbreaking macro-level factor analysis of armed forces strength, transport capacity, and defense infrastructure, he concludes that a dramatic imbalance in military power now exists among the region's larger coun-

tries and that some states may view nuclear proliferation as an attractive means of restoring influence (p. 174). Although a provocative analysis, additional factors such as the following should be included in this model to improve its predictive ability: the influence of population demographics upon military expansionism, health and welfare as important factors in protracted warfare, and tax effort and social service capacity as measures of penetrative capability.

Notwithstanding some unevenness among the contributions, this is a valuable collection for peace researchers and students of comparative foreign policy. There is a broad consensus among the contributors on the increasing self-assertiveness of Latin America and on the role of the Malvinas dispute as a milestone in hastening interstate cooperation (Moneta), inhibiting arms control (Augusto Varas, Morris and Slann), and illustrating the futility of equivocal negotiations (Ruben de Hoyos). An up-to-date bibliography on Latin American conflict resolution considerably enhances its value for scholars.

DAVID LEWIS FELDMAN

Moorhead State University

Soviet Allies: The Warsaw Pact and the Issue of Reliability. Edited by Daniel N. Nelson. (Boulder, Colo.: Westview Press, 1984. Pp. xiii + 273. \$22.50.)

The author and six colleagues specify the variables thought to affect Warsaw Pact reliability and address the measurement of these variables, then analyze the changing Warsaw pact alliance structure, the relationship of East European military establishments to the USSR, and the interplay of Soviet and East European security concerns. In the case of the six "allies," the authors assess domestic-level variables (e.g., party-military relations, domestic political conditions, material and manpower resources, economic performance) and their relationship to the mobilization potential of Eastern Europe.

In the introduction, Nelson views mobilization potential in terms of a political system's ability to ensure efficient compliance when extracting manpower and material resources from its society and economy, only part of which is the military *per se*. He sees mobilization potential as the principal limitation on force availability, and thus the key determinant of reliability. He then introduces six indicators by which to rate Warsaw Pact reliability: (1) scenario of hostility (combat in homeland or in other countries); (2) duration of involvement in hostile action; (3) systemic integration (of each Pact state with the USSR); (4) domestic socio-

economic conditions; (5) domestic political conditions; and (6) military preparedness.

As to scenario, Nelson believes that only very select East German and Polish units would be involved in the initial Soviet attack on the German central front. On duration, the longer the hostilities and the more removed from their homelands, the less reliable Pact forces become. On systemic integration, the East German and Bulgarian forces rate best. As for socioeconomic factors, Nelson sees the Czechs, Poles, and Hungarians as the least reliable. Politically, the Poles, Czechs, and East German regimes "must confront the most volatile populations" (p. 28). And on military preparedness, the Soviets have favored select units of the three Northern Tier states (East German, Polish, and Czech). The concentration of Warsaw Pact mobility and firepower is in the Northern Tier.

Nelson concludes that only East Germany and Bulgaria are "reliable," but the Bulgarians are remote from the Central Front. Poland is the weak link in the Northern Tier, and the Czechs and Hungarians might only perform well on their homelands. Romania's mobilization potential for Soviet military adventures "is virtually nonexistent." Only East Germany exhibits consistently "a high level of systemic integration, a developed and productive economy, political control and military preparation" (p. 265). But this most reliably ally "has a bothersome record of political unrest and a persistent, disaffected minority" (p. 266).

Soviet forces stationed in Eastern Europe, Byelorussia, and the Ukraine are strong enough to deal with disaffection, and Nelson believes that the Warsaw Pact is "more a symbol of Soviet weakness than of Soviet strength." The Warsaw Pact "has now become an entity fraught with indications that communist party regimes cannot be assured of their own militaries" (p. 267).

The notes at the end of each chapter constitute a gold mine of references from both sides of the Iron Curtain.

ANTHONY TRAWICK BOUSCAREN

Le Moyne College

The Antinomies of Interdependence: National Welfare and the International Division of Labor. Edited by John Gerard Ruggie. (New York: Columbia University Press, 1983. Pp. xiv + 506. \$35.00, cloth; \$15.00, paper.)

This collection of original essays, including a long introduction and conclusion by the editor, has as its stated aim the production of a political economy of human welfare focusing on the kinds

of policy options third-world countries can adopt to maximize welfare gains for their citizens in the current international division of labor. It is a good collection, and its strengths are numerous. Above all, it is a well-balanced discussion of the *new* international division of labor (NIDL).

To date, such discussion has been from one of two competing perspectives. The first, informed by neoclassical economics, has been a largely uncritical eulogy to the role of "comparative advantage" in explaining the rise of the newly industrializing countries (NICs) and a new era in North-South relations. The second, informed by *neo-Marxism*, sees the NIDL as merely one more stage in the process of capitalist accumulation. This collection is a splendid critique of these, respectively, voluntarist and determinist positions. From these two theoretical positions two basic policy options do, however, emerge. Ruggie calls these "associative" and "dissociative" postures, and they themselves form the book's division of labor.

In Part 1, chapters on American development from 1776 to 1860, small European states, South Korea, Venezuela, and Ivory Coast examine how the management of economic association helps or hinders welfare distribution. In Part 2, three chapters examine respectively the pros and cons of technological, linguistic, and security dissociation. It is always difficult for collected anthologies to be genuinely comparative, but to the extent that the essays in Part 1 represent a good mix of countries and the authors have stuck to the task of asking similar questions about the international contexts of development and specifically the role of the state in this process, then this collection is as good as one is likely to get. The same can also be said for the thematic chapters of Part 2.

The central argument of the collection is that the international division of labor, in its response to welfare demands from developing countries, is "not really as elastic as its advocates assume, but not quite as rigid as its detractors insist" (p. 482). In addition, the abilities of developing countries to secure the best welfare distribution from this division are often more dependent on the political structures than rigid adherence to specific strategies predicted on one or another set of macroeconomic assumptions—be they "associative" or "dissociative." Nowhere is the importance of appropriate political infrastructure better highlighted than in Peter Katzenstein's chapter on economic dependence and corporatism in small European states and the discussion of "state-led" industrialization in South Korea. Both chapters support Ruggie's general assertion that governments pick up "market signals . . . then utilize a battery of interventionist techniques to maintain and diversify the basis of their comparative advantage" (p. 18).

By contrast, Part 2, although at pains to stress the difficulties attached to attempted dissociation, leads Ruggie to draw the conclusion for us that intervention to secure greater association similar to the strategies of the NICs discussed in Part 1 might not always be in the best interests of all states of the Third World. Although there may well be a NIDL characterized by systemic shifts in patterns of industrial production, trade, and finance, these should not be overstated and certainly not seen as universal. The history of the Asian NICs, for example, is unlikely to be repeated by all states on the lower rungs of the current international division of labor, and this book is a splendid reminder of the complexity and diversity of these processes currently in train.

RICHARD HIGGOTT

Murdoch University (Western Australia)

NATO's Nuclear Dilemmas. By David N. Schwartz. (Washington, D.C.: Brookings Institution, 1983. Pp. x + 270. \$28.95, cloth; \$10.95, paper.)

The North Atlantic Treaty Organization (NATO) was founded in 1949. The key article of the founding agreement, Article V, stated that the parties would consider "that an armed attack against one or more of them in Europe or North America shall be considered an attack against them all." In the event of such an attack, Treaty members would take action, "including the use of armed force, to restore and maintain the security of the North Atlantic Area" (*NATO Facts and Figures*, Appendix 2 [Brussels: NATO Information Service, 1978]).

From that time, NATO has faced the issue of how to implement the pledge of Article V. On January 12, 1954, Secretary of State John F. Dulles unveiled the nuclear deterrence doctrine of "massive retaliation." While NATO has since relied on the American strategic nuclear arsenal to deter Warsaw Pact aggression, the deployment of nuclear weapons on both sides has raised the costs of policy error for all.

The December, 1979 decision by NATO to engage the Soviet Union in arms control talks and to deploy Pershing II medium-range ballistic missiles and ground-launched cruise missiles, according to David N. Schwartz, was the latest response to complex and unpleasant choices about the role of nuclear weapons in NATO defense policy. *NATO's Nuclear Dilemmas* is a well-written and incisive analysis of those difficult choices. Schwartz makes no claim to writing the definitive NATO history. His general purpose is to examine the way NATO nations deal with ambiguities, dis-

agreements, and uncertainties surrounding nuclear deterrence. The specific purpose is to describe how NATO arrived at the December, 1979 decision.

Schwartz hopes to help policymakers learn from the past by presenting a historical analysis of NATO's nuclear choices. He notes that some aspects of the December, 1979 decision—to modernize NATO strategic weapons while also pursuing arms control talks—are incomprehensible unless seen in broad perspective. *NATO's Nuclear Dilemmas* encompasses seven chapters. The main part of the book examines "episodes in which the United States initiated action in part to address NATO's nuclear dilemmas" (p. 11). These episodes include the U.S. offer of Thor and Jupiter intermediate-range ballistic missiles to the allies in December 1957; General Lauris Norstad's plans to create a NATO mobile land-based medium-range ballistic missile force; the U.S. effort to create a multilateral sea-based medium-range ballistic missile force for NATO between 1960 and 1965; the U.S. attempt between 1962 and 1967 to have NATO adopt a strategy of flexible response; and the creation of the NATO Nuclear Planning Group.

These events are considered together because they were initiated by the United States to assuage NATO's uneasiness over the American nuclear guarantee. Schwartz analyzes these events by explaining why the U.S. proceeded with each initiative, elucidating the Europeans' responses and describing the reasons for each initiative's success or failure.

NATO's Nuclear Dilemmas can be used for upper-division undergraduate and introductory graduate courses in foreign and defense policy. It also could be used for courses on organizational behavior because it provides great examples of the interplay of multiple values and uncertainty in decision making.

MARK D. MANDELES

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The Military Origins of Industrialisation and International Trade Rivalry. By Gautam Sen. (New York: St. Martin's Press, 1984. Pp. vii + 277. \$30.00.)

Trade disputes are complex and worthwhile analyzing. Although Sen states that his major concern is to explain the occurrence of such conflicts in the area of manufacturing products, this volume is particularly intriguing in the way it covers the characteristics of industrial development antecedent to trade disruptions. A second

theme running throughout the book is Sen's attempt to account for protectionism.

The evidence that Sen provides to support the premise that strategic industries are the critical area of industrialization and trade rivalry is so painstakingly organized and logically argued that his analysis is both the most convincing part of the book and the most difficult to summarize. All specialists in economic growth should take the time to consider this aspect of Sen's work. He first documents the familiar view that all nations, first-comer as well as late-comer, follow a similar process of industrialization based on a similar structure of production. Strategic industries lead all economic development. Sen then limits the application of strategic to those industries that bring about and nurture self-sustained economic growth and ensure national economic self-reliance (p. 49). Sen selects the criteria of import-substitution, backward and forward linkages, and economies of scale to identify a group of six capital goods and heavy industries as strategic.

Military needs of the "human collective" provide the impetus to establish these industries and the patterns of production they encompass. All societies hold security as their paramount objective. Because its achievement is predicated on the successful pursuit of relative economic autonomy, the structures of strategic production, even in weaker nations (p. 87), embody a "secular tendency towards the creation of surplus capacity" (p. 158). Governments are prompted by overproduction in these intermediate goods to defend their industries through protectionism rather than to shift to other areas of productivity (p. 49). Free trade has been the exception rather than the rule since the late nineteenth century. The intra-firm trading activities of transnational corporations across national boundaries have modified the structure of production and thereby encouraged free trade. But state policies have eschewed the private endorsement of a liberal economic order by replacing tariff with nontariff barriers to trade as the means to safeguard economic autonomy (p. 226).

Sen's analysis loses credibility when he adopts the realist paradigm to explain similarities in the patterns of economic development and the presence of protectionism. The author's rejection of liberal and Marxist theories of international trade disjunctures leads him to conclude that the explanation must be found in an international system comprised of autonomous states who act in a unitary fashion at the national level. Sen accepts the state-centric model without either responding to recent, well-founded critiques of the approach or considering such relevant alternative models as the issue-based paradigm.

Among the more glaring problems in Sen's approach are his uncritical assignment of motivation to state policy and his assertion that the state is autonomous. According to the author, "the primacy of political considerations in governing the behavior of the State is apparent in its determination to institute industrialisation because of international political rivalry" (p. 67). He assumes that the international system is characterized by a struggle for power among nation-states and that the state is motivated solely by this struggle when it attempts to establish and maintain strategic industries within its domain. The case studies Sen provides do not, however, undertake the kind of policymaking analysis needed to ascertain the motives of the state. His espousal of state autonomy with regard to internal economic interests is based on equally tenuous grounds. Although he contends that the state modifies the means whereby it pursues security, Sen holds that this goal is unchangeable and not always consistent with economic interests. This view cannot be sustained without investigating how conflicts between security and other societal values, such as wealth, are perceived and resolved by industrialists, policymakers, and other political actors who contend for stakes in economic issue areas. Sen's work is nonetheless worth studying as a solidly researched and well-written economic analysis of industrialization.

M. MARK AMEN

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Information Technology in the Third World: Can I.T. Lead to Humane National Development?

By William James Stover. (Boulder, Colo.: Westview Press, 1984. Pp. 183. \$15.00, paper.)

Popular periodicals and trade journals are repeatedly filled with tales of the dawning promise of the "information age" brought about by the fusion of computer and communication technologies. Variations on this theme frequently trace the current events in transborder data flows, international conflicts over the satellite geostationary orbit, calls for a "new world information order," and, in all, a blizzard of loosely related issues strung together by developments in a fast-moving technology. Rarely does a scholarly work such as Stover's appear that takes a first step toward investigating any one of these issues in any serious depth.

Stover attempts and partially succeeds at covering a very wide range of topics concerning communication and development. First, the book describes the familiar disparities between developed and developing states; indicators of general

development inevitably correlate with corresponding indicators of information technology—televisions, telephones, computers, and the like. There is also a concise discussion of the context of communications in the process of overall political, economic, and social development. Here Stover relies on examples and on some of the theoretical writings by such social scientists as Daniel Lerner, Everett Rogers, and Wilbur Schramm. Two chapters follow that describe the technological dominance of the West in information technology, with emphasis on the redistributive claims of some developing states under the banner of a “new world information order” and the corollary doctrines of dependency theory and self-reliance. The final three chapters deal with means and ends in the use of technology and an agenda for humane development, which Stover defines as a process of meeting basic human needs while encouraging self-expression and decentralization (p. 11).

Stover succeeds in mentioning a variety of perspectives, although he offers a particular approach not significantly different from much contemporary thinking on how to solve the problems of the Third World. There are familiar appeals to international cooperation, to technology transfer, and to commitments by national leaders. He makes assertions such as “humankind is divided between the rich and the poor. . .” (p. 2), or that “[information technology] is the machine; communication is the product. . .” (p. 3). Information itself can be thought of as a commodity or product, but information technology is the use of machines to store, process, send, and receive information faster, cheaper, and more reliably. Communication refers to the means by which information transfer takes place.

Despite such minor definitional problems, the book benefits by a good organization of topics and at least outlines most of the salient issues on development and information technology. It is not likely to be dated by its treatment of these issues for a long time to come.

Even so, there are a number of errors and omissions that deserve mention. By emphasizing meeting basic human needs, alternative advantages of other approaches to development are understated. For example, Stover relies on much of the secondary source literature to restate the standard critique of multinational corporations and their apparent domination of information and its channels of communication. Yet many corporations that have operations in developing states assist greatly in advancing communication networks in these countries, and their very act of communicating puts many millions of dollars into national treasuries of the developing states.

Likewise, Stover is quick to cite the apparent

shortcomings and inequities of American political advertising and the media in general in juxtaposition to critiques of Soviet practice insofar as they serve as rival development models. The book fails to do justice to the realities of how information technology is used today to keep tyrants in power versus its use in democratic states.

Finally, Stover is also on weak ground in his criticism of efforts toward international cooperation. Development communication projects coming into their own in programs sponsored by the UNDP, World Bank, and several regional organizations could have received better treatment. Stover's plea to reform the INTELSAT satellite system is similarly erroneous (p. 153). Contrary to his claim, developing countries do not pay higher prices for satellite communications than do the developed countries. In fact, INTELSAT's global satellite network consciously averages prices to favor less economical routes to and from the Third World, hardly a situation in need of reform. This inaccuracy may be due to Stover's reliance on a few secondary literature sources for these criticisms instead of relying on research from primary source material such as documents of the international organizations themselves.

For the novice interested in information technology, communication, and development, *Information Technology in the Third World* is a reasonable introduction. For those requiring something more substantial, the field is ripe for further inquiry.

ERIC J. NOVOTNY

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Soviet Policy in Eastern Europe. Edited by Sarah Meiklejohn Terry. (New Haven, Conn.: Yale University Press, 1984. Pp. xv + 375. \$27.50.)

The continuing Polish drama has drawn attention to East Central Europe in the 1980s as had the invasions of Hungary in the 1950s and Czechoslovakia in the late 1960s. These and other events are discussed in *Soviet Policy in Eastern Europe*. No doubt the area has strategic, economic, and ideological importance to—and will continue to be a major concern of—the Soviet Union. To go beyond this observation and to make more specific predictions about the relations of the Soviet Union to this area is hazardous, a fact of which most contributors to this large volume have been fully aware.

Among the excellent surveys of the developments in Eastern Europe up to the early 1980s are those on “Soviet Policy toward the German Democratic Republic” by Angela E. Stent, “Soviet Policy toward Poland” by Andrzej Kor-

bonski, "Soviet Relations with Yugoslavia and Romania" by William Zimmerman, "The Political Economy of Soviet Relations with Eastern Europe" by Paul Marer, "Soviet Energy Policy in Eastern Europe" by John P. Hardt, "Theories of Socialist Development in Soviet-East European Relations" by Sarah Meiklejohn Terry, "The Warsaw Pact: Soviet Military Policy in Eastern Europe" by A. Ross Johnson, "Soviet Policy in Western Europe: The East European Factor" by Pierre Hassner, and "Eastern Europe in the Context of U.S.-Soviet Relations" by Raymond L. Garthoff.

Korbonski points out that the Soviets have great strategic interest in Poland, and in order to protect their lines of transportation and communication with their divisions in East Germany, "two Soviet divisions have been permanently stationed in Poland since 1945" (p. 62). He notes the significance of the 1978 election of a Polish pope, whose visit to Poland "proved to be a major catalyst to the birth of Solidarity a year later" (p. 76). One may take exception to his statement that there was "every reason to expect that an invasion of Poland in response to the combined challenge was only a matter of time" (p. 87). The conventional view was that because the Soviets invaded Czechoslovakia in 1968, a similar military invasion would take place in Poland in 1981. This "conventional wisdom," however, overlooked the obvious: Although the Soviets have kept their troops in Poland ever since the end of World War II, they did not have them in Czechoslovakia. Thus, the most important reason for the invasion of Czechoslovakia was the decision to secure the Soviet strategic and military interests in that country by having their troops there. Because the Soviets had their divisions in Poland, a military invasion of the country would make no sense from their point of view and would be counterproductive.

One may also take an exception to Jiri Valenta's claim made in his essay "Soviet Policy toward Hungary and Czechoslovakia" that "the Polish situation of 1980-81 may not be unique" (p. 123). I also question some of Valenta's other contentions, such as the idea that the Czechoslovak reform and infighting between members of the Czechoslovak ruling elite was "almost unnoticed by the Soviet leadership until matters came to a head in 1968" (p. 95). The known facts do not support this assertion. Indeed, the Soviets followed the developments in Czechoslovakia very closely. Furthermore, every country in the area is unique, and so are events in history including the Hungarian situation in 1956 and the Czechoslovak in 1968. In Hungary, the Hungarian armed forces were challenging the Soviet and Communist rule, whereas in Czecho-

slovakia, the Communist leaders, including Alexander Dubček, professed their unyielding loyalty to the Soviet Union and the Warsaw Pact. Only after the shocking experience in Moscow late in August did Dubček confess that he and his comrades underestimated Soviet "strategic interests" in Czechoslovakia (*The New York Times* and *The Washington Post*, September 2, 1968). While in Moscow as a member of the Czechoslovak Communist party delegation that signed the surrender, Zdeněk Mlynář realized, as he put it: "Actually we are imbeciles, but our imbecility has the appearance of ideology of reform Communism" [*Jsme vlastně blbci, ale naše blbost má podobu reformního komunismu*] (*Mráz přichází z Kremlu*, 2nd ed., Köln: Index, 1979, p. 297).

More recently, new testimony about the 1968 events in Czechoslovakia has been given by Vratislav Pěchota, who was directly involved in the Czechoslovak-Soviet negotiations in the summer of 1968 as a legal advisor to the Czechoslovak president Ludvík Svoboda, and who currently lives in the United States. He has stated that as early as April, 1968 the Soviet agents in Czechoslovak political circles made it plain that the real issue was not the reform, but rather the establishment of Soviet military bases in Czechoslovakia.

As A. Ross Johnson writes, "the Soviet stake in Eastern Europe involves security considerations and is based on military power" (p. 255). Thus, in the last analysis, the issues are simple: The Soviets did not invade Poland because they were already there since 1945; and because Dubček failed to invite the Soviets in, they invited themselves.

The book is equipped with footnotes and index, and does make a contribution to useful knowledge about the Soviet policy in Eastern Europe.

JOSEF KALVODA

Saint Joseph College

Soviet Decisionmaking for National Security.

Edited by Jiri Valenta and William C. Potter. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. xiv + 319. \$40.00, cloth; \$18.50, paper.)

This is a useful collection of 12 essays written by prominent Soviet specialists. Most of the selections were first presented at the 1980 Conference on Soviet Decisionmaking for National Security sponsored by the Naval Postgraduate School and UCLA's Center for International and Strategic Studies.

Arthur Alexander provides a "simple model of decisionmaking" (p. 9) for Soviet national security comprised of four elements: the foundations of Soviet behavior (Russian history, Bolshevik

ideology, and learned behavior); constraints (resources, technology, domestic and foreign reactions); interaction between high- and low-level decision makers; and decisional and policy outputs (resource allocation, budgets, weapons).

Vernon Aspaturian stresses the importance of Stalin's personality and character on, and dominance of, Soviet decision making from 1934 to 1953. The continuity of basic party and state institutions is part of Stalin's legacy, although the current functions of various institutions and their interrelationships have evolved and changed over the years. Dimitri Simes characterizes Soviet decision making during the Brezhnev era as "controlled pluralism." He cautions, however, that increased pluralism among the elite and the official institutions need not lead to greater international restraint by the Soviets or to improved relations with the West.

Jerry Hough examines the role of the military in weapons development from the 1930s to the present and concludes that "the basic relationships between the military, the defense industries, the designers and the scientists" (p. 106) remain largely unchanged. Ellen Jones contends that organizational momentum or bureaucratic inertia are less important to the Soviet defense policy processes than are party-set policy goals and military doctrine. The higher level of centralization in the defense sector and oversight mechanisms generate greater responsiveness from the bureaucracy than is found in other areas. Raymond Garthoff argues that the Soviet military, which initially viewed the SALT negotiations with suspicion, played a constructive, although conservative, role during the negotiating process.

Jiri Valenta analyzes the Soviet interventions in Czechoslovakia and Afghanistan from a bureaucratic politics perspective. In the Czechoslovak case, he is able to identify interventionist and noninterventionist coalitions that reflected different bureaucratic interests and functional responsibilities. Those concerned with domestic affairs (security, ideology, non-Russian nationalism) favored military intervention, whereas those dealing with foreign affairs and international communist party relations initially preferred the use of political and economic coercion. Among the differences between the Czechoslovak and Afghanistan interventions, Valenta notes that the Afghanistan action combined defensive and offensive motivations, and the Soviet military played a larger role in assessing the political situation in Afghanistan before intervention.

Galia Golan examines Soviet decision making during key stages of the Yom Kippur War and concludes that the leadership miscalculated the effects of its actions on détente with the West and suffered a decline of influence in the Arab world.

Dennis Ross argues that the dynamics of oligarchic and coalition politics and the fear of failure create a pronounced risk aversion in Soviet decision making. This conservative bent does not, however, preclude the taking of decisive action when fundamental interests are at stake or when inaction is perceived as failure.

Stephen Meyer provides a stimulating critique of numerous models of Soviet decision making for national security. He notes that these contending models are often pitched at different levels of analysis and can be integrated into a composite model. Meyer calls for greater methodological rigor, empirical testing of hypotheses, and the employment of "focused-comparison" case studies. Finally, William Potter underscores the dearth of studies in Soviet national security decision making in the areas of policy initiation and implementation.

In conclusion, our understanding of Soviet national security decision making is deficient in many areas, due in large measure to data limitations. This volume, however, represents a stimulating, timely, and valuable contribution to the literature.

JAROSLAW BILOCERKOWYCZ

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More for the Least? Prospects for Poorest Countries in the Eighties. By Thomas G. Weiss and Anthony Jennings. (Lexington, Mass.: D.C. Heath, 1983. Pp. xxiv + 176. \$24.95.)

The United Nations Conference on the Least Developed Countries was held in September, 1981 in Paris; this book is an analysis and evaluation of its work. The analysis is certainly informative in a straightforwardly narrative way, if somewhat less resonant to larger issues than its authors would appear to intend. The evaluations are always clearly presented and vigorously argued, if somewhat more ambiguous in overall effect than their authors might wish to admit. Both are worth reading. But there are problems.

The historical analysis primarily narrates with increasing intensity of detail both the sequence and variety of preparations made for the conference and the management and outcomes of negotiations in it. It is based on apparently full access to, and extensive exploitation of, all documentary records preparatory or pertaining to the conference, which are carefully cited, and—more obscurely—direct observations of these meetings and/or interviews with those who were participants in them, none of which are cited in particular, or even generally characterized or described. That is too bad. I have no grounds on which to

question the validity of the authors' narrative, which is informative and clear. I only lament their occasional obscurity of reference, which perhaps needlessly weakens their case, no matter how slightly.

Evaluations, and generalizations from them, raise different problems. The authors end up saying (p. 156) that they "deem the Paris Conference a qualified success" and proceed to extrapolate from it a series of recommendations for the future conduct of conference diplomacy. The first problem is that the authors do not always agree with each other in this estimate, at least to essentially the same degree. Anthony Jennings seems to take, in chapter 5, a somewhat more jaundiced or perhaps more radical view of the outcomes of the conference than Thomas Weiss takes elsewhere. In any event, he seems a bit more disposed than Weiss toward what may be characterized as the apparent least-developed-country or third-world view of the conference—which was clearly less "exultant," as Jennings says (p. 127), than that professed by representatives of countries with "developed market economies."

This disagreement, finally, makes a difference. Whether the "Paris process" is in whole or in part a promising model for future global negotiations of North-South issues depends in large measure on which parties to the effort were pleased or displeased, and how much. The authors together are clear enough on the demise or the failure thus far of other earlier models, and all may want to agree that "if states are to make policy internationally through global conferences, a new framework needs to evolve, one based on a process of give and take" (p. 144). But what if one side perceives that it gave quite a bit more than it took? And what if events in the predictably near future (at the "mid-term" evaluation and review conference scheduled for 1985) bear out that perception, as even these authors confess that it well may? Then the allegedly good features of the Paris process become much less attractive, and the model itself much less promising. It is a problem that these authors have necessarily left unresolved. But they do not even agree fully enough between themselves convincingly to persuade others that the models that they recommend for the future are either desirable or likely.

KEITH S. PETERSEN

Rhodes College

Caught in the Middle East: Japan's Diplomacy in Transition. By Michael Yoshitsu. (Lexington, Mass.: D.C. Heath, 1984. Pp. xi + 113. \$20.00.)

The thrust of this short but pithy volume is that Japan's foreign policy has been undergoing a fundamental change since 1973. From the end of World War II until the international oil crisis of 1973, Japan's government was content to follow America's lead in world affairs and cultivate the image of a loyal and trustworthy ally. But by the 1980s, Japan had become a largely independent actor in world affairs.

In the Middle East, Japan's interests have corresponded only in part with those of the United States. Dependent on the Middle East for most of its energy, Japan's economy has been hostage to the good will of the Arab and Iranian oil producers. Japanese policymakers whom Yoshitsu interviewed perceive the Arab-Israeli dispute, and especially the Palestinian question, as the fundamental cause of instability and conflict in the Middle East. The unneutral American policy of siding with Israel is seen to stand in the way of any permanent solution. The Japanese have therefore sought to cooperate with the EEC powers in order to correct America's "bad habits" and mediate a settlement in the Middle East. The Japanese government in 1973 proclaimed its support for Palestinian self-determination, in 1976 permitted the establishment of a PLO liaison office in Tokyo, and in 1981 welcomed Yasir Arafat's visit to Japan.

Japan's principal concerns in the Iranian hostage crisis were to protect Iran from superpower intervention and internal collapse. The Japanese feared that Washington's policies might complicate the securing of oil supplies and the blunting of the Soviet advance in the Persian Gulf area. By cooperating with the Western European countries, Japan hoped to restrain America. Of continuing concern to the Japanese was the safety of the Iran-Japan Petrochemical Company, in which nearly \$2 billion of Japanese capital was invested. The Iran-Iraq War inflicted physical damage on the uncompleted facilities of the IJPC and threatened the future of the project. Although the Mitsui Company wanted to cut its losses and get out, both the Iranian government (desirous of profits) and the Japanese government (seeking to protect Japan's oil supply) wanted to preserve the project.

With respect to the Soviet invasion of Afghanistan, the Japanese hoped that further Soviet aggression could be deterred by cooperating with the Western allies. Ronald Reagan's suspension of the grain embargo seemed to signify a lack of American resolve. At the same time, the Reagan

administration's insistence that Japan drastically increase her defense expenditures seemed unreasonable to Japan.

Yoshitsu presents the theoretical conclusion of his study, a "regional view of world politics," in the Appendix of the book. In a regional perspective, Japan created a "network of bilateral, multi-lateral, and transnational measures applied selectively to three Middle Eastern concerns," namely regime instability, intra-area hostility, and foreign (superpower) intervention. The outlines of

Japanese foreign policy delineated here are new to observers who are accustomed to seeing Japan in the simple role of an American ally. Based on authoritative sources, and written with a fresh perspective, Yoshitsu's book is one that no specialist on Japanese or American foreign policy can afford to ignore.

THEODORE MCNELLY

University of Maryland

Normative Theory

Political Judgment. By Ronald Beiner. (Chicago: University of Chicago Press, 1984. Pp. xvi + 199. \$20.00, cloth; \$9.50, paper.)

Beiner's articulate essay has a political purpose, "to disclose a mental faculty [political judgment] by which we situate ourselves in the political world without relying upon explicit rules and methods, and thus to open up a space of deliberation that is being closed ever more tightly in technocratic societies. . . . Ultimately, what is sought in this study is a redefinition of citizenship" (p. 3). If his political aim is hardly a novel one, the means he suggests are extraordinary. Elaborating on his remarks in his edition of Hannah Arendt's posthumous *Lectures on Kant's Political Philosophy* (University of Chicago Press, 1982), Beiner attempts nothing less than a synthesis of Kantian and Aristotelian political philosophies.

Why have political theorists ignored a political concept as important as judgment? Beiner concludes from a survey of contemporary arguments in political theory—those of Jürgen Habermas, Hans-Georg Gadamer, and Arendt—that we must reconsider the fundamental alternatives of Kant and Aristotle. Reflecting on *The Critique of Judgment*, Beiner concludes that Kant might (and should) have developed a political philosophy based on the notion of objectivity of taste rather than have proceeded as he did in the later works on history (such as *Perpetual Peace*). "In judging the political world we exercise taste, but it is a faculty of taste as all-encompassing as human experience itself" (p. 165). But such a formal, principled notion of judgment does not adequately consider man's social and political character.

Hence, Beiner turns to Aristotle's account of prudence (*phronēsis*) in Book VI of the *Nicomachean Ethics*. Beiner interprets qualities often associated with prudence, "judgment" (*gnōmē*) and "understanding" (*sunesis*), to mean that

"judging-with" or "sympathy" (*suggnōmē*) are important elements of prudence. Thus friendship and rhetoric are crucial in Aristotle's political philosophy, for they bind together the political community, reflecting common judgments about the common good. With the importance of prudence, friendship, and rhetoric, Aristotle's notion of judgment is "diametrically opposed" to that of Kant (p. 71).

Beiner wants political judgment to embrace both Kantian autonomy and Aristotelian community, detached taste and grounded prudence, Thucydides and Pericles. "Political judgment must be aesthetic *and* teleological: it must encompass formal-transcendental features of the faculty of judgment, as well as orient itself to rationally desirable human ends" (p. 102).

Now Beiner (a Canadian) is certainly correct in saying that the theory of political judgment should be "exemplified" by the particulars it illuminates. But consider his own examples of political judgment reflecting its "tragic responsibility"—Arendt's *Eichmann in Jerusalem* (Penguin, 1977) and Maurice Merleau-Ponty's *Humanism and Terror* (Beacon Press, 1969), the latter a despicable apology for Soviet tyranny. Other examples are problematic as well.

In a way Beiner's concerns remind one of Kant's inspiration, Rousseau. But Rousseauian and Kantian morality reflect the lowness of Machiavelli's project even more than those earlier adherents of Machiavelli they criticized. Ignoring Machiavelli's significance here, Beiner remains within the horizon of the modern intellectual debate (deproletarized Marx and academized Heidegger). Moreover, Beiner depreciates Aristotle. He leaves undiscussed the greatest tension between Aristotle and Kant: Aristotle saw philosophy arising from wonder, whereas Kant sought to put an end to wonder, that is, to philosophy itself. (Hence Kant elevated morality

above philosophy.) Finally, Beiner, while admiring *phronēsis*, has no use for Aristotle's virtue as perfected in the magnanimous man (*megalopsuchos*). This reflects Beiner's slighting of classical political philosophy's standard of the best regime. That is, he does not make the question of natural right the central one. Had he followed through these steps he might have concluded that a proper study of political judgment would have its *archē* in its particular manifestations—such as Lincoln and Churchill.

KEN MASUGI

The Claremont Institute for the Study of Statesmanship and Political Philosophy

The State and Political Theory. By Martin Carnoy. (Princeton, N.J.: Princeton University Press, 1984. Pp. 282. \$27.50, cloth; \$8.95, paper.)

Often reading works on Marxist theory is like reading a bad novel: One encounters a cast of thousands, each player appearing briefly and then disappearing into the great mass never to be heard of again with little in the way of story line to connect the disparate characters or events. Happily, Martin Carnoy avoids these difficulties and provides a masterful analysis of recent Marxist thought.

The framework for this study is set in the opening chapters, which provide two alternative understandings of the nature and function of the State. The opening chapter, misleadingly titled "The State and American Political Thought," actually presents the case for liberalism as developed by Adam Smith and John Locke. Only after analyzing free-market and social contract theories does Carnoy turn to a discussion of the State from an explicitly American perspective in the form of pluralism and corporatism.

The minimalist view of the State, as represented by classical liberalism, is then contrasted with the Marxist view of the State. Marx's own view of the State is threefold according to Carnoy: The State emerges from the material conditions of society, particularly the relations of production; rather than representing the common good, the State represents the class dominant in productive relations; therefore, "the State in bourgeois society is the repressive arm of the bourgeoisie" (p. 50).

Although this basic theoretical framework holds throughout Marx, it is not fully developed, and there are inconsistencies among Marx's various writings. The limitations and tensions found within Marx—especially concerning why and how the State is an instrument of the ruling class, the possibility of State autonomy from the class structure of the economic system, and the

role of democracy in a socialist revolution—provide the grounds for the reinterpretation of Marx.

Lenin's theory of the State, which is actually a strategy for revolution, eliminates all of the tensions and ambiguities found in Marx. But given his narrow understanding of the State, Lenin derails rather than develops Marxian theory.

As thesis begets antithesis, so Lenin begets Rosa Luxemburg, who defends the possibility of a worker democracy against Lenin's new centralism and a host of other revisionists. The major part of Carnoy's book deals with some of the most important of the post-Leninist theorists, all of whom attempt to put the red train back on the tracks. Each of these core chapters provides an introduction to a thinker or school of thought (among those discussed are Gramsci, Althusser, Poulantzas, Offe, Bobbio, Cardoso, and a number of Americans) and an analysis of the thinker as he relates to or develops the basic Marxian model. The real strength of Carnoy's treatment, however, is that he shows the relationship of these thinkers not only to Marx, but also to each other. Thus, his analysis takes the form of a debate between various points of emphasis and interpretation within the Marxist camp.

Carnoy concludes that contemporary Marxian analyses "are generally opposed to Lenin's view of the State: the State is not regarded simply as an instrument of the ruling class" (p. 250). The State, rather than the means of production, has become the focus of class conflict. In addition, Carnoy argues that recent Marxist thought sees the transition to socialism in terms of a democratic movement rather than a violent struggle.

Although one may disagree with some of Carnoy's editorial decisions (for example, no discussion of Jurgen Habermas), this book is well organized and cogently written. It can be used effectively at two levels: It is a fine introduction to recent Marxist theory, but it is also a sophisticated analysis of recent controversies within Marxist thought that will be of value to those already initiated in these concerns.

STEVEN D. EALY

Armstrong State College

The Analysis of Power: Core Elements and Structure. By Geoffrey Debnam. (New York: St. Martin's Press, 1984. Pp. x + 112. \$19.95.)

Debnam has written a concise and relatively balanced book on social power, which can be read at two levels. At one level the book is a critique of the existing community power literature. Although he devotes more space to attacking critics of pluralism such as Floyd Hunter, Peter

Bachrach and Morton Baratz, Matthew Crenson, John Galvanta, and Steven Lukes, Debnam does critique pluralists, especially Nelson Polsby and Robert Dahl, for their narrow focus on "Who governs?" and their consequent avoidance of the "Who benefits?" question. Because much of this criticism has already appeared in print, including Debnam's 1975 article in the *American Political Science Review* (69, 889-899), students of power will largely find this material familiar.

At a second level the book introduces Frederick Frohock's core properties framework as a new approach to the study of power. Noting the wide variations in definitions of power and the further gap between these formal definitions and the operational terms actually used in research, Debnam argues that it is necessary "to identify the core elements that are present in all conceivable instances of power," otherwise analysis can be flawed by a fatal preoccupation with other features, such as conflict (p. i). After a search of the community power literature for nominees to the "core elements" category, Debnam produces a list of 15 elements, which through a series of ad hoc arguments, he reduces to four: action, actor, intention, and outcome.

Debnam's identification of these four core elements does not, however, free his study of power from all major methodological difficulties. By his own admission, both intention and outcome are difficult to apply in practice because their meaning varies depending on whether one is applying the term from the viewpoint of the observer or the observed (pp. 50, 59-60, 84). For Debnam, this problem of duality is one inherent in the social sciences; yet, according to Debnam, the problem of ignoring the duality (i.e., a covert ideological stance masquerading as objective analysis) is much worse than the problem of ambiguity which is inherent in any approach which honestly faces up to the duality. Granted that his conclusion is correct, Debnam's argument would be more convincing if he had dealt with several books by and about Michel Foucault, which explicitly address this structural aspect of power.

Those familiar with his 1975 article may also be troubled by an unexplained shift in emphasis in Debnam's present argument. In 1975, he explicitly and repeatedly argued that power was a descriptive rather than an explanatory concept (pp. 898-899, 906). Although his focus in this book remains the same—identifying the context of power and demanding evidence of a causal relationship between intention and effect—he no longer explicitly rejects power as an explanatory term.

In contrast to the problems noted previously, Debnam's discussion of action and actors is crisp and well fleshed out, especially his analysis of col-

lective actors. In addition, his attempt to deal explicitly with the subjective side of power in that chapter and throughout the book represents an important, albeit incomplete, start toward a proper understanding of that dimension of power, which is all too frequently ignored by political scientists. Debnam himself could have profitably incorporated insights by social psychologists in this regard, especially those of Samuel Bacharach and Edward Lawler. Still, what Debnam has written should be useful to students of power and should be especially attractive to those who feel uncomfortable with either side in the polarized community power literature, which remains divided among pluralist and anti-pluralist lines.

EDWARD S. MALECKI

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The Feminist Case against Bureaucracy. By Kathy E. Ferguson. (Philadelphia: Temple University Press, 1984. Pp. xviii + 286. \$24.95.)

Those who work in bureaucratic organizations and those who are the clients of bureaucracies often know what it is like to be marginalized, either through being made invisible in relationship to superiors or, ironically, by being constantly exposed, being made highly visible through supervision, techniques of surveillance, and the structuring of time so that one's time is accounted for. This book takes the insights of those who have had the bureaucratic experience and develops a singular and striking argument: The modern, bureaucratic organization of social life is universalizing the processes by which women have been subjugated to men. Contemporary guides to success in business, the use of language as a means of insuring monopolies of expert knowledge, the function of bureaucratic rationality as a technique for denegrating, and discounting the validity of emotion are among the topics taken up in this unique study of modern bureaucracy.

This book is divided into five essays. Ferguson starts by arguing that the critique of bureaucracy is best advanced through the use of a discourse that not only comprehends the subtle ways in which bureaucratic power is exercised, but that also has potential for presenting an alternative vision of collective life. Radical feminist thought does just that, in her view. She evokes Michel Foucault's idea that the resistance to dominant discourses might provide the basis for the construction of new, less threatening forms of subjectivity. The discourse of bureaucracy, Ferguson argues, constitutes the dominant language

through which modern subjects are shaped. It is that discourse she wishes to challenge.

Chapters 3 and 4, "The Bureaucrat as the Second Sex" and "The Client as the Second Sex," detail the charges Ferguson makes in the earlier chapters. She develops the concept of "feminization," initially used by Ann Douglas in *The Feminization of American Culture* (Alfred A. Knopf, 1977). "Feminization" involves the extension of the depoliticizing, privatizing aspects of women's traditional role to the sectors of the population who are the victims of bureaucratic organizations, both the administrators and the clientele," she explains (p. 93). Through a rigorous examination of contemporary organization theory, the use of interviews, and a skillful reading of labor industrial relations literature, Ferguson buttresses her argument with the sort of compelling evidence that is immediately recognizable to anyone who has struggled to cope with working within a bureaucratic organization or who has confronted a bureaucracy as a client.

Ferguson does not call for an alternative "masculinization" of modern, bureaucratic society, but instead argues for the development of a feminist discourse of resistance. The concluding chapter, "Elements of a Feminist Discourse," presents the case for alternative value structures based upon values that are associated with the experience of women but that have been devalued precisely by modern bureaucratic forms of organization.

Ferguson's conclusion may strike some as a bit too facile. Such is the fate, it seems, of almost all works that attempt to move off the spot of radical critique to present an alternative vision. But in her sense of the tragic elements of contemporary, bureaucratically determined social life, and in her willingness to offer an alternative discourse, Ferguson has opened what should be a new and refreshing debate about the future of bureaucracy and the future of feminism. If this book does little else, and it does much more, it should end any remaining doubt about the value of feminist political thought for providing a critical perspective on contemporary political problems.

THOMAS L. DUMM

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- **Beyond Subjective Morality: Ethical Reasoning and Political Philosophy.** By James S. Fishkin. (New Haven, Conn.: Yale University Press, 1984. Pp. vii + 201. \$22.00.)

James Fishkin's new book is an ambitious work, spanning as it does the fields of moral philosophy, moral psychology, and what might be

called speculative cultural analysis. The main part of the book is an account of the ways in which "ordinary moral reasoners" develop subjectivist positions on ethics as a way of dealing with their own positions on moral issues. Fishkin believes that such skepticism is characteristic of our contemporary moral culture. However, he argues, this common skepticism is not inevitable, and it may be avoided by a better understanding of the conditions of morality. The speculative social analysis of the book is to be found in Fishkin's interesting thesis that the moral subjectivism of our culture undermines the legitimacy of the liberal state.

An important part of Fishkin's argument is a taxonomy of ethical positions, constructed for an analysis of the "meta-ethical" positions of ordinary moral reasoners. The proposed classificatory scheme consists of six claims about moral judgments, each alleged to entail those that follow. The strongest claim is that "One's judgments are *absolute*, that is, their inviolable character is rationally unquestionable" (p. 11); the weakest is simply that "One's judgments apply [only] to oneself" (p. 14). The proponent of the first claim is committed to the position ("absolutism") that there are "rationally unquestionable principles that hold inviolably, with objective validity, with universalizability, and . . . that apply to others as well as to oneself" (Kant is cited as an example on page 15). The "amoralist" rejects all six claims, including the weakest. In between the absolutist and the amoralist are to be found defenders of "rigorism," "minimal objectivism," "subjective universalism," "relativism," and "personalism."

Using this classificatory scheme, Fishkin analyzes the meta-ethical claims and assumptions of some moral reasoners with whom he has conducted extensive interviews. Although it is possible that the latter are not representative of ordinary moral reasoners—all (but one) university students at Yale or Cambridge and all (but one) male—any reader of this journal who has ever discussed ethics with college students will recognize the positions articulated. Fishkin argues that his moral reasoners have certain "expectations" (e.g., "An objectively valid moral position must have a basis that is rationally unquestionable" [p. 52]), and that these give rise to subjectivism. Thus, a rejection of these assumptions may enable one to avoid skepticism.

The crisis of liberal culture to which Fishkin wishes to draw our attention is due to the fact that the legitimacy of the liberal state cannot be based "on the controversial religious or metaphysical assumptions of any particular group" given liberalism's commitment to state neutrality (p. 154). Yet if we make the assumptions that lead Fish-

kin's moral reasoners to skepticism, it is unclear how the legitimacy of the liberal state is to be defended given the unavailability of a foundation in "ultimate convictions" about religious or metaphysical matters.

Although Fishkin's project is interesting and original, there are a number of problems that should be mentioned. For one, the religious and metaphysical skepticism that he appears to accept in his account of contemporary liberal culture risks undermining his attack on moral skepticism. More importantly, there are serious problems with the taxonomy of moral positions that Fishkin develops. It is hard to see how to use the scheme to classify the moral theories of many classical and contemporary philosophers (e.g., Plato, Aristotle, Hobbes, Rousseau, Hegel, Philippa Foot, David Gauthier). Yet Fishkin claims that "the seven positions defined by the total scheme combine to capture the full range of possible consistent positions on the issues that they classify" (p. 10). The scheme seems to work well only for "Kantian" moral theories. Further, the scheme reflects some of the very confusions that seem to lead Fishkin's moral reasoners down the path of skepticism. The various claims conflate epistemological requirements (e.g., that moral judgments be rationally unquestionable), scope requirements (e.g., that moral judgments apply interpersonally), logical or formal requirements (e.g., that relevantly similar cases be treated similarly), and overrideability conditions (e.g., that duties be absolute or inviolable). In one of the appendixes, Fishkin discusses some elaborations of this taxonomy, but we are still left with a scheme much less general than that promised in the beginning of the enquiry and thus unlikely to enable contemporary moral reasoners to find a particularly stable foundation for the liberal polity.

CHRISTOPHER W. MORRIS

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Beyond Domination: New Perspectives on Women and Philosophy. Edited by Carol C. Gould. (Totowa, N.J.: Rowman & Allanheld Publishers, 1984. Pp. xii + 321. \$24.95, cloth; \$11.95, paper.)

Beyond Domination is an articulate collection for scholars in philosophy, the social sciences, and psychiatry. It should be read for the excitement of new or revised conceptualization and possible paradigm shift and as a corrective to inadequate thought modes; Linda J. Nicholson (pp. 221-230) nicely combines these strengths. These are the supplemented proceedings of a 1981 interdisciplinary conference on the philosophy of women's

liberation convened by the Jewish, Catholic, and Protestant Institute of Women Today. The relative moderation of the proceedings may render radical ideas palatable to the intellectually curious. One result of this circumstance is that many contributions, for example, that of Janice Moulton and Francine Rainone (pp. 189-203), demonstrate tension between creativity and palatability, which may not amount to full disclosure (see, for example, Gould's chapter 1).

These largely historically grounded and valuable perspectives (Mitchell Aboulafia, pp. 175-185; Paula Rothenberg, pp. 204-220) also sometimes become dismayingly ahistorical through nonrecognition of blinders that have characterized past feminist thought—for example, the centrality of the complementarity of women's and men's roles. Space limitations notwithstanding, the underdevelopment of overinclusive arguments is frustrating (e.g., Anne Donchin, pp. 89-103; Eva Feder Kittay, pp. 145-174).

The juxtaposition of the sophisticated (for example, Caroline Whitbeck on gender orientation) and the naive (for example, uncritical reliance on the linear socialization model) is problematic. Discussions of mother-child separation explore the child's but not the mother's need for independence; compare, for example, Aboulafia (pp. 179-183) and Janet Farrell Smith (p. 265). The nonetheless fairly uniform reliance on androgyny is outmoded in much recent feminist thought; the emphasis on liberty, ethnocentric. Alison Jagger's to-the-point critique of traditional Marxism seems overinspired given her lackluster critique of liberal feminism.

Also problematic is a tendency to sidestep issues: Gould argues the deinstitutionalization of marriage without discussion of battering (p. 22); Donchin, Freud's discovery of the unconscious mind without discussion of its incest-based aspects. Kittay's chapter on pornography is confusing. The complex abortion issue is mostly tangentially and not satisfactorily explored; the sterilization issue seems not to exist; the differently-abled are not recognized as worthy of sustained analysis. Excepting Rothenberg's analysis, race is ignored. Geraldine Perreault criticizes radical feminists for racism, then mentions certain white but no black "exceptions"; third-world women fare no better (p. 299).

The weakest chapter may be Rosemary Radford Reuther's, which presents history that is already reasonably known and avoids some of the most creative thought on women, spirituality, and empowerment. Reuther's discussion of women's marginalization by organized religion turns from the ethical failure of corporate responsibility in an individualistic society to founder on the argument that "liberation feminists" see biblical history

monofocally. These feminists demonstrate aspects of biblical history and its precursors that retain, reinstate, revise, or seek to destroy goddess worship, and hence awareness of the precursors' significance to biblical history.

Negative references to the attractiveness of goddess worship to propertied individuals are equally unconvincing: Anne Hutchinson's and Mary Baker Eddy's beliefs were similarly attractive; this does not defeat the importance to many women of Hutchinson's and Eddy's work. "Liberation feminists" are involved in the search for documentation of the nature of societies engaging in goddess worship.

Often, the further removed from theology or controversial subjects in organized religion, the stronger the presentations. The strongest of the 17 essays may be Whitbeck's and Sandra Harding's. Harding's concise epistemological analysis is concerned with how sexual politics shape legitimated beliefs. Her orientation emphasizes the disproportionate reliance on (select) men's social experiences and the denial of validity to women's perceptions of them and their own. Further, denial is based on men's experiences, which model most study and progress. Claims to universality for any such paradigm are suspect because of the historical, differential objectifications of women's and men's senses of self or gender. The masculinized abstraction of moral development and thus standards for "practical rationality" (p. 55), are inadequate to judge allocations of rationality between women and men. Harding asks (p. 57), "Why should [criteria for maximally complete and undistorted scientific accounts] not insist on the neutralization only of epistemologically regressive social values?" The gender-typed rational reconstructions of the scientific process may be amiss rather than the process itself.

Whitbeck turns to a feminist ontology and its core concept, the self-other relation, to demonstrate brilliantly its difference from self-other opposition. Whitbeck's point is not the continuation of opposition but the value of social vision organized cooperatively for purposes of mutual realization.

The diversity of Harding's and Whitbeck's essays is a tribute to feminist philosophical diversity and thoughtfulness. Both essays deserve separate and combined study.

SARAH SLAVIN

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Wealth and Virtue: The Shaping of Political Economy in the Scottish Enlightenment. Edited by Istvan Hont and Michael Ignatieff. (New York: Cambridge University Press, 1983. Pp. x + 371. \$59.50.)

The debate that is at the center of this collection of essays is over the relative influence of two intellectual traditions upon the origins of political economy: civic humanism, with its concerns for virtue and corruption, and the natural jurisprudence tradition, with its focus on justice. Five of the 13 papers collected here were originally presented at a colloquium held on civic humanism and Scottish political economy in May, 1979 at King's College, Cambridge, and the others were either commissioned for this volume or were presented during a seminar series that preceded the colloquium. On the whole, the essays suggest that although civic humanism may describe the intellectual background against which political economy emerged, it is the jurisprudence tradition that captures more of the content of the new science.

In his contribution, J. G. A. Pocock seems to back off from his earlier strong claims for the civic humanist tradition, arguing now that civic humanism should be seen as a paradigm serving a heuristic purpose for the historian. As such, it is just one of a set of competing paradigms for the understanding of Scottish philosophy and political economy—the tradition of civil jurisprudence is another such paradigm. For the Scots themselves, the competing paradigms take the form of competing rhetorics available to serve a variety of ideological purposes so that the different languages may wind up present in the same theorist as ideological need dictates. "It is certainly not the case," Pocock argues, "that the Scottish theorists in general regarded republican and jurisprudential language as distinct and ideologically opposite rhetorics" (p. 251).

Donald Winch, however, argues that Adam Smith was entirely free of the civic humanist influence. Instead, Winch maintains that when Smith subsumed political economy under the broader framework of "the science of the legislator," he was rejecting the civic humanist lessons of his teacher Francis Hutcheson in favor of a natural law perspective combined with "a comparative historical treatment of law and government considered as social phenomena" in the manner of Montesquieu and Hume (p. 264). Winch acknowledges that the resulting view of politics as constitutional machinery is a pale one compared with the active citizen virtue of civic humanism but defends it nonetheless, choosing to see Smith as an eighteenth-century theorist in eighteenth-century terms.

A more modern and interesting interpretation

of Smith is given in the editors' introductory essay. Hont and Ignatieff claim that Smith set out in the *Wealth of Nations* to solve a "paradox" he had identified in his earlier *Theory of Moral Sentiments*; namely, that while commercial society was inequalitarian in its distribution of property, it also provided more adequately for the needs of the poor than had earlier, more egalitarian (and more virtuous) societies. The paradox is resolved through Smith's theory of economic growth, a theory that takes into consideration the role of inequality in fostering emulation and thus the incentive to accumulation. That incentive, when combined with the increased productivity made possible by the division of labor, results in a system in which the living standard of the lowest orders steadily improves, absolutely if not relatively to the orders above them. Security of property is essential to such a system, and Hont and Ignatieff argue that it is for this reason that Smith adopted, following Grotius, a "commutative" rather than a "distributive" theory of justice (i.e., justice as police rather than as allocation of property).

This is a convincing account of Smith, despite the unnecessary interpretive ploy of the "paradox," which was never so stated by him. In separate essays, Hont goes on to trace the development of arguments in the eighteenth century concerning the relationship of rich and poor countries in the competitive scramble for markets that broke free of the civic humanist projection of cycles of growth and decline, while Ignatieff provides an insightful analysis of John Millar, arguing that the Scottish philosopher played a key role in marginalizing the civic humanist discourse by reducing it to a moralism of private life. The combined weight of these arguments is more than enough to overwhelm the defenders of the civic humanist interpretation represented here. John Robertson, in an essay comparing Andrew Fletcher with David Hume, claims that with Hume, "the Scottish Enlightenment came to the limits of the civic tradition" (p. 141) while still remaining within them since Hume argued that commercial society would make civic virtue possible despite employing nonvirtuous means. No more convincing is Nicholas Phillipson's view that Smith's civic moralism must be interpreted against the background of Scottish provincialism and the chance it provided "responsibly-minded men of middling rank" (p. 179) to escape the worst features of commercial society. Robertson and Phillipson both appear to be fighting a losing battle.

Other contributions include an informative essay by James Moore and Michael Silverstone on Gershom Carmichael, who was responsible for the particular shape taken by natural jurisprudence in Scotland, and an article by John Dunn

contrasting the theocentrism of John Locke with the practical reason of Hume and Smith, further attesting to the modern temper at the root of political economy.

NICHOLAS XENOS

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The Reconstruction of Patriotism: Education for Civic Consciousness. By Morris Janowitz. (Chicago: University of Chicago Press, 1984. Pp. xiv + 220. \$22.50.)

Morris Janowitz examines an issue that seldom is subject to social and political analysis—patriotism. His thesis is clear: The long-term trend in politics has been to enhance citizen rights without effective articulation of citizen obligations (p. ix). A meaningful balance between the two, he contends, must be restored.

Democratic institutions cannot endure unless the citizens possess a sense of attachment or allegiance to the nation-state. It is, therefore, a matter of great concern to democracies that a vital system of civic education be established whereby their political traditions, the operation and organization of their governmental institutions, and the "essential identifications and moral sentiments required for performance as effective citizens" can be learned (p. 12). Traditionally, the central agencies of civic education in the United States have been the school system and military. Yet, for a variety of reasons, as Janowitz notes, these institutions have declined in their effectiveness as instruments for civic education.

His assessment of civic education in America is both institutional and historical. In addition to the military service, he examines primary, secondary, and collegiate institutions as agencies for citizen education and contrasts them with experimental work and national service programs such as the Civilian Conservation Corps and VISTA. In addition, he depicts the changes that have taken place in the popular definition of citizenship since the American Revolution, contrasting especially the post-World War II period with the decades just preceding it.

Especially after 1960, Janowitz charges, the decline of civic education was led by politically motivated intellectuals and social scientists suspicious of traditional notions of patriotism. There also occurred the emergence of what Janowitz calls a "new communalism" in which an emphasis on ethnic and racial affiliations, particularly among groups which felt excluded from economic advantages, outweighed "concern with national citizenship" (p. 20). Social studies instructors further aggravated the delegitimization of social and

political institutions by endorsing in the name of civil and economic rights many of the claims of aggrieved minorities (p. 23). The new directions in civic education, the seeds of which were planted by the social realism professed by left-of-center intellectuals, emphasized "various forms of political dissent of the status quo" rather than traditional patriotism (p. 98). As a result of these trends, schooling came to play a less effective role as an agency for civic education.

Between the American Revolution and the introduction of the all-volunteer military force in 1973, the military constituted the other major agency of civic education. The idea of the citizen soldier, which embodied the belief in obligatory national service, enhanced civic consciousness. Yet, with the end of military conscription "the contribution of military service to civic education becomes problematic" (p. 42). In effect, the citizen soldier concept has been abandoned. Although Janowitz would prefer the return of conscription, he realizes that this prospect is not presently politically feasible (p. 198).

To bring about the reconstruction of patriotism, then, Janowitz proposes establishing a program of voluntary national service in which options for either military service or community civilian work would be provided. "Work pursued on behalf of the community or nation is a form of civic education" (p. 170). To guarantee participation from a socially heterogeneous group of people, Janowitz suggests that federal aid to education be made dependent upon national service.

The strength of this study lies in Janowitz's persuasive argument that the durability and vitality of democratic institutions require that a sense of community, or shared values, be preserved. Without civic consciousness, he rightly observes, social and political fragmentation ensues. My only complaint (albeit a minor one) with this lucid and impressively researched polemic is Janowitz's implication that the military and educational institutions are almost entirely responsible for the present decline in civic consciousness. Other equally important contributing factors are not given their proper due. The loss of family authority, the decline of religious faith, mass industrialism, and the retreat from localism and traditions have been identified by such social critics Russell Kirk and Robert Nisbet as having inclined us toward a loss of real community. Since the problem of community is intimately connected with the problem of civic consciousness, this book would have benefitted from a consideration of the insights of scholars such as these.

W. WESLEY McDONALD

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Marxism and Totality: The Adventures of a Concept from Lukács to Habermas. By Martin Jay. (Berkeley: University of California Press, 1984. Pp. xi + 576. \$29.50.)

This book is a monumental history of Western Marxism. It contains separate chapters on almost every thinker of note operating within this tradition: Lukács, Korsch, Gramsci, Bloch, Horkheimer, Marcuse, Adorno, Lefebvre, Goldmann, Sartre, Merleau-Ponty, Althusser, Della Volpe, Colletti, and Habermas. Yet the work is more than a mere encyclopedia. Its organizing theme is the concept of totality. Indeed this concept has enjoyed a singularly privileged position within both Marxist and non-Marxist social theory.

There are two distinct senses in which the idea of totality can be used. The first is a primarily methodological sense that enjoins us to understand the parts of any complex phenomenon in terms of their relational properties to some larger whole. Methodological holists as they are sometimes called tend to look not for external, causal associations between events, but for relations of opposition, mediation, and reconciliation between them. The second sense of the term, however, implies a normative longing for coherence, wholeness, plenitude, or what Rousseau once poignantly called "*le moi sans contradiction*." Implied here is a call to transform and overcome the present state of the world which may be variously described as alienated, reified, disenchanted, or anomic.

The concept of totality, Jay notes, was introduced into the Marxist tradition by Lukács who credited Hegel, not Marx, with its discovery. In *History and Class Consciousness* (1923) he wrote: "It is not the primacy of economic motives in historical explanation that constitutes the decisive difference between Marxism and bourgeois thought but the point of view of totality." Lukács hoped the introduction of this concept would liberate Marxism from the dead hand of economic determinism and the belief that revolution was only possible when the productive forces of society had reached an appropriate stage of development. Like Gramsci, who also called for a "revolt" against the deterministic laws of *Capital*, Lukács endowed the term with a high degree of political activism or "voluntarism," that is, Leninism.

And yet the relation between the concept of totality and political practice was far from unambiguous as the heirs of Lukács were to discover. In their joint work, *The Dialectic of Enlightenment* (1947), Horkheimer and Adorno presented modern society as a repressive totality of such stability and cohesion as to present literally no way out. The internal dynamics of "en-

lightenment" with its twin forces of capitalist production and the modern sciences of nature had drawn a closed circle from which there could be no means of escape. The subsequent history of the Frankfurt School divided over those like Horkheimer, who resigned themselves to the "iron cage" of modernity and who turned increasingly toward literary and aesthetic criticism as a form of consolation, and those like Marcuse, who remained loyal to Marx's original utopianism, but who saw it as a "Great Refusal," a nihilistic rejection of whatever exists.

Jay fulfills his intention of portraying the "adventures" of this concept to an impressive degree. While deepening and completing, he builds upon the earlier interpretive studies of scholars like Perry Anderson, Alvin Gouldner, and Russell Jacoby. He has read and digested not only the primary sources, but also all of the important critical studies in English, French, German, and Italian. In addition, he moves through the minefields of critical theory, phenomenological, and structural Marxism with an ease that comes only with long familiarity. Given these merits, it seems ungenerous to search for faults. Yet one wonders whether the history of Western Marxism can be written as so many variations on the theme of totality, or whether the concept is being made to bear more weight than it can stand. Jay implicitly assumes that there is some essential structure or core meaning to the term that persists throughout the writings of the diverse authors that he studies. But is there? If so, it is not demonstrated.

To take only the two limiting cases. Ernst Bloch maintained a highly speculative sense of the term that sought to unify man and nature, subject and object, in a single cosmic process. Louis Althusser, by contrast, emphatically rejects the concept of totality even for limited purposes of historical explanation. Instead, he prefers to use terms like *overdetermination* and *structure in dominance* to describe the relations obtaining within a society. His critique of historicism and insistence on the "relative autonomy" of the superstructure points away from the tradition of holistic thinking with which Jay seeks to assimilate him. Indeed, it is never fully explained why Althusser merits inclusion as a contributor to a concept that he everywhere claims to reject.

In the final analysis I question whether the concept of totality can provide the unifying thread that runs throughout Western Marxism. Furthermore, one would like to have seen some kind of overall assessment of both the strengths and weaknesses of the concept, what it can help us to interpret and what it cannot. Until such an evaluation is provided, the concept of totality will remain vulnerable to attacks from methodological

individualists like Popper on the Right and Nietzschean deconstructionists like Foucault and Derrida on the Left.

STEVEN B. SMITH

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Public Life and Late Capitalism: Toward a Socialist Theory of Democracy. By John Keane. (New York: Cambridge University Press, 1984. Pp. vii + 340. \$39.50.)

Bureaucratic organizations increasingly administer the populations and social life of late capitalist societies. Keane's thesis is that this bureaucratization and rationalization of capitalism—the professional effort to administer, control, and depoliticize—both repress and incite demands and resistance that constitute and show the need for spheres of autonomous public life. Keane formulates a democratic road to socialism characterized by decentralized decision-making power and a plurality of public spheres.

He begins with an analysis of the internal contradictions of the bureaucracies and policies of the welfare state. It is an unstable and transitional social formation that cannot reproduce itself. Yet not even Max Weber was able to appreciate fully the limits of bureaucratic domination. Although he identified some of its internal tensions and was committed to a view of conflict based on a polytheism of values, Weber overlooked how his own analysis pointed to the possibility of public life. Keane's review of the theory of late capitalism in Claus Offe and Jürgen Habermas rejects Theodor Adorno's view of it as a totally administered society. Rather, it is a contradictory unity that fails to reproduce meaningful social and political relations and to rationalize state and corporate policy planning over time. Keane is critical of Habermas on the relationship between labor, science, and technology. Habermas's defense of instrumental rationality in work and relations with nature obscures the need for autonomous realms of public life in production and the ecological need not to treat nature as mere "standing reserve."

Keane's fifth chapter, where Michel Foucault's influence is most pronounced, is the finest portion of the book. He first reviews the contributions of Ferdinand Tönnies and John Dewey to a socialist theory of public life. Rejecting Habermas's theory of universal pragmatics as a framework, Keane proposes four elements in a socialist theory of public life. Its communication is sensuously embodied and depends upon gestures. A socialist public life values a rhetorical speech that produces new meanings whose linguistic density

remains ambiguous, beyond rational consensus. It emphasizes as well the aesthetic dimensions of communicative action. And, it incorporates festivity, mutually negotiated consent, and disobedience. In short, the forms of communication in socialist public life are free, pluralistic, and self-interrogating, that is, as disorderly as possible.

Keane leaves several questions unanswered. First, there is a too-ready acceptance of the legitimation crisis thesis. Deficiencies in mass loyalty may or may not pose legitimation problems for late capitalist societies and their regimes. Legitimation crisis can perhaps take different forms. It may be latent or manifest. It may constitute no threat, a threat to a regime or system, or a threat to both. Not every decline in mass loyalty necessarily means that legitimation crisis is at hand.

Second, Keane holds that a wide array of diverse local movements and struggles represents the existing framework for autonomous spheres of public life. However, these may not all be genuine forms of resistance. Although they share an opposition to established disciplinary controls, Keane does not indicate other principles of local, fragmented, particularistic resistances that suggest their commonality as autonomous spheres of public life. Perhaps none is necessary. But we do need to know which are forms of resistance constituting autonomous public life and which reinforce established mechanisms of power. Not every local movement embodies a sphere of autonomous public life.

Third, local movements, when they embody public life, may introduce a bit more slack in the social order of late capitalism. The relaxation of some disciplinary controls no doubt can revive and democratize public life. Perhaps this is the best that can be hoped for in the struggle against domination. Keane argues for a democratic socialism that limits the controls of centralized bureaucracies and maximizes the decentralized power and slack in the social order. But he does not adequately address the likelihood that the construction of legitimate forms of institutionalized power in a democratic socialist society will introduce its own forms of imposition and controls in order to prevail over and/or minimize the enormous powers of corporate and state bureaucracies.

CHARLES E. ELLISON

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Analyzing Marx: Morality, Power and History.

By Richard W. Miller. (Princeton, N.J.: Princeton University Press, 1984. Pp. xi + 319. \$30.00, cloth; \$8.95, paper.)

In the past decade or so, Anglo-American philosophers have discovered Marx. Richard Miller is, rightly, not content with the Marx his colleagues have found. Miller uses the techniques of analytic philosophy to interpret Marx, show the implausibility of technological determinist readings, and suggest his relevance to current controversies in analytic philosophy. But Miller does not demonstrate that analytic philosophy represents an essential or superior approach to Marx scholarship. Neglect of secondary sources particularly relevant to his project exaggerates Miller's claims for analytic philosophy and the uniqueness of his reading of Marx. Marx may clarify contemporary philosophical debates, but it is not clear that analytic philosophy makes a unique contribution to Marx scholarship.

For Miller, Marx rejects morality as a basis for social choice and political decision. No ethical theory—whether based on distributive, rights, utilitarian, or humanitarian claims—can justify institutional arrangements. Conflicting group needs and class interests, strategies for change, limits of rationality, and inevitable value differences suggest a new approach. In one of the book's more suggestive chapters, Miller sketches Marx's alternative to the moralization of politics, the experience and practice of class solidarity as the foundation of political commitment.

Miller holds that taking Marx seriously might improve social scientific analyses of power in advanced industrial societies. The concept of a ruling class, often dismissed as too outrageous to merit investigation, can be made intelligible and testable. A society has a ruling class when government actions serve the long-term interests of a group or coalition of groups (which controls the surplus produced by and opposes the interests of the majority) through definite but shifting mechanisms, and actions designed to break or disrupt this pattern are not permitted by government. Only extragovernmental uses of force can move resources enough to break the bond between policy and interests. Unfortunately, the effort to show the potential benefits of the concept overlooks studies of power published in the past decade.

Miller argues against Cohen's technological determinist reading of Marx's theory of history. Ruling-class dominance explains social stability. Historical change is rooted in the self-transforming tendencies of the means of production or, more narrowly, transformations in the economic

bases of class power or new patterns of access to increasingly productive power that are fettered by existing production relations. He offers a much broader and more political interpretation, but one that still grants great causal independence to economic structures.

Finally, Miller's attack shifts to positivism as a philosophy of inquiry that deforms Marxism and social science. Miller claims that positivist method denies intelligible and scientific status to his reading of Marx's theory of history because it has no empirical laws of predictive import and employs shifting definitions of key categories in the theory. Miller resolves the conflict by suggesting that positivists have fundamentally misconstrued the logic of the social and the natural sciences. He opts instead for a mode of inquiry in which explanation and confirmation require a plurality of causal accounts and empirically controversial alternatives to explain social and historical phenomena. On this view, a general theory describes the causal mechanisms that bring about certain social phenomena or produce specific social changes. Confirmation requires that hypotheses be tested by fair causal comparison, with rival theories also able to generate hypotheses to explain existing data. This method of inquiry, based on specificity of times and fields and dependent on agreed-upon background principles, makes it more difficult for scientists simply and arbitrarily to exclude claims with disturbing political and cultural effects from scientific investigation.

Miller ably uses Marx to enter current debates in ethical theory, the analysis of power, and philosophy of inquiry. One wonders, however, if analytic philosophy has a particularly important role to play in Marx scholarship or if, indeed, it can be used to construct a credible Marx without assistance from other key disciplines.

CHARLES E. ELLISON

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The Tragedy of Political Science: Politics, Scholarship, and Democracy. By David M. Ricci. (New Haven, Conn.: Yale University Press, 1984. Pp. xiii + 335. \$30.00.)

This is a provocative, rich, and rewarding book that advances a thesis that will be hotly and widely debated by political scientists. The tragedy of political science, according to Ricci, lies in the conflict between two goods: the commitment to science and the commitment to the good, wise, democratic life. Ricci's focus is American political science. In developing his supporting argument he highlights two crises in the conflict-ridden mar-

riage between science and democracy—one in the 1930s before World War II, the second in the decade from 1965 to 1975 when the behavioral establishment was challenged by a host of powerful critics. Ricci argues that the champions of a scientific political science were able to deal with the first crisis with the help of Dewey and Popper. The two philosophers advanced ideas that seemingly supported the compatibility of science and democracy. Some of these ideas helped the behavioralists to finesse both the premise that science could not defend democracy's values *and* the fact that science sometimes came up with findings that cast doubt on the truth of other liberal democratic postulates. The victory of the United States in World War II also helped to restore confidence in the marriage of science and democracy. These developments influenced behavioralism's understanding of, and high expectations about, science and liberal democracy. Political science could become a real science; liberal democracy was the good society! However, such confidence was soon to be undermined by practical events such as the civil rights movement and the Vietnam War and by another understanding of science—Thomas S. Kuhn's *The Structure of Scientific Revolutions* (University of Chicago Press, 1962). And although some in the behavioral establishment tried to coopt Kuhn, they were unable to still the adverse criticism of behavioral political science: that behavioral political science was a failure as science, and that it undermined a good, wise, democratic society.

As of this date, the second disturbing clash between science and the good, wise, democratic life has not been resolved. What can be done about this tragedy? Will it endure? Although Ricci emphasizes that the "tragic vision" enables some of us to learn from the tragic clash of ideals, he does not believe that most behavioral political scientists (devoted as they are to studying politics scientifically) will have the insight or courage to moderate their aspirations in order to do justice to democracy, the common good, or wisdom. Hence, that insightful minority seeking a more truthful and fruitful political science must practice "deception," or "robinhooding." Those in this minority must pretend to be involved in the "small conversation" of the behavioral political science establishment while they remain faithful to the "great conversation," which is the conversation concerned with the good life, wisdom, genuine human needs, illuminated by the intelligence derived from a study of history and rewarded by an appreciation of the political verities to be found in the "good" or "great books."

Those of us who have long been convinced that political science has three main concerns—the

search for the good political life, for a more scientific understanding of politics, and for wise judgment in politics and public policy—will applaud Ricci's critique. Empirical political scientists (even though disagreeing with Ricci's analysis) will be made more self-critical of their operative assumptions by this intelligent, lucid, scholarly, well-argued book. However, not all of those who share Ricci's concerns will necessarily endorse his view of, or response to, our tragic future. Some may hold that a genuine integration of the major components of political science is theoretically possible if the discipline could be persuaded to accept a concept such as that of political health as political science's central integrating concept. Such a concept would do justice to the discipline's three main concerns. And, of course, successful integration (theoretical and practical) would overcome the tragedy of political science. Such integration is, moreover, quite compatible with Ricci's rightful concern for ethical standards for the good political life, for a fruitful empirical political science, and for political wisdom. Only time will tell if Ricci is right about the continuing tragedy of political science, or if genuine integration of the discipline's concerns will come about.

NEAL RIEMER

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Ordinary Vices. By Judith N. Shklar. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 268. \$16.50.)

In this brilliant, psychologically insightful, and deeply humane book—which ranks cruelty first (and worst) among the vices, and argues for a skeptical, Montesquieuian “liberalism of fear,” a “division and diffusion of power that will permit the flow of political cruelty to be dammed up”—Montaigne and Hegel meet to wonderful effect: Montaigne supplies the “ordinary vices” (cruelty, hypocrisy, snobbery, treachery, and misanthropy) that Shklar will illuminate, and Hegel brings the method of illumination—the method of reading great works of literature, in the manner of the *Phenomenology*, as clues to the political and moral beliefs and practices of nations and of ages.

As it happens, Hegel's favorites, *Antigone* and *Hamlet*, are very nearly the only literary works that Shklar does not employ in her storytelling about vices, and above all about the political effects of practicing those vices—the political effect, for example, of “Europhile,” aristocracy-chasing snobbery in an American liberal democracy that values equality and simple manners. This she illustrates with a brilliantly compressed capsule version of Henry James's *Portrait of a*

Lady, characterizing expatriate Gilbert Osmond as an “already demoralized American aesthete” whose “European snobbery” leaves him “no community” and “no culture, a mere connoisseur of an alien one” (p. 114).

But Shklar doesn't simply illustrate ordinary vices; she brings them vibrantly to life through their most notable practitioners in the “treasure house” of our literature. Thus, snobbery springs onto the stage in the person of Molière's *Bourgeois Gentilhomme*, M. Jourdain, but also in that of Jane Austen's Lady Catherine de Bourgh and Sinclair Lewis's Seneca Doane (in *Babbitt*). Hypocrisy is made to live and breathe through Molière's *Tartuffe* (the “matchless black comedy” of Puritan fear and dissembling), but no less through Dickens's “immortal hypocrite” Uriah Heep and through Hawthorne's Judge Pyncheon—who hides private wickedness and political ambition behind a false “free and hearty manner” that may gain him “the honors of a republic.”

For all her striking resuscitation of snobbery and hypocrisy, however, Shklar thinks that some social critics—above all Hegel and Pascal—hated hypocrisy so much that insufficient room was left for a Montaignesque “cruel hatred of cruelty” as the vice to be put *first*. After all, as Shklar says, the artful dissembling of a Benjamin Franklin may be necessary if one hopes to influence a democracy without revealing one's “enormous superiority,” and “culture snobs,” silly as they are, support threadbare orchestras and museums.

Incomparably worse, unredeemed, is treachery—quickened by Conrad's Lord Jim, Thucydides' assorted traitors in *The Peloponnesian War*, and Josephus' betrayal of the Jews, but above all by remarkable retellings of Shakespeare's *Richard II* and *Coriolanus*. And misanthropy is made vivid partly through the writings of Machiavelli and Nietzsche—for whose “glory” and “creative cruelty” Shklar has no use at all—but especially through a dazzling reading of Shakespeare's *Timon of Athens*, the passionate hater who pays Alcibiades to destroy the *polis* that had despised and rejected him.

Nonetheless, misanthropy is for Shklar “ambiguous” and “politically a paradox”; whereas the “passionate and active” misanthrope like Timon is “indeed a political menace” who will cruelly “initiate slaughter,” the “purely intellectual and distrustful misanthrope”—Montaigne, Montesquieu, Kant in some moods—feels a reasonable fear (of limitless power) that can “serve as the basis of political decency, legal restraint and the effort to create limited government that would attenuate the effects of the cruelty of those who rule” (p. 193). Indeed, for Shklar, “misanthropy's finest hour” arrived with

Montesquieu's "legal" government, in which "procedure replaced personality," in which people "could do no better than to indulge in lesser vices in order to avoid worse ones" (p. 197). When one recalls the social cost of producing Aristotle's virtuous great man—slavery for the many who are "living instruments" to an aristocratic project of "self-perfection"—Shklar observes tartly, "the advantages of Montesquieu's self-assertive vices may correspondingly recommend themselves" (p. 236). (Aristotle also loses a four-page *agon* with Kantian egalitarianism (p. 233): "You do not have to be an aristocratic, superbly endowed Greek male to be a decent character. This [Kantian respect for "humanity"] is a thoroughly democratic liberal character . . . neither demanding nor enduring servility.")

But if, for Shklar, a "liberalism of fear" avoids the worst single vice (cruelty) by fragmenting power and relying on the lesser vices—Montesquieuan "commerce," Kantian self-love of intelligent "devils" (who embrace republicanism and peace from "legal" motives), Madisonian ambition-counteracting-ambition—that does not make liberalism "base," a decline from an imaginary antique age of civic virtue: "The alternative . . . still before us, is not one between classical virtue and liberal self-indulgence, but between cruel military and moral repression and violence, and a self-restraining tolerance that fences in the powerful to protect the freedom and safety of every citizen" (p. 5). That is the voice of an Enlightenment that continues to enlighten.

Ordinary Vices is a wonderful book. It preserves what is greatest in Hegelianism—the reading of Western literature from Thucydides and Euripides to Shakespeare, Molière, Hawthorne, and James as the best clue to the complexities and ambiguities of vice and virtue—while avoiding what is (arguably) least attractive in Hegel: excessive reverence for the "modern state" viewed as rational freedom sufficiently (and safely) "realized." For such a book, which deepens important political and moral questions by relating them to the greatest artworks of the past two millennia, one can only be profoundly grateful.

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Reading Althusser: An Essay on Structural Marxism. By Steven B. Smith. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 230. \$19.95.)

Louis Althusser's structuralist Marxism presented a significant and controversial challenge to

neo-Marxist social theory. Born in a dispute with the orthodox dialectical materialism of social democracy and communism, the neo-Marxism of Lukács, Gramsci, the Frankfurt School, and the French existential Marxists offered a vision of Marxism that was at once Hegelian, subjective, humanistic, and historical. From this perspective, the early works of Marx were not the prescientific sins of youth, but the keys to the interpretation of the mature Marxian world view. Perhaps a theoretical reaction was inevitable. From the mid-sixties through the mid-seventies, Althusser set himself to the task of attacking the fundamental principles of neo-Marxism. Without reviving the old dialectical materialism, Althusser formulated a structuralist interpretation of Marxism that was Spinozist, scientific, objective, nihilistic, and anti-historical. In the process, the work of the young Marx was relegated to the status of a footnote in intellectual history.

Steven B. Smith's lucid book will reward both the specialist and the interested general reader. Smith's exposition of the central concepts of Marxist structuralism—that is, the "symptomatic reading" of texts, the "rupture" of tradition as the basis for progress in science, the "overdetermination" of the ideological superstructure, the notion of human beings as the unconscious "bearers" of the relations of production—is far more accessible than it is in Althusser's frequently arcane works. And the intellectual context for this exposition touches many of the foundations of contemporary political thought. Althusser's concepts are compared to developments in orthodox Marxism, neo-Marxism, philosophy of science, Freudian psychoanalysis, structural linguistics, and structural functionalism.

Although Smith's exposition is free from the polemics that have marked most previous studies of Althusser, it is far from uncritical. Each of Althusser's central ideas is subjected to criticism. Many of these criticisms are Smith's own. Others are derived from sources as diverse as Karl Marx and Alasdair MacIntyre, Hannah Arendt and Alfred Schmidt, Roger Garaudy and Leszek Kolakowski, to name only some. Ultimately, the reader is left with the impression that Althusser's structuralist Marxism is as conceptually complex as it is substantively impoverished.

Smith's unpolemical treatment of Althusser is refreshing, but not above reproach. From time throughout his study, Smith associates Althusser's structuralism with technocratic modes of thought and political organization. This thesis, if it really was a thesis, is suggestive and well worth developing. But Smith does not confront Althusser with this technocracy thesis. Rather, Smith's eclectic mode of criticism whittles away at

Althusser's position, but never engages it directly in a critical dialogue.

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Max Weber and the Dispute over Reason and Value: A Study in Philosophy, Ethics, and Politics. By Stephen P. Turner and Regis A. Factor. (Boston: Routledge & Kegan Paul, 1984. Pp. ix + 274. \$28.95.)

In this study of Max Weber, Turner and Factor have produced a work of impeccable scholarship, intellectual excitement, and theoretical importance. The content of the study addresses two primary areas: Weber's position on the relationship between reason and value emphasizing its historical and theoretical origins, and the controversy surrounding the broader issue for nearly 100 years.

The authors make their way through the intricacies of the arguments by keeping focused on Weber's ideas. Their mastery of both the details of the political environment where the theoretical battles emerged and of the subtleties of dozens of different positions is particularly remarkable.

Despite the many positive aspects of the work, there are troubling questions about its ultimate value as a contribution to the development of social scientific theory. These are reflected in the primary purpose that Turner and Factor give for writing the book and in the nature of the work itself.

The authors open their first chapter with a question, "why should anyone care about the history of the arguments for and against Max Weber's conception of the relation of reason, or, more broadly, rational and cognitive knowledge, and values?" (p. 1). Their answer seems to be twofold: that Weber's position has not been accurately understood, and that the general impression that we now have a theoretical consensus in the social sciences on the reason/value issue is mistaken. They suggest that because the issue is still alive and vital, we have to return to dealing with it. But although they effectively illustrate the great confusion that rages in hidden form over the reason/value problem, Turner and Factor do not show how anything can be done to reopen fruitful discourse on the problem.

Why they do not reveals both the underlying condition that permitted the confusion and controversy to continue so long without being resolved and what may be the fundamental problem of contemporary theory as well. The historical chain of misinterpretations of Max Weber that Turner and Factor describe has the same

source as our current inability to discuss the reason/value issue meaningfully, let alone achieve mutual understanding on it: The differences in theoretical perspective have been and remain too fundamental.

At the end of the book, the reader is left with its opening question, Why bother? One hopes that after their exciting and stimulating history of the controversies, Turner and Factor will draw their conclusions about the relationship between reason and value, taking into account the limitations of the past so that discourse can begin again. But for them to do so would require taking sides in the historical controversies they describe so fairly; they would have to take sides because there are no fundamental theoretical approaches available to them other than those whose limitations they have spent chapters revealing. Following the only other available alternative, Turner and Factor appeal to us concerning the urgency of reopening the issue, but remain aloof themselves and, consequently, provide us with no idea of how to go about the important task. We remain caught in a theoretical cul-de-sac.

This is an important book in its own terms, however, and even though its major contribution to the future development of theory is negative, that contribution is enormously significant because it shows so dramatically the need today for neither new ideas nor for the recognition of the continuing significance of old ideas, but for a new idiom that could bridge the basic theoretical disparities keeping us from productive discourse.

RONALD E. PUHEK

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Le droit et les droits de l'homme. By Michel Villey. (Paris: Presses Universitaires de France, 1983. Pp. 170. FF78.)

For several decades the name of Michel Villey was almost synonymous with French legal philosophy. His publications, especially *La formation de la pensée juridique moderne* in 1968 and *Critique de la pensée juridique moderne* in 1975, culminated in a two-volume history of legal philosophy in 1979 and 1980.

The book under review is an abbreviated version of the course given by Villey before his retirement from the Sorbonne. It provides more, but also less, than its title promises. Over a half of the book is devoted to the genesis of the notion of "law" (pp. 15-104), while a considerable part of the rest demonstrates the absence of the concept of "human rights" in the tradition of the Catholic Church (pp. 105-130). The final part includes a highly articulate critique of Villey's views signed

by "E.F." (pp. 155-159) and the texts of "the French Declaration of the Rights of Man and of the Citizen of 1789," as well as of the "Universal Declaration of Human Rights" adopted in 1948 by the United Nations (pp. 161-163, 164-169).

All of Villey's work reflects the original Romanist specialization of an author captured by the culture of antiquity and its rebirth in the Middle Ages. His genetic concept of the notion of law leads from Aristotle over Cicero, Gaius, Ulpian, and Justinian's *Corpus Juris Civilis* to Thomas Aquinas, in whom Villey sees the apogee of legal thought. He considers further development to be only a nominalistic decline.

Yet the nominalism rejected by Villey seems rather to be particularism, an ideographic approach if we borrow Windelband and Rickert's terminology for this specific case. Also, Villey's presentation of the unfolding notion of law is rather his vision projected into history than a plain description.

Villey criticizes the concept of "human rights" as unreal, inconsistent, subservient to the interest of certain social groups or classes, and always prone to misuse. His own objective is "to search for what kind of norm is appropriate for the protection of the universality of men" (p. 92). "Legal rules are not the law: they describe the law. The law is a thing that precedes them (*jus quod est*), it is the object of permanent search and of dialectic discussion, with which our norms will never coincide" (p. 67). Villey compares the work of Gaius to a "quasi-sociology" that is different from ours by not being neuter, but by trying to discern "the just from the unjust" (p. 72).

There is no doubt that many legal theoreticians will disagree with Villey's opinion and believe that "human rights," even if only fictitious and "operatory," have nevertheless played an important positive role in legal development. Still, even if we do not accept Villey's conclusion, we have to recognize the significance of his book. It offers the same qualities that for years filled his weekly philosophical meetings with the cream of legal thinkers. On the basis of remarkable erudition Villey shows that law has much larger dimensions than legal norms reveal and forcefully invites us to study law as an eternal quest for justice in the service of society and humankind.

ZDENEK KRYSTUFEK

University of Colorado

The Emergence of Dialectical Theory: Philosophy and Political Inquiry. By Scott Warren. (Chicago: University of Chicago Press, 1984. Pp. ix + 262. \$26.00, cloth; \$12.95, paper.)

Scott Warren begins with the fairly recent debate in political science between behavioralists and those who would restore the classical tradition of political inquiry. Warren argues that although those working in the tradition of classical political theory have scored decisive objections against the behavioralists, the traditionalists have not always been very clear about what their alternative would look like. Indeed, says Warren, in many respects the behavioralists and traditionalists share a number of common assumptions, such as an "objectivist conception of reality, either in implicit or in explicit formulations" (p. 182). It is the purpose of Warren's book to show that there is a philosophical tradition which, although not well known to American political scientists, constitutes a third alternative. Most of Warren's book is an attempt to show that a seemingly disparate group of authors—Kant, Hegel, Marx, Merleau-Ponty, Sartre, Horkheimer, Adorno, and Habermas—constitute such a tradition: that of dialectical theory. In particular, Warren seeks to show how the dialectical tradition avoids what he regards as the false objectivism of both the behavioral and classical traditions while not succumbing to relativism.

Warren's is a work of original scholarship. Taken one at a time, Warren's interpretation of these authors is generally quite conventional. However, in showing how they constitute an identifiable tradition, he breaks new ground and makes a genuine intellectual contribution. For example, Warren calls attention to dialectical elements in Kant's thought that have been insufficiently noticed and demonstrates how these elements resonate in the work of a surprising group of authors. As might be expected, however, the major weakness of Warren's book is his frequent inability to bring the work of disparate authors together in more than superficial fashion. Too frequently the dialectical tradition that these authors share is reduced to such things as a concern for self-critical and self-reflective theory, a focus on totality, a critique of false objectivism, a concern with the historicity of knowledge, that philosophy be forward-looking, and so forth. The dialectical tradition comes to stand for all the good things that theory should be. Conversely, traditions that are not dialectical are sometimes treated in rather stereotypical fashion—as everything dialectics is not. For example, we learn that it is "misleading to approach reality in terms of linear causal relations" (p. 190). Quite so in many cases, but it is hardly the unique contribution of

dialectics to have pointed this out. What is lacking on Warren's part is an appreciation that perhaps some of the dialecticians whose positions he so carefully represents may themselves not have always faithfully represented the standpoints they criticize.

Warren claims in the first chapter that he is going to show how the dialectical tradition "may prove to be important for political scientists today" (p. 27). Yet he never does so in more than the very general terms discussed above, for example, political science should be concerned with relatedness and totality. In another study of dialectical thought, *The Origin of Negative Dialectics* (Free Press, 1977), Susan Buck-Morss has a wonderful discussion (pp. 182-184) of how Theodor Adorno's anti-system of negative dialectics

influenced the research program that produced the famous postwar study, *The Authoritarian Personality* (Harper, 1950). It is something like this that is missing in Warren's book—an attempt to link a philosophical tradition with actual political issues, phenomena, or studies. As Buck-Morss's study shows, it is possible to do so without trivializing dialectics, or reducing it to a formula.

Warren's book is nonetheless a serious effort that repays careful study. The scholarship is scrupulous, the topic is important, and Warren does indeed call attention to a neglected tradition within political science.

C. FRED ALFORD

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Empirical Theory and Methodology

History—The Human Gamble. By Reuven Brenner. (Chicago: University of Chicago Press, 1983. Pp. xiv + 239. \$17.50.)

With an economist's discerning eye, Reuven Brenner surveys the broad sweep of human history and offers two insights. On substance the Marines are right: When the going gets tough, the tough get going. The going invariably gets tough. On method most economists are wrong: Envy and ambition, not just greed, motivate people to act.

Brenner finds that demographic currents largely set the course for individual endeavors. When individual endeavors set the course for history, they do so by chance because people are gambling. People gamble when some exogenous shock upsets the social equilibrium, sending everyone scrambling for creative ways to reestablish stability. Thus, the invention of agriculture was not a random event that promoted population growth. On the contrary, population growth induced the invention. Calvinistic doctrine did not release mankind from penurious guilt into opulent materialism. Rather, it was "an endogenous, although random, event, representing a gamble on ideas that justified already existing behavior patterns and so appealed to a segment of the public when the wealth distribution changed" (p. 124).

In one sense Brenner's method is enlightening but unexceptional. Behind his reading of history is

Frank Knight's classic notion of uncertainty and its signal role in inducing people to innovate. Traditional assumptions from economics power the analysis. The pursuit of wealth matters, as does the tendency of individual activities to generate social equilibrium.

In another sense, Brenner's method parts company with its intellectual forebearers. He assumes that an individual's "satisfaction depends not only on his wealth . . . but also on the fraction of people who are richer than he is" (p. 3). The key to understanding human events, then, is self-preservation, characterized here as the probability of securing at least one's relative position in the distribution of wealth, if not more. An agile mathematical treatment (relegated largely to appendixes) buttresses Brenner's exposition.

Brenner presents evidence that the perception of inequality, and especially changes in that perception, affect people's attitudes towards risk. Most susceptible to taking risks are those who suffer most from a change in the distribution of wealth. As a result, we observe systematic and regular behavior with respect to lotteries, crime, discrimination against minorities (anti-Semitism in particular), entrepreneurial activity, and a host of phenomena from tribal customs to usury laws.

Many political scientists will recognize Brenner's message if not his method. The trust and customs that govern intimate societies break down

as population grows and anonymity prevails. Political institutions emerge because they are more efficient than customs but serve the same purpose: to sustain social stability in terms of the distribution of wealth. The pivotal position of distribution in this theory identifies Brenner's method as one more contribution to the neo-institutionalist school. Its adherents combine the individual's perception of fairness, equity, or norms with self-interest to explain political behavior.

Editorial lapses make more difficult an argument already burdened by being counterintuitive, but logical lapses deserve greater note, especially because Brenner expounds on the notion of causality. First, he fails to establish an empirical link critical to his argument, that between population increases and increased wealth inequality. Second, the weight of his evidence is not always compelling, even when he uses appropriate statistical rigor. His examples seem at times incomplete. For example, one wonders whether discrimination really abates in stable societies or merely takes forms alien to Brenner's perspective. Finally, he treats envy and ambition as one. They are not. If envy motivates people, then, as Brenner suggests, equality stifles creativity and thinking; inequality excites it. But equality motivates ambitious people to excel, perhaps more so than does inequality. Despite such lapses, Brenner's basic insights remain exciting and rich, well worth further attention.

STEVEN M. MASER

Willamette University

Political/Military Applications of Bayesian Analysis: Methodological Issues. By Douglas E. Hunter. (Boulder, Colo.: Westview Press, 1984. Pp. xviii + 293. \$22.50, paper.)

The essential feature of Bayesian analysis is that a priori estimates of the probability of an event are altered in light of subsequent observations. The attempt to formally apply this technique in political science is to be commended. Hunter makes a start at demonstrating possible uses.

In the book's first chapter, Hunter reviews some basic concepts in probability theory. Here and throughout the rest of the book he sometimes tediously proceeds step by step through the arithmetic in light of his desire to include the nontechnical reader as part of the intended audience.

Basic aspects of Bayesian analysis are discussed in the second chapter, an "Introduction to Bayesian Analysis." The rest of the book consists entirely of models that are variations of those that appear in this chapter. The book's sometimes-

colloquial style makes it appear that some sections were simply transcribed from lectures. One substantive weakness that might be an outgrowth of this style is Hunter's use of the phrase "revised probability" (p. 60) to represent what are typically the a posteriori estimates. If he feels that these conditional probabilities should be "revised," then their estimates should be recomputed with the new figures until the result stabilizes.

After a concise argument of the benefits of Bayesian analysis, problems associated with this technique consume the rest of the book.

The material in the remaining chapters follows a canonical form. First, a problem associated with Hunter's paradigm is stated briefly. Second, that problem is more fully described. Third, a solution is proposed, or extensive obstacles are indicated. A variation is problem 32 (p. 237), which discusses "source reliability"; no solution appears, and suggestions by readers are solicited. A similar request is made in problem 27 (p. 208), which deals with overweighting the chances of non-occurring events. Fourth, a general and extended discussion of each problem is presented.

The problems are occasionally illuminated by a historical example. Given that Hunter is presumably concentrating on methodological issues that have political or military applications, these examples are not only too sparse, but are also not sufficiently rich in texture. One well-written section is Hunter's discussion of the problem that sub-hypotheses may be buried in the model. He begins (p. 103) with two hypotheses. The first is that "the Soviet Union has 'decided to invade Poland within one month.'" The second hypothesis is "the Soviet Union has not 'decided to invade Poland within one month.'" Several sub-hypotheses are discussed on the following pages: the Soviet Union has decided to invade after a month, or she has decided not to invade, or has decided not to decide. Coincidentally, the examples throughout the text universally posit potential nefarious behavior on the part of America's adversaries. Not even ostensibly neutral examples such as "Graf von Berchtold decides to send an ultimatum to Serbia" are entertained.

A more serious problem in applying this technique directly from this work is the high degree of uncertainty in the parameters. For example, the probability for an event given that "the North Koreans have decided to attack within a year" (p. 117) is assessed at 0.8. From where does this 0.8 come? The problem is acknowledged (p. 258), but not resolved. Unlike Glenn Snyder and Paul Diesing's *Conflict among Nations* (Princeton University Press, 1977), where ordinal rankings of a dozen analyzed cases from the diplomatic and historical literature are interwoven throughout the

manuscript, this Bayesian method requires reasonably accurate interval level data.

Four other comments on style are in order. First, frequently the denominator of a fraction is written on two lines. I found this at first confusing, and then unnerving. Second, Hunter makes a unique contribution to the sir/madam controversy by a use of "s(he)" (p. 211). Third, the book contains no index. Fourth, it is refreshing to see Hunter regularly cite student con-

tributions.

In many ways, Hunter thoughtfully raises a number of questions and methodological issues associated with the use of Bayesian analysis. Those expecting to find practical applications for those methodological issues, however, will find this a preliminary rather than definitive text.

WALTER W. HILL, JR.

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Annotations

Access to Medical Care in the U.S.: Who Has It, Who Doesn't. By Lu Ann Aday, Gretchen V. Fleming, and Ronald Andersen. (Chicago: Pluribus Press, 1984. Pp. 240. \$24.95, paper.)

Based upon data from a 1982 National Survey of Access to Medical Care, this book provides an overview of policy, ethical, conceptual, and empirical issues; the measurement of access, potential and realized access; equity of access; and special problems of access. Volume contains several methodological appendixes, an executive summary, and an index.

Computers in Criminal Justice Administration and Management: Introduction to Emerging Issues and Applications. By William G. Archambeault and Betty J. Archambeault. (Cincinnati: Anderson Publishing, 1984. Pp. 186. \$12.95.)

This text explores computers as management tools in criminal justice administration and gives particular emphasis to the use of microcomputers in data base management, networking, word processing, and computer-assisted instruction. Chapters include an overview of computer terminology, hardware, and software, information management, organizational communication, instruction, and criminal justice issues in computer technology. Volume includes glossary, bibliography, and index.

A Middle East Studies Handbook. By Jere L. Bacharach. (Seattle: University of Washington Press, 1984. Pp. 172. \$20.00, cloth; \$9.95, paper.)

This revision of the 1974 edition includes brief sections on transliteration and Islamic names, an Islamic calendar and conversion table, lists and genealogical tables of dynasties, rulers, and administrators, a historical chronology from 570 through 1983, a historical atlas, and more. Volume contains a general index and index to the historical atlas.

Data Construction in Social Surveys. By Nicholas Bateson. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. x + 147. \$28.50, cloth; \$12.50, paper.)

Focusing on the construction of survey items, this text examines the basic concepts of data construction, the validation of survey data, design of a data matrix, and procedures for data construction. Volume includes a brief discussion of measurement error, references, and a subject index.

The Being of the Beautiful: Plato's *Theaetetus*, *Sophist*, and *Statesman*. Translated by Seth Benardete. (Chicago: University of Chicago Press, 1984. Pp. li + 539. \$42.50.)

This modern English translation presents together the three Platonic dialogues formulating a conception of philosophy while Socrates prepares for his trial. Text includes a brief discussion of *Hippias Major* and commentaries following each of the dialogues. Volume also includes select bibliography and an index.

Secondary Analysis of Available Data Bases. Edited by David J. Bowering. (San Francisco:

Jossey-Bass Publishers, 1984. Pp. 115. \$8.95, paper.)

This text advocates the secondary analysis of available data bases instead of the more costly and time-consuming data gathering associated with most policy research. Chapters include discussions of available large-scale data bases, the technical requirements for subfiling these data sources, and examples of how such subfiles might be analyzed. Volume contains index.

A Guide to Marx's *Capital*. By Anthony Brewer. (New York: Cambridge University Press, 1984. Pp. xiv + 211. \$29.95, cloth; \$8.95, paper.)

Designed as a companion volume for Marx's *Capital*, this guide provides a chapter-by-chapter introduction to, and explanation of, Marx's argument. Text includes a general introduction on the roots of Marx's political economy and appendixes instructing the reader on the prefaces and afterwords to *Capital*, *The Communist Manifesto*, and the preface and introduction to *A Contribution to the Critique of Political Economy*. Volume also contains glossary and brief index.

A Campaign of Ideas: The 1980 Anderson/Lucey Platform. Compiled by Clifford W. Brown, Jr. and Robert J. Walker. (Westport, Conn.: Greenwood Press, 1984. Pp. lxxii + 486. \$35.00.)

This compilation of documents from the Anderson-Lucey presidential campaign of 1980 includes the full platform, a summary version of the platform titled "Rebuilding a Society that Works: An Agenda for America," and the "Budget Impact Statement." Volume includes index.

Harry S. Truman: A Bibliography of His Times and Presidency. Compiled by Richard Dean Burns. (Wilmington, Del.: Scholarly Resources, 1984. Pp. xlviii + 297. \$50.00.)

This bibliography of Truman and his times includes more than 3,000 annotated entries on Truman himself, the Truman presidency, personalities in the Truman administration, domestic affairs (agriculture, business, the economy, labor, and veterans; the arts, culture, health and welfare, science and technology; civil rights, internal security and civil liberties, politics and public opinion, and the Supreme Court), foreign affairs (the end of World War II, the beginnings of the Korean

War, and bilateral relations), military affairs, the Korean War, and general reference works. Volume includes both an author and a subject index.

Social Impact Assessment and Monitoring: A Guide to the Literature. By Michael J. Carley and Eduardo S. Bustelo. (Boulder, Colo.: Westview Press, 1985. Pp. xii + 250. \$32.50.)

Reviewing some 600 publications on social impact assessment, this text provides an introduction to social impact assessment generally, its use in policymaking and planning, various methodological issues, problems, and applications, interdisciplinary perspectives on social impact assessment, and its use in developing countries. Volume also includes an overview of selected periodicals and other bibliographies and an author index.

Women in China: Bibliography of Available English Language Materials. Compiled by Lucie Cheng, Charlotte Furth, and Hon-ming Yip. (Berkeley: Institute of East Asian Studies, University of California, 1984. Pp. xiv + 109. \$12.00, paper.)

This bibliography contains more than 4,000 entries catalogued under such topics as education; emancipation movements; health and population; labor and production; literature, art, and folklore; marriage and the family; politics and the law; psychology and religion; science, technology, and the military; sports and fashion; and Western women in China. Volume includes an index of authors and an index of Chinese women as subjects.

Countries of the World and Their Leaders Yearbook 1985: Vol. 1, Chiefs of State and Cabinet Members of Foreign Governments; States of the World's Nations; U.S. Embassies, Consulates, and Foreign Service Posts; Background Notes: Afghanistan-Luxembourg; Vol. 2, Macau-Zimbabwe; Foreign Travel; International Treaty Organizations; Climates of the World. (Detroit: Gale Research, 1984. Pp. xiv + 1,546. \$95.00/set.)

This compilation provides the most recent State Department reports on some 168 countries of the world. In addition to the background notes for Afghanistan through Luxembourg, Volume 1 contains information on chiefs of state and cabinet members of foreign governments, a brief survey of the world's nations, and a section on

U.S. embassies, consulates, and foreign service posts. Volume 2 includes the background notes for Macau through Zimbabwe, a section on foreign travel, a section on international treaty organizations, and a brief section on climates of the world. Volumes contain no index.

Political Parties of the World, 2nd ed. Compiled and Edited by Alan J. Day and Henry W. Degenhardt. (London: Longman Group, and Detroit: Gale Research, 1984. Pp. x + 602. \$90.00.)

This edition covers more than 1,300 political parties found throughout the world. Country entries include a brief political introduction followed by a description of each legally active political formation, including such information as leadership, date of founding, history, and political orientation. Appendixes catalog international party groupings under such rubrics as Democratic Socialists, Christian Democrats, Liberals, Conservatives, Ecologists, and Communists and Marxist-Leninists. Volume contains index of parties and index of names.

Information Sources in Politics and Political Science: A Survey Worldwide. Edited by Dermot Englefield and Gavin Drewry. (Boston: Butterworth Publishers, 1984. Pp. xviii + 507. \$69.96.)

Designed as an international survey of the discipline, this volume by British scholars and librarians includes discussions of library and bibliographical aids to the study of politics, essays on comparative politics, political behavior, international politics and international relations, political thought, and group approaches to the study of politics, another section of essays on the study of politics and government in the United Kingdom, and a final collection of essays on the study of politics and government elsewhere. Volume contains an index.

The Europa Year Book, 1984: A World Survey: Vol. 1, International Organizations, Europe, Afghanistan-Burundi; Vol. 2, Cameroon-Zimbabwe. (London: Europa Publications, and Detroit: Gale Research. Pp. xx + 2,761. \$210.00/set.)

The twenty-fifth edition of this yearbook, the survey includes a section on the function, structure, and activities of more than 1,500 international organizations; a section on the countries

of Europe plus the USSR, Cyprus, and Turkey; and another section on Africa, the Americas, Asia, and Australasia. Country information includes descriptive overview, statistical survey, and a directory of government agencies, political organizations, religion, the press, the media, finance, trade and industry, and more. Volume 1 contains an index of international organizations, and Volume 2 contains a brief index of territories cited in both volumes.

Documents on the Foreign Policy of Israel: Vol. 2, October 1948-April 1949. Edited by Yehoshua Freundlich. (Jerusalem: Israel State Archives, 1984. Pp. 680 and lxvi + 251. \$69.50, includes companion volume.)

Official Israeli documents included in the main volume and referenced in the English companion volume focus on the conclusion of Israel's war for independence, Israel's relations with neighbors and the United States, the role of the United Nations, and such issues as Jerusalem, Arab refugees, Israel's admission to the United Nations, and more. Companion volume contains extensive index.

The Papers of Dwight David Eisenhower: Vol. 10, Columbia University; Vol. 11, Columbia University. Edited by Louis Galambos. (Baltimore: Johns Hopkins University Press, 1984. Pp. xxiii + 845 and vi + 857. \$60.00/set.)

These two volumes chronologically follow Eisenhower from termination of active duty as Chief of Staff in February, 1948 through his presidency at Columbia University up to his assumption of duties as Supreme Allied Commander of NATO in 1950. The selected and annotated papers cover the transition period from military to university presidency in 1948, his continued role in national affairs, programs and problems at Columbia, and his reemergence as a key military figure. Volume 11 contains an extensive chronology and index for the 1948 to 1950 period.

Foreign Relations of the United States, 1952-1954: Vol. 12, Part 1: East Asia and the Pacific. Edited by John P. Glennon. (Washington, D.C.: U.S. Government Printing Office, 1984. Pp. xiv + 1,113. \$20.00.)

Part 1 of this volume contains official documents regarding the military role of Australia and New Zealand, ANZUS relations more generally, Five Power discussions on Southeast Asia, the

Southeast Asia problem, Pacific security and Chinese communist aggression more generally, U.S. policy toward Formosa and Japan as it affects Chinese policy, Asian economic development, a collective security pact for Southeast Asia, the French in Indochina, and more. Part 1 contains index.

Foreign Relations of the United States, 1952-1954: Vol. 15, Part 1 and Part 2: Korea. Edited by John P. Glennon. (Washington, D.C.: U.S. Government Printing Office, 1984. Pp. xxix + 1,151 and xxix + 845. \$29.00.)

This important volume contains official U.S. documents preceding and following the Korean Armistice. Part 1 focuses on the armistice negotiations at Panmunjom, economic and political crises in South Korea, United Nations debates, and decision making in both the Truman and Eisenhower administrations. Part 2 includes documents on problems with Rhee and the armistice, final negotiations, postarmistice issues such as Korea's reconstruction, military and economic aid, and the redeployment of U.S. troops in Korea. Volume contains index.

The Caribbean Basin to the Year 2000: Demographic, Economic, and Resource-Use Trends in Seventeen Countries, a Compendium of Statistics and Projections. By Norman A. Graham and Keith L. Edwards. (Boulder, Colo.: Westview Press, 1984. Pp. xv + 165. \$18.50, paper.)

Volume contains statistics and case commentaries for 17 Caribbean Basin countries. Chapters include a discussion of method and data sources; country profiles; a discussion of interrelationships among demographic, economic, and environmental trends; and implications for U.S. interests and policy. Volume has listing of tables and figures but no index.

Western Interests and U.S. Policy Options in the Caribbean Basin: Report of the Atlantic Council's Working Group on the Caribbean Basin. Edited by James R. Green and Brent Scowcroft. (Boston: Oelgeschlager, Gunn & Hain, Publishers, 1984. Pp. xix + 331. \$27.50, cloth; \$12.50, paper.)

Chapter 1 of this volume contains the policy paper of the Atlantic Council's Working Group on the Caribbean Basin, defined as the United States, the Caribbean Islands, Central America,

Colombia, Guyana, Mexico, Suriname, and Venezuela. Other chapters include signed essays on a general overview of U.S. policy in the region, economic, social, and political issues, security issues, the Southern flank, migration, energy, and options for U.S. policy. Volume contains index.

Encyclopedia of American Political History: Studies of the Principal Movements and Ideas, 3 vols. Edited by Jack P. Greene. (New York: Charles Scribner's Sons, 1984. Pp. xiii + 1,420. \$180.00/set.)

This topical guide to American political history contains some 90 specially commissioned essays. Topics include agricultural policy, the American Revolution, the Articles of Confederation, the Bill of Rights, church and state, citizenship, civil disobedience, the civil rights movement, Congress, the Constitution, the Declaration of Independence, federalism, the Federalist Papers, feminism, Jacksonian and Jeffersonian democracy, labor movements, lobbies and pressure groups, machine politics, the New Deal, pluralism, political parties, the presidency, Progressivism, Reconstruction, slavery, sectionalism, and secession, state government, suffrage, tariff policies, taxation, territories and statehood, veterans' movements, and many more. Third volume contains an index.

The PAC Directory: Vol. 2, The Federal Committees. Edited by David U. Greevy, Chadwick R. Gore, and Marvin I. Weinberger. (Cambridge, Mass.: Ballinger Publishing, 1984. Pp. ix + 873. \$195.00.)

This book and its companion volume on the federal candidates presents a gold mine of information on political action committees. Financial tables for more than 1,600 PACs report gross receipts and disbursements for 1981 and 1982, contribution breakdowns by size, Senate or House recipient, incumbency, and winner, plus who received the largest contributions from that PAC. Volume also includes a PAC directory, a listing of PACs and sponsoring organizations, and a top-200 rank of PAC contributors to federal candidates. Volume has no index.

Encyclopedia of Associations, 1985: Vol. 4, International Organizations. Edited by Katherine Gruber. (Detroit: Gale Research, 1984. Pp. 508. \$160.00.)

This volume focuses on nonprofit organizations

that are international in scope and headquartered outside the United States. Some 2,000 entries are catalogued under agricultural organizations and commodity exchanges; legal, governmental, public administration, and military organizations; scientific, engineering, and technical organizations; educational organizations; cultural organizations; social welfare organizations; health and medical organizations; public affairs organizations; fraternal, foreign interest, nationality, and ethnic organizations; religious organizations; veteran, hereditary, and patriotic organizations; hobby and avocational organizations; athletic and sports organizations; and labor unions, associations, and federations. Basic entry typically includes address, date of founding, number of members, staff, language, brief description of purpose, publications, conventions or meetings, and more. Volume includes alphabetical index, geographic index, and executive officer index.

The President and the Council of Economic Advisors: Interviews with CEA Chairmen.

Edited by Erwin C. Hargrove and Samuel A. Morley. (Boulder, Colo.: Westview Press, 1984. Pp. 511. \$29.50, paper.)

This text compiles oral histories of 10 former chairmen of the Council of Economic Advisors as a means of illuminating the relationship between the CEA and the President, executive departments, and federal financial agencies. Sections on Leon Keyserling, Arthur Burns, Raymond Saulnier, Walter Heller, Gardner Ackley, Arthur Okun, Paul McCracken, Herbert Stein, Alan Greenspan, and Charles Schultze include both a summary history and an oral interview. Volume contains bibliography but no index.

Introduction to Causal Analysis: Exploring Survey Data by Crosstabulation. By Ottar Hellevik. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. xxiii + 211. \$28.50, cloth; \$13.50, paper.)

Written as a crosstabulation approach to path analysis, this text examines crosstabulation generally, the meaning and measurement of causation, the development of causal models, deductions from causal models, design and design problems, analyzing interaction, regression analysis, log-linear analysis, and interpreting results. Volume also contains references and subject index.

British Intelligence in the Second World War: Its Influence on Strategy and Operations: Vol. 3,

Part 1. By F. H. Hinsley et al. (New York: Cambridge University Press, 1984. Pp. xvi + 693. \$39.50.)

This volume contains Parts 8 through 12 of the official history of British intelligence strategy and operations during the Second World War. Most chapters cover the time period from June 1943 to June 1944 and include a general assessment of strategic intelligence, the Mediterranean theater, the war at sea, the air war, and V-weapons. Volume includes 27 appendixes and an extensive index.

The Annual Register: A Record of World Events, 1983. Edited by H. V. Hodson. (Detroit: Gale Research, 1984. Pp. xiv + 543. \$90.00.)

The 225th in its series, this volume provides a registry of recent events in the United Kingdom; the Americas and the Caribbean; the USSR and Eastern Europe; Western, Central, and Southern Europe; the Middle East and North Africa; Equatorial Africa; Central and Southern Africa; South Asia and the Indian Ocean; South-East Asia and East Asia; Australasia and the South Pacific; and international organizations. Topical chapters cover events in religion, the sciences, law, the arts, sports, and economic and social affairs. Volume also includes a chronicle of principal events in 1983 and an index.

An Atlas of EEC Affairs. By Ray Hudson, David Rhind, and Helen Mounsey. (New York: Methuen, 1984. Pp. xiv + 158. \$31.00, cloth; \$12.95, paper.)

This graphic and descriptive survey looks at the history, organizational structure, powers, and policies of the European Community, its changing population, its labor market, its economic performance and resources, its social conditions, consumption patterns, and lifestyles, and examines issues facing the community during the rest of this century. An appendix discusses the technical basis for the atlas. Volume also includes author, location, and subject indexes.

Bridges to Knowledge in Political Science: A Handbook for Research. By Carl Kalvelage, Albert P. Melone, and Morley Segal. (Pacific Palisades, Calif.: Palisades Publishers, 1984. Pp. 153. \$6.95, paper.)

This text has been designed as a guide to sources in political science and a guide for concep-

tualizing, researching, and writing term papers. Chapters discuss such matters as developing a topic and hypotheses, finding and using available information, the use of original sources, and footnotes and bibliographies. Volume contains index.

The Role of Courts in American Society: The Final Report of the Council on the Role of Courts. Edited by Jethro K. Kieberman. (St. Paul: West Publishing, 1984. Pp. xiii + 171. \$10.00, paper.)

This volume examines the proper role of courts in American society. Text includes an executive summary and chapters on the role of courts as a public issue, what courts are doing and have been doing, what courts do and do not do effectively, strengthening the capacity of courts, plus conclusions and recommendations. Volume contains some tables and appendixes but no index.

Churchill and Roosevelt: The Complete Correspondence: Vol. 1, Alliance Emerging, October 1933-November 1942; Vol. 2, Alliance Forged, November 1942-February 1944; Vol. 3, Alliance Declining, February 1944-April 1945. Edited by Warren F. Kimball. (Princeton, N.J.: Princeton University Press, 1984. Pp. clxiv + 674, 773, and 742. \$150.00/set.)

This massive three-volume set contains a complete collection of the Churchill-Roosevelt correspondence. Headnotes provide historical background to the documents. Volume 1 contains editorial and bibliographical notes and an extensive digest of documents. Materials are arranged chronologically and cover a period from October, 1933 through Roosevelt's death in 1945. Volume 1 ends just before the North African invasion in November, 1942; Volume 2 contains documents to February 29, 1944; and the final volume ends with Truman's assumption of the presidency. Each volume contains maps and photographs, and the final volume contains a glossary and an index.

Strategic Requirements for the Army to the Year 2000. Edited by Robert H. Kupperman and William J. Taylor, Jr. (Lexington, Mass.: D.C. Heath, 1984. Pp. xi + 539. \$35.00.)

The results of a forecasting project guided by a steering committee from the Center for Strategic and International Studies and an Army Study Advisory Group, this volume includes essays on forecasting as a policy instrument; U.S. interests

in the 1990s; the American public and U.S. foreign and defense policies; future trends and their impact on the army; arms and energy technology; army mobilization; manpower and training; unconventional war; terrorism; Soviet proxy warfare; the world environment; and several regional studies. Volume contains an index.

Aristotle: The Politics. Translated by Carnes Lord. (Chicago: University of Chicago Press, 1984. Pp. 284. \$35.00.)

This modern English translation of *The Politics* includes the editor's brief introduction and outline of the argument, annotative and connotative notes, a glossary of key terms, and an index of names.

The Diary of Beatrice Webb: Vol. 3, 1905-1924, "The Power to Alter Things." Edited by Norman MacKenzie and Jeanne MacKenzie. (Cambridge, Mass.: Harvard University Press, 1984. Pp. xvii + 460. \$25.00.)

This third volume of the diary covers Beatrice Webb's service on the Royal Commission investigating the Poor Law and the public controversy surrounding her campaign, her trip around the world with husband Sidney, the days of World War I, and the events surrounding Sidney's election to Parliament in 1922. Volume includes photographs, a short bibliography, and an index.

Selected Letters of Edmund Burke. Edited by Harvey C. Mansfield, Jr. (Chicago: University of Chicago Press, 1984. Pp. viii + 497. \$27.50.)

Based on the 10-volume edition of Burke's complete correspondence, this selected edition includes correspondence on private and public life, literary friends and philosophical concerns, toleration and religion, early and later views of party, America, reform and revolution, counterrevolution, India, and Ireland. Volume also includes a discussion of Burke's theory of political practice, a sketch of his life, and an index.

Middle East and North Africa 1984-85, 31st ed. (London: Europa Publications, and Detroit: Gale Research, 1984. Pp. xx + 801. \$120.00.)

The 31st in its series, this volume includes a general survey of the Middle East and North African region, a selection on regional organiza-

tions, and 24 country surveys. The general survey contains essays on the region's religions, the Arab-Israeli conflict, its natural resources, and more. Country surveys provide basic information on physical and social geography, history, economy, a general statistical overview, a directory of constitution and government, legislature, diplomatic representation, judicial system, religion, the press, publishing, radio and television, finance, trade and industry, transport, tourism, atomic energy, defense, education, and a bibliography. Volume contains some maps but no index.

Sisterhood Is Global: The International Women's Movement Anthology. Compiled and edited by Robin Morgan. (Garden City, N.Y.: Anchor Books, 1984. Pp. xxiii + 815. \$24.95, cloth; \$12.95, paper.)

This international feminist anthology covers 70 nations and the United Nations. A statistical preface precedes each contribution and includes such information as demography, government, economy, gynography, herstory, and mythography. Individual contributions are very heterogeneous. Volume includes a glossary, bibliography, and index.

Handbook of the Nations, 4th ed.: A Brief Guide to the Economy, Government, Land, Demographic, Communications, and National Defense Establishment of Each of 191 Nations and Other Political Entities. Compiled by the National Foreign Assessment Center. (Detroit: Gale Research, 1984. Pp. xiv + 274. \$64.00.)

This fourth edition is a reprint of *World Factbook—1984* prepared by the Central Intelligence Agency's National Foreign Assessment Center. The 191 entries are alphabetized by country and include information on land, people, government, economy, communications, and defense forces. Appendixes include a diagram on the United Nations as a system, selected United Nations organizations, selected international organizations, country membership in selected organizations, and a table of conversion factors.

The Reagan Record: An Assessment of America's Changing Domestic Priorities. Edited by John L. Palmer and Isabel V. Sawhill. (Cambridge, Mass.: Ballinger Publishing, 1984. Pp. 440. \$28.00, cloth; \$12.95, paper.)

This volume examines the Reagan shift in

domestic policy over the past four years, assesses consequences, and explores implications. Text includes a general overview of Reagan's goals and philosophy, a look at governance, the economy, the budget, natural resources and the environment, social policy, federalism and the states, nonprofit organizations, businesses, and family incomes. Volume also includes some statistical appendixes, a summary of major social programs, and an index.

The Foreign Office and the Kremlin: British Documents on Anglo-Soviet Relations 1941-45. Edited by Graham Ross. (New York: Cambridge University Press, 1984. Pp. xi + 303. \$49.50.)

The documents selected for inclusion in this volume emphasize the British Foreign Office and professional diplomats. Volume includes some 46 documents generally illustrating the Foreign Office attitude toward the war and introductory chapters on Anglo-Soviet relations up to 1941, developments in 1941, Eden's visit to Moscow and the Anglo-Soviet treaty, and the periods up to the Russian break with Poland, between Katyn to Tehran, then Yalta, then Potsdam, and finally Moscow. Text contains bibliography but no index.

Politics and Government in the Federal Republic of Germany: Basic Documents. Edited by Carl-Christoph Schweitzer et al. (Leamington Spa, Great Britain: Berg Publishers, and New York: St. Martin's Press, 1984. Pp. xix + 443. \$37.50, cloth; \$15.95, paper.)

This compilation of key documents on postwar Germany include several on the origins of the Federal Republic of Germany, 1944 to 1949, the Bundestag, the chancellor, cabinet, and president, the judiciary, basic rights and constitutional review, federalism, political parties, public opinion, economic and social policy, foreign policy, defense policy and the armed forces. Berlin, and the Federal Republic and the German Democratic Republic. Volume also includes several statistical tables, a select bibliography, and an index.

Foreign Relations of the United States, 1952-1954: Vol. 2, Part 1 and Part 2: National Security Affairs. Edited by William Z. Slany. (Washington, D.C.: U.S. Government Printing Office, 1984. Pp. xxvi + 884 and xxvi + 1,083. \$28.00.)

Official documents included in Part 1 of this volume focus on U.S. government's national security policy generally (1952 to 1954), its objectives and programs, its estimates of national security threats, its military posture, and its organization for national security. Documents in Part 2 center upon U.S. atomic energy policy, arms control, U.S. strategic bases in the United Kingdom and Canada, the U.S. International Information Administration, and other international information activities. Volume includes index.

American Landmark Legislation: The Railway Labor Act of 1926, Vol. 1. Edited by Irving J. Sloan. (Dobbs Ferry, N.J.: Oceana Publications, 1984. Pp. 506. \$100.00.)

This compilation of documents focuses on the Railway Labor Act of 1926. Included are a brief legislative history of the act, the statute itself, hearings before and reports of the Senate Committee on Interstate Commerce and House Committee and Interstate and Foreign Commerce, the congressional debates of 1926, and text of the Supreme Court decision in *Texas & New Orleans Railroad Company et al. v. Brotherhood of Railway & Steamship Clerks et al.* Volume has no index.

Legislative Acts of the USSR, Book 3. Edited by L. N. Smirnov. (Moscow: Progress Publishers, and Chicago: Imported Publications, 1984. Pp. 358. \$7.95.)

The third in its series, this volume contains translations of major Soviet legislation in the areas of public health, public education, marriage and the family, the judicial system, civil legislation, criminal legislation and procedure, corrective labor, the utilization of historical and cultural monuments, universal military service, and pensions and allowances to collective farm members. Text includes brief index.

Social Research on Arabs in Israel, 1977-1982: A Bibliography. Edited by Sammy Smooha. (Haifa: University of Haifa, Jewish-Arab Center, 1984. Pp. 100. \$10.00, paper.)

Prepared as a supplement to *Social Research on Arabs in Israel 1948-1976, Trends and an Annotated Bibliography*, this volume contains about 240 citations for social scientific publications and more selected citations from other publications and publications on the Palestinians. Text in-

cludes Hebrew bibliography as well as English bibliography.

Yearbook on International Communist Affairs, 1984: Parties and Revolutionary Movements. Edited by Richard F. Staar. (Stanford, Calif.: Hoover Institution Press, 1984. Pp. 590. \$49.95.)

The eighteenth in its series, this volume includes profiles on communist or Marxist-Leninist parties in 104 countries, two regional organizations, and 10 international fronts written by some 71 scholars. Text includes entries under Africa and the Middle East, the Americas, Asia and the Pacific, Eastern Europe and the Soviet Union, and Western Europe. Volume includes a registry of communist parties, select bibliography, and name and subject indexes.

The Visual Display of Quantitative Information. By Edward R. Tufte. (Cheshire, Conn.: Graphics Press, 1983. Pp. 197. \$34.00.)

In this remarkably rich volume, Tufte argues that "graphics are instruments for reasoning about quantitative information." The first part of the book examines the uses and abuses of graphics in the two centuries following William Playfair's work on graphical designs. Part 2 introduces a theory of data graphics through chapters on data-ink and graphical redesign, chartjunk, data-ink maximization and graphical design, multifunctioning graphical elements, data density and small multiples, and aesthetics and technique in data graphical design. Text includes index.

The Use of Public Services by Undocumented Aliens in Texas: A Study of State Costs and Revenues. (Austin: Lyndon B. Johnson School of Public Affairs, University of Texas, 1984. Pp. 280. \$12.50, paper.)

Researchers interviewed state and local public service agencies and 253 undocumented aliens to determine whether taxes paid by these aliens covered the costs of public services provided to them. Report suggests that state revenues from these people exceed the cost of providing services, but that localities pay more for services provided than they receive in revenue from undocumented aliens. Overall, state and local revenues exceed the cost of services provided. Volume includes several appendixes but no index.

Strategic Materials: A World Survey. By Rae Weston. (Totowa, N.J.: Rowman & Allenheld, 1984. Pp. 189. \$34.50.)

This text examines the extraction of, access to, and stockpiling of strategic materials worldwide. Topics include the anatomy of strategic materials, their supply, substitution, and recycling, supply disruptions or restrictions, and markets and pricing. Appendixes cover U.S. stockpiles, operating mines in the Western world, major new projects and expansion programs, a bibliography, and an index.

The Legal Research Manual: A Game Plan for Legal Research and Analysis. By Christopher G. Wren and Jill Robinson Wren. (Madison, Wis.: A-R Editions, 1983. Pp. xiv + 197. \$8.95, paper.)

This guidebook provides a basic procedure for legal research. Chapters include discussions of sources and citations, gathering and analyzing facts, identifying and organizing the legal issues, finding, reading, and updating the law, note-taking, and more. Appendixes include sections on computerized legal research, other research sources, an overview of civil procedure, briefing a case, and other research aids. Volume contains index.

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